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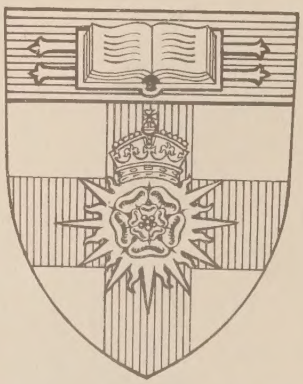

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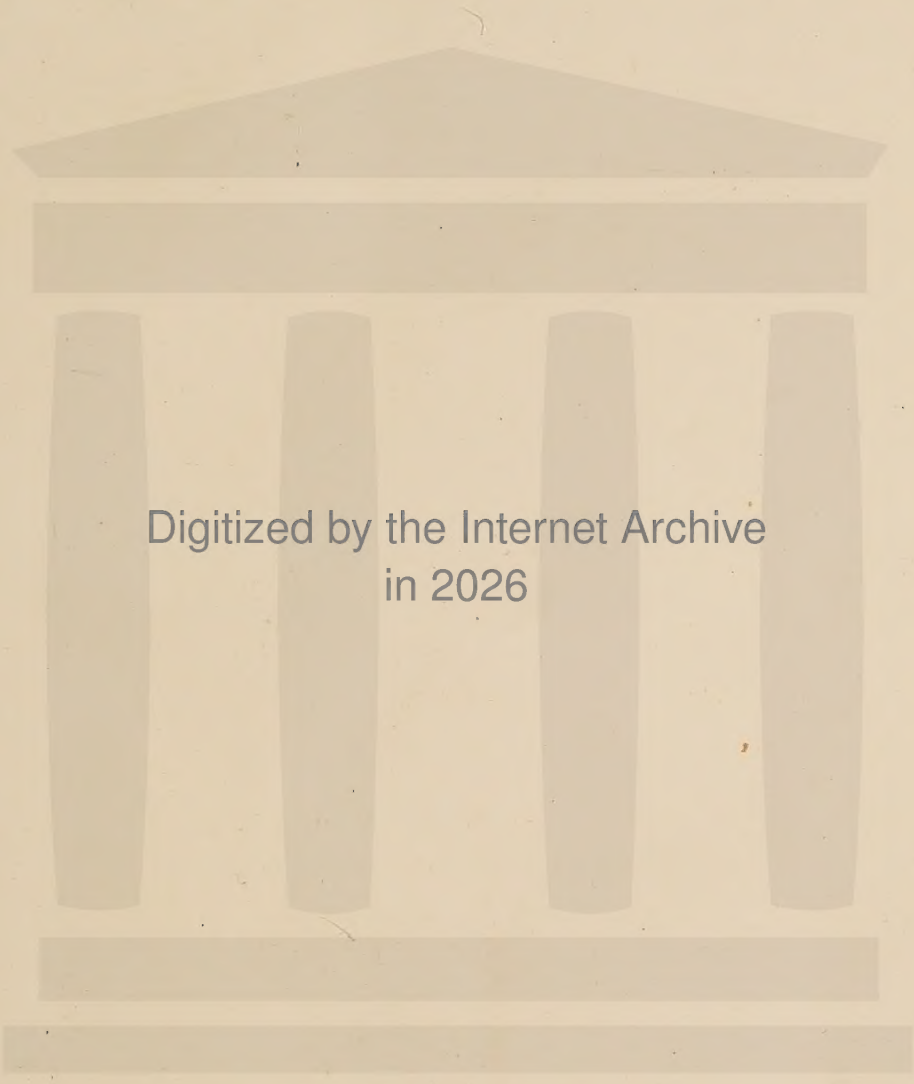

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REPORTS

FROM

COMMISSIONERS, INSPECTORS,

AND OTHERS:

FIFTY-ONE VOLUMES.

— (47.) —

TUBERCULOSIS (ROYAL COMMISSION);
VIVISECTION (ROYAL COMMISSION).

Session

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N.B.—*THE* Figures *at the* beginning of the line, *correspond with the* N° *at the foot of each* Report; *and the* Figures *at the* end of the line, *refer to the* Paging *of the Volumes arranged for The House of Commons.*

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SECOND INTERIM REPORT

OF THE

ROYAL COMMISSION

APPOINTED TO INQUIRE INTO THE RELATIONS OF

HUMAN AND ANIMAL TUBERCULOSIS

PART II. APPENDIX.

VOLUME III.

ADDITIONAL INVESTIGATIONS OF BOVINE & HUMAN VIRUSES

BY

DR. L. COBBETT, DR. A. STANLEY GRIFFITH, & DR. F. GRIFFITH.

Presented to both Houses of Parliament by Command of His Majesty.

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PREFACE.

The details and results of the experiments made to determine the relative virulence and pathogenic effects of tuberculous viruses of Bovine and Human origin on various species of animals have been published in the first and second volumes of the Appendix to the Second Interim Report of the Commission.

Whilst those experiments were in progress others, undertaken to throw light on collateral questions, were undertaken at Stansted, and the results of these additional investigations are collected together in this volume.

The results of ten separate investigations are included. Each of these was designed by, and carried out under the direct supervision of, the Commission, but the actual reports have been written by the investigators whose names are attached to them. In their own report (Part I.) the Commission expressed the opinions they themselves had founded on the results of the whole of the experimental work carried out at Stansted, and these general opinions were in every instance in complete accord with those of the resident investigators. In the individual reports contained in this volume it has been considered right to allow each of the investigators to set forth not only the definite facts arising out of the experimental work, but also his own views based on the work done by himself. (Report, Part I., page 42.)

E. J. S.

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THE CULTURAL CHARACTERS OF THE BOVINE TUBERCLE BACILLUS.

Dr. A. STANLEY GRIFFITH and Dr. F. GRIFFITH.

THE CULTURAL CHARACTERS OF THE BOVINE TUBERCLE BACILLUS.

This report gives the results of the investigation as to the cultural characters of the bovine tubercle bacillus.

The investigation has been carried out with two chief objects in view:

1. To find the most favourable media for the growth of the tubercle bacillus.

2. To contrast the cultural characters of different strains of bovine tubercle bacilli.

The cultures investigated were derived from bovine animals naturally infected with tuberculosis, and were isolated, in the majority of cases, direct from the original material.

300 strains of tubercle bacilli have been isolated, representing 30 viruses.*

Failures to get the bacillus to grow have been very few and occurred in the early part only, in the cases of a few guinea-pigs where either unsuitable media were used or an insufficient number of tubes sown.

The following is a list of the media used:—

Serum media. Serum has been obtained from 12 species of animals, namely, the ox (cow and calf), horse, pig, goat, sheep, baboon, monkey, lemur, dog, cat, rabbit, and guinea-pig.

Glycerin serum.

Serum agar, with and without glycerin.

Egg: the yolk and white have been used separately and combined.

Glycerin agar.

Glycerinated potato.

Glycerin broth.

ditto. with 10 per cent. gelatine.

Asparagin liquid medium.

Mode of Preparation of Media.

Serum media: The serum media are prepared in the following way: Blood is collected in sterile flasks from animals, either by inserting a cannula in the external jugular vein during life, or in the common carotid artery after shooting, or anæsthetising; the blood of rabbits and guinea-pigs is usually collected by allowing the blood to flow direct into a large sterile test-tube pressed against the side of the neck after exposing and severing the carotid and jugular vessels.

The blood is allowed to stand from one to three or four days, according to the speed with which the serum separates; the serum is then decanted, either by means of a pipette or a syphon, and distributed into tubes, about 6 cc. being used for each. The tubes are put in an inclined position in a serum inspissator, and kept for about two hours at a temperature that varies in different cases between 75°C. and 80°C. In the inspissator is placed a bowl containing water. Each tube is then sealed with paraffin and stored.

By this method the different kinds of sera, at the time of inoculation, necessarily varied in age and moistness.

To make the media more strictly comparable, the serum after distribution into test tubes was at one period fractionally sterilised (*i.e.* kept at 60°C. for an hour on five consecutive days) and stored in a liquid state in an ice chest, each tube having a rubber cap placed over its aperture to prevent evaporation. When required for use a series of tubes were coagulated as above. In this way it was thought that variations in the physical characters of the serum, produced by slight differences in the temperature and duration of the coagulation, and in the amount of moisture, would be eliminated.

The method was abandoned because it did not appear to possess enough advantage over the old way of preparing the serum to make it worth the extra trouble; there was the distinct disadvantage that the tubes were never ready for immediate sowing and, probably on account of the concentration of the serum, a greater tendency to the formation of a film of salts on the surface was exhibited.

Glycerin serum: 5% sterile glycerin is added to a measured quantity of serum before distribution into tubes; the procedure is then the same as for serum.

Serum agar is prepared by adding 0·5 cc. serum to 5 cc. agar liquefied and cooled down to about 50°C.; the two are well mixed and sloped.

Egg mixture: fresh eggs are broken into a sterile beaker and the white and yolk beaten up with a sterile glass rod; sterile salt solution is then added in the proportion of 1 to 3 and the mixture is distributed into tubes; the tubes are sloped in the serum inspissator and kept for 2 hours at 75°C. on two consecutive days.

The yolk and white, when used separately, are coagulated in the same way as the egg mixture; no saline is added.

Yolk agar is made in the same way as serum agar.

Glycerinated potato: the potato wedges are first soaked in a 1 in 1000 sodium carbonate solution for 24 hours and then in a 5 % glycerin solution deeply tinted with litmus until the potato acquires a definite blue colour. The potato is afterwards put into test tubes containing at the bottom a little cotton wool soaked in 5 % glycerin solution and sterilized in the usual way. Growth is readily observed on a potato tinted in this manner.

Glycerin agar, and glycerin broth are prepared after the usual formulae.

The asparagin medium was prepared by dissolving asparagin (5 grms.), KH_2PO_4 (5 grms.), sodium chloride (5 grms.), magnesium citrate (2·5 grms.), potassium sulphates (2·5 grms.), in distilled water to 1000 cc. and filtering the solution after reducing the acidity to 20 per litre.

Source of Cultures.

In the case of the viruses the direct culture has usually been obtained from the thoracic glands and occasionally from the lung, mesenteric glands or perlsucht growths on pleura or diaphragm.

In experimental bovines the material for inoculating the tubes has been taken from the prescapular, mesenteric thoracic or supramammary lymphatic glands, the prescapular gland being most often used.

In pigs the direct cultures have been raised from the submaxillary and bronchial glands.

In the guinea-pig the sternal gland is the most convenient gland from which to make cultures, but when the sternal gland is not much enlarged, the liver or the spleen has been chosen on account of the softness of the tissues of these organs.

The lesions in a monkey, rabbit or mouse are well adapted for the production of cultures; they are usually not only rich in bacilli, particularly in the latter animal, but also, being softened, easy to emulsify and spread evenly over the surface of the medium.

Method of Raising Primary Cultures.

Whatever organ or gland is selected it is first freed from all surrounding connective tissue, dried and then well seared with a Paquelin cautery. An incision is made with a sterile knife in the centre of the seared area, and a small piece of tissue is removed.

In the early part of the work a bit of the tissue was deposited on the surface of each medium, and was then picked up with a stout platinum wire and carried to the condensation water at the foot of the slope; here it was squeezed and pressed against the sides of the tube until the water became definitely milky; the emulsion thus found was allowed, by slightly inverting the tube, to flow over the whole surface and the broken up pieces of tissue were well rubbed over every part of the medium.

When caseous and softened lesions were being dealt with an even distribution of the material was readily secured, but when the tissue was composed of the firm, dense, caseo-necrotic material met with in acute tuberculosis in bovines a good distribution was not so readily effected.

In such cases various modifications were tried; at first pieces of the gland were broken up in a sterile mortar and emulsified with normal saline, a loopful of the emulsion being smeared over the surface of each

* Virus is the term applied to the original material; a culture derived from that virus, either directly or through an experimental animal, is called a "strain."

medium. Then, instead of in a mortar, the piece of tissue was broken up and emulsified in a short stout test tube by means of a glass rod.

The last method, the one now exclusively used, has several advantages; a small piece of tissue which in the first method could be used for one tube only, can by this means be used to inoculate a large series and there are fewer risks of air-borne contaminations than when emulsified in a mortar; there is also the advantage that the emulsion so prepared can be examined microscopically before sowing the tubes, and if too rich in bacilli, *e.g.* for the production of discrete colonies, can be diluted to any required extent, or if too poor a larger loop can be used for sowing the tubes.

After sowing, the tubes are resealed with paraffin and placed in the incubator in a vertical position. At one time, to prevent undue evaporation the tubes were placed in a position inclined so as to allow the water of condensation to flow over the greater part of the surface of the medium.

The practice was discontinued for primary cultures, because it was found that this position enabled contaminating organisms which, in the vertical position might have remained localised to one spot, to overrun the whole surface, thus rendering it impossible to secure a pure culture.

Estimation of the Value of Various Media for Primary Cultures.

Many observers have experienced difficulty in raising cultures of the bovine tubercle bacillus on ordinary media. This investigation has been carried out to find what media are most suitable for the growth of the bacillus in primary culture and incidentally to ascertain whether the serum of different species of animals has any specific value. For this purpose whenever a culture was desired a series composed of as large a variety of media as was available at the time was sown. The results are given in tabular form.

Medium.	Number of tubes sown.	Number of Positive Results.	Percentage.	Medium.	Number of tubes sown.	Number of Positive Results.	Percentage.
Cat Serum - -	70	56	80	Cat Serum Agar - -	8	5	62
Goat „ - -	83	53	64	Goat „ „ - -	26	23	80
Baboon „ - -	36	22	61	Baboon „ „ - -	21	9	43
Rabbit „ - -	45	27	60	Rabbit „ „ - -	41	15	37
Guinea-pig Serum -	48	29	60	Guinea-pig Serum Agar	2	1	50
Pig „ -	191	116	60	Pig „ „	20	3	15
Dog - „ -	198	112	57	Dog „ „	74	59	80
Sheep „ -	53	30	57				
Rhesus „ -	89	44	50				
Adult Bovine „ -	229	90	43	Adult Bovine „ „	59	46	78
Calf „ -	81	24	30	Calf „ „	23	8	35
Horse „ -	43	7	16	Horse „ „	16	0	0
				Rat „ „	10	0	0
				Lemur „ „	2	2	100
Egg mixture - -	498	459	92	Glycerin Agar - -	88	5	6
Egg yolk - - -	19	17	90	Glycerinated potato -	144	25	17
Egg white - -	16	8	50	Glycerinated Serum -	148	19	13
Egg yolk agar - -	50	30	60	Glycerin Agar + 5 % dog Serum - -	38	15	42
				Glycerin Agar + 5 % pig Serum - -	16	9	56
				Glycerin Agar + 10% Bovine Serum - -	12	10	83

The egg medium has been the most successful and can always be relied upon for the production of primary cultures. It has the additional advantages of being readily obtained and easily prepared. The yolk and the egg mixture are of about equal value and far superior to the white alone.

On the first three media the colonies are visible in from eight to twelve days and appear as transparent points. If very numerous they remain small and transparent, but when there are only a few they may grow to a considerable size becoming raised and creamy white, and either moist, or dry and warty.

The sera of different species of animals have been found, as Professor MacFadyean predicted, to vary less than the sera of animals of the same species. The sera of a large number of animals of each species have been used and at least one serum of each species has given a high percentage of successes. The serum from the cat has been found the most uniformly good, while that of the horse up to the present has been generally poor. Several poor sera in the case of the dog have considerably lowered the high percentage of successes obtained at first. From the calf only occasionally has a good serum been obtained; the adult ox, on the other hand, has yielded a serum which has been invariably good, especially when of a golden yellow colour. In several instances serum containing a large number of blood corpuscles has given better results than the same serum without corpuscles.

The failure to produce growth in the case of some sera appears often to be due to the formation of a film over the surface. It was chiefly to avoid this disadvantage that serum agar was used.

Serum agar is a satisfactory and economical substitute for pure serum, and has the advantage of being perfectly transparent; it has been found to be very little inferior to the egg medium for raising primary cultures.

On several occasions a serum which has been found

poor when coagulated, has given excellent results as serum agar.

Glycerin serum and potato are not very useful for primary cultures and when growth occurs the bacillus is usually one which grows well on glycerinated media in subculture. On potato in one instance a most luxuriant primary growth was obtained, much superior to any subculture on this medium.

On glycerin agar in only a few instances has a primary culture been obtained.

Subcultures.

The tubercle bacillus having once started to grow on artificial media grows readily when transplanted on almost every variety of serum media although early generations grow less abundantly than later. Growth takes place from the outset and usually attains its maximum at the end of three weeks' incubation. On the same variety of serum all the viruses produce the same type of growth. On bovine serum the growth is in the form of a dry, usually uniform grey coherent layer; on dog serum it is moist and more easily emulsified and exhibits no tendency to spread over the condensation water; on pig serum the growth is scaly and more abundant than on bovine serum.

On different varieties of serum the amount of growth varies, being most abundant on those which give the best results in primary culture. The egg medium is unsatisfactory for subcultures.

The bovine tubercle bacillus will not grow at a temperature of 42°C. or when placed under anaerobic conditions.

The bacillus retains its vitality in culture for a considerable time and by means of the egg medium a growth was obtained from a subculture 597 days old (the virulence of this culture for rabbits remained unaltered.)

Comparison of the Cultural Characters of the Different Viruses.

In these comparisons the importance of employing the same technique throughout was recognised, and the following method adopted.

A small amount of culture, approximately equal in all cases, was taken and smeared evenly over the surface of each medium; the even distribution was effected by emulsifying the material on the surface of the medium with a drop of the condensation water. By the use of a small amount of culture, the differences between the various strains are brought out more clearly than when a large amount is used. The most uniform results are obtained when the inoculated material is taken from a vigorous subculture.

The media used for the comparisons of the growth characters have been serum and glycerin serum, glycerin agar, glycerinated potato, and glycerin broth.

On glycerin serum the viruses of bovine origin exhibit three types of growth.

In the first, no growth can be observed until the second or third week, when discrete colonies make their appearance. In some cases the colonies remain small and discrete, in others they continue to grow, and the amount of growth at the end of six weeks may equal or exceed that on the corresponding pure serum. In this first series, the distinguishing features are the initial delay on glycerin serum compared with the steady uniform increase on pure serum, and the absence of any considerable variation between the strains which exhibit this type of growth.

In the second, growth takes place from the outset and up to the end of the second week is similar in appearance to that on serum alone, and about equal in amount. The amount of growth finally produced at the end of the third or fourth week varies in different strains; with Viruses II., VII., XXIX., and XXX. growth is about equal to that on pure serum, with Virus XVI. rather less, and with Viruses VIII., XIV., XX., XXII., XXIII., and XXVII. rather more than on the corresponding serum.

In the third, growth during the first week is more abundant than on pure serum, and progressively increases, becoming, at the end of the fourth week, from two to four times as much as on serum. The characters of the growth also differ markedly on the two media; that on serum is grey and uniform with, in many cases, scattered raised colonies on the surface, while that on glycerin serum is whiter and moister with a more irregular and often wrinkled surface.

The bovine viruses which exhibit the first type of growth are Nos. I., III., IV., VI., XII., XIII., XV. XVII., XVIII., XIX., XXI., XXIV., and XXVI.

Those which exhibit the second type are II., VII., VIII., XIV., XVI., XX., XXII., XXIII., XXVII., XXIX., and XXX.

Those which exhibit the third type are V., IX., X., XI., XXV., and XXVIII., the latter growing rather less well than the others.

On pure serum the characters of the growth are the same for all viruses, and no differentiation on this medium can be made after they have been in cultivation some time. A uniform grey layer, often with raised white colonies on the surface, is formed at the end of the first week; this increases until the third week, when in the majority of cases growth ceases.

Viruses of Class I. do, however, grow in early generations less readily on serum than those of Class III., thus showing that the differences between the various viruses brought out by the use of glycerinated media are differences in saprophytic power and are in freshly isolated cultures capable of demonstration on pure serum.

On glycerin agar the results are in accordance with those on glycerin serum, and the same differentiation of the viruses can be made.

The first series of viruses grows with difficulty; in some cases no growth whatever occurs, in others discrete colonies make their appearance after several weeks, sometimes in the fourth, and often much later. As in the case of glycerin serum, there is the initial delay before the appearance of colonies, which when numerous may coalesce to form a thin continuous layer, and when fewer and discrete may attain a considerable size, becoming heaped up, warty, and slightly pigmented.

In the second series growth takes place from the outset, a thin grey layer being formed at the end of the first week, which increases slightly up to the end of the third week. Subsequently, in some cases, raised colonies develop on the surface.

In the third, growth is more abundant, and attains a moderate thickness at the end of the third week, showing in some cases a fine wrinkling on the under-surface but no pigmentation. In the case of Virus V., however, a definitely wrinkled and slightly pigmented layer was obtained.

On glycerinated potato growth is very variable, but in the main corresponds with that on glycerin agar. The viruses in the first and second series form after many weeks discrete colonies, which grow slowly becoming pearly-white and up to a pinhead in size. Occasionally larger, irregular, creamy nodules are obtained after several months. Those in the third series grow more readily, and produce in from two to three weeks a grey layer, varying in thickness, in which raised warty yellowish colonies may subsequently develop. The growth of viruses in all the series may be attended with the formation of pigment to some extent, varying from creamy white to a definite yellow.

On glycerin broth great difficulty was experienced in getting many of the viruses to grow when inoculated direct from serum. This has been especially the case with viruses in the first series, the inoculated material either not increasing or forming warty nodules, which might, after many weeks, send out over the surface a very thin grey membrane.

On the asparagin medium, growth was as a rule more readily obtained than on broth, but the pellicle formed was often incomplete and in all cases thin and uniform. As all the viruses produced more or less the same type of pellicle, little differentiation was exhibited on this medium.

Various devices have been adopted to enable the inoculated material to form a pellicle on the surface. In one the broth in six-ounce medicine bottles, contained a pledget of cotton wool which just projected above the surface of the fluid, and on to which the culture was inoculated. Growth took place on the wool in the form of white or creamy irregular nodules from which after a long time a delicate grey pellicle developed.

In another, 10 per cent. gelatine was added to the broth forming at 37°C. a liquid medium. This medium gives uniform results since, being solid at room temperature it can be inoculated from young serum cultures of equal ages; it has the additional advantage, that by allowing it to solidify and adding formalin the pellicles can be preserved as permanent specimens. The characters of the growth do not differ from those of glycerin broth cultures.

The method devised by Dr. Eastwood of combining solidified serum and broth has also been attended by

constant results in producing surface growths characteristic of the different viruses.

Those viruses which grow with most difficulty on glycerinated media show a corresponding reluctance to grow on the surface of glycerin broth; the type of pellicle formed is delicate grey and almost transparent, and the growth is occasionally broken up into islands which incompletely cover the surface.

Viruses which grow more readily on glycerinated media generally grow more rapidly on broth, and produce a thicker translucent pellicle with opaque patches. In all cases, after the pellicle has begun to form, the surface of the broth is quickly covered, and there is little subsequent thickening, the growth often showing a tendency to break up, become moist and sink.

A consideration of the results on the differential media shows that bovine bacilli can be arranged in a more or less complete series beginning with a bacillus which grows badly on all the differential media and ending with one which grows relatively well. On this point we are in entire agreement with Dr. Eastwood in his classification. We have not, however, attempted to arrange the strains in a consecutive series; our experience with various culture media makes us hesitate to rely too much upon small points of difference.

Nevertheless, there is a marked contrast between the extreme types of growth on all media containing glycerin. For example, a comparison of the weight of growth produced on serum and glycerin serum, respectively, has been made with strains representing each series.

The method adopted was to sow in each case a number of tubes of serum and glycerin serum and incubate for three weeks. An equal number of tubes on which the best growths had occurred was then selected and the growth scraped off and weighed. In the case of Virus 20 the growth after being weighed directly was emulsified with saline and estimated by the volumetric method devised for culture inoculations. The weight as estimated by this method was found to agree closely with that obtained on the balance.

TABLE OF COMPARISONS OF WEIGHTS.

Virus.	Number of tubes on each medium.	Weight on Serum.	Weight on Glycerin Serum.	Ratio.
B. IV.	4	53	5	10 : 1
B. XX.	4	100	120	1 : 1·2
B. V.	6	90	300	1 : 3·3
B. IX.	4	35	142	1 : 4

In the above comparisons the same serum of excellent quality was employed. When only a moderate serum is used the differences are more marked, as in the following cases which were incubated for 4 weeks.

B. X. G.P. 1072	1	4	47	1 : 11
B. X. G.P. 1070	1	14	45	1 : 3
B. XIV.	1	12	40	1 : 3·3

The following belong to the most difficult growing viruses, and would yield a result similar to Virus 4. The comparison shows their increased luxuriance on glycerin serum after growing for a few generations on that medium.

B. XVIII.	1	17	27	1 : 1·6
B. XII.	1	5	10	1 : 2

A. STANLEY GRIFFITH.
F. GRIFFITH.

DESCRIPTION OF THE CHARACTERS OF THE VIRUSES ON THE DIFFERENTIAL MEDIA.

Bovine Viruses.

Virus B. I.

Mediastinal Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Cow 44	17th	Moderately thick uniform grey layer.	No growth occurred until the end of the 2nd week, when a few minute colonies appeared. At the end of the 3rd week the colonies were more numerous but discrete. At the end of the 4th week the colonies had coalesced and covered the whole surface with a grey layer.	The final pellicle was thin uniform grey and semi-transparent with fine whitish lines. After several broth subcultures a rather thicker translucent pellicle with fine wrinkling was formed.	No growth was visible during the first 3 weeks. At the end of the 4th week there were very numerous minute discrete grey colonies scattered over the surface. These increased in size, forming thin grey filmy colonies, some with opaque central foci; these subsequently coalesced in patches.	At the end of 3 weeks minute grey points were seen with a hand lens. In 10 weeks there were moderately numerous small raised colonies of various sizes.

Other strains of this virus—G.P. 14, Cow 40, Calf 106, Baboon 66, Pig 16, Pig 96, and Pig 98—which were tested on the differential media, closely resembled Cow 44.

In the culture obtained from Baboon 8, fed with the milk of Cow 64 (Virus I.), a decided increase of luxuriance has resulted. This increase was maintained after passage through a second baboon (No. 64).

Virus B. II.

Lung.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Heifer 66.	13th	Uniform grey layer with a few raised white colonies.	Growth at the end of 4 weeks was equal to that on pure serum. There was no difference in character with the exception that the small white colonies on the surface were more raised and numerous.	The culture from Heifer 66 was not grown on broth, but in four other strains of this virus the type of pellicle was identical. That from Heifer 100 is described. Growth was slow in beginning: in 3 weeks there was one delicate grey island. In 5 weeks the surface was covered with a delicate grey transparent pellicle composed of small islands speckled with opaque greyish-white foci. In 8 weeks all the growth had sunk.	At the end of the second week there was a thin uniform grey layer with numerous small raised colonies superimposed. A slight increase was noted at the end of the 4th week after which there was no further change.	Very minute discrete greyish-white colonies were first visible at the end of the 4th week. These increased to a very slight extent subsequently.

Cultures raised from six other strains of this virus: Bull 22, Heifer 100, Calf 134, Calf 178, Guinea-pigs 537, 172, 1322 and 1735 agreed very closely in cultural characters with that isolated from Heifer 66.

Guinea-pig 1735 was the 24th of a series of guinea-pigs the first of which was inoculated with the original material of Virus II. The virus was kept going in guinea-pigs to see if any modification by prolonged residence in these animals could be produced. The result showed not the least change in cultural characters.

Virus B. III.

Mediastinal Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct.	15th.	Uniform grey layer of moderate thickness.	There was no growth during the first 2 weeks. At the end of the 3rd week the surface was covered with minute grey colonies. These increased a little in size but remained discrete.	No growth has been obtained on broth. On glycerin gelatine the surface became covered with transparent filmy islands. On an asparagin medium (2 per cent. glycerin) two-thirds of the surface was covered with a delitate grey pellicle in 7 weeks.	At the end of 5 weeks there was no growth. In 10 weeks there were two flat grey colonies with opaque centres.	In 4 weeks there were two minute grey colonies. These increased to four in the 5th week. In 10 weeks the four colonies were rather less than a pin-head in size, creamy in colour and surrounded by a group of very minute grey colonies.

Two other strains of this virus, Monkey 64 and Calf 214, have been investigated, and gave similar results, except that more numerous colonies appeared on glycerin agar in the case of Monkey 64.

Virus B. IV.

Mesenteric Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct.	29th.	Uniform layer of moderate thickness in 3 weeks, with numerous raised white colonies.	In 14 days there was no growth. At the end of 3 weeks the surface was covered with numerous minute grey and greyish white colonies. In the fourth week these coalesced and formed a complete layer, covered with raised white colonies. This increased to a moderate thickness in the sixth week and appeared almost equal in amount to that on serum alone.	On serum and broth, no pellicle was formed at the end of 5 weeks; there were several small yellowish nodules floating on the surface. After subculture for several generations on gelatine broth, a growth was obtained on transference to broth. The type of pellicle produced was thin grey and translucent, with delicate wavy whitish lines.	No growth was obtained with this generation of culture. Several other attempts on different agar media with earlier generations also failed to produce growths.	At the end of 3 weeks minute grey points were seen, which in 3 months showed no change. An earlier generation (17th) showed half-a-dozen opaque white granules at the end of 8 weeks. At the end of 3 months there were 26 colonies of all sizes up to that of a pin-head, slightly creamy in colour.

Cultures of this virus were isolated after passage through different species of animals, namely, Chimpanzees, 4. 6. 8. 10. 16. Baboons, 22. 30. 50. 62. Dogs, 26. 54. 56. and 74. Cat, 12. Rabbit, 5. Calf, 142. Cow, 172. The results on the differential media with these strains closely resembled the above: the only difference was that some of the strains produced discrete colonies on glycerin agar after several weeks. The culture from Dog 18 was quite different from the original virus.

Dog 18.	5th. Broth 6th and 7th.	Thin uniform layer at the end of 3 weeks.	At the end of the first week there was a uniform layer thicker than on serum alone. In the second week it was twice as thick and at the end of the fourth week there was a very thick opaque white finely wrinkled growth.	A bottle of broth sown with the pellicle from the condensation water of a serum tube, showed a single opaque white island in 4 weeks. This was subcultured on broth and in 4 weeks there was a thick opaque white, slightly wrinkled pellicle, covering about a quarter of the surface; growth then ceased.	At the end of the second week, there was a uniform grey layer slightly wrinkled. In the fifth week this had become thick and opaque with well-marked wrinkling of the surface.	In 2 weeks, there was a greyish growth composed of very minute colonies closely aggregated. These increased, becoming slightly yellow and at the end of the seventh week there was a raised opaque granular layer of a yellow colour.

Virus B. V.

Mesenteric Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Heifer 80	12th Broth 19th	Uniform layer of moderate thickness at the end of 3 weeks.	At the end of the 1st week there was a moderately thick grey layer rather better than on serum alone. During the 2nd week the growth became thicker and covered with small white elevated colonies. Growth continued during the 3rd week, the colonies increasing in size. At the end of the 6th week there was a thick greyish white layer, better than on serum, with a moist roughened surface and a slightly wrinkled margin.	In 14 days the surface was covered with a thin uniform translucent grey pellicle already with a tendency to sink.	There was a uniform grey layer at the end of the 1st week. Steady increase took place until the end of the 5th week, when the surface was covered with a grey ground glass layer with fine wrinkling round the edges. At the end of 8 weeks there was very little further increase.	Growth was visible in 11 days as numerous minute grey colonies. In the 5th week there was a layer composed of minute grey colonies. At the end of the 7th week a slight increase was indicated by a few of the colonies becoming a little larger than the rest, being white and slightly raised. No further progress was noticed.

Cultures were also isolated from G.P.'s 711 and 812 inoculated with the original material and were similar to Heifer 80. On 5 per cent. glycerin broth with G.P. 812, the surface was covered in 14 days with a grey translucent pellicle, dotted over with opaque white foci and several warty nodules.

Virus B. VI.

Mediastinal Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	14th.	Uniform layer of moderate thickness in 3 weeks.	No growth was evident until the 3rd week, when the surface was covered with minute grey colonies coalescing in parts. At the 6th week the colonies had fused together, forming an almost continuous thin grey layer.	In 7 weeks there were numerous small grey islands over the surface and a circular patch one and a half inches in diameter rather thicker and speckled with white foci. The growth subsequently sank.	No change was observed until the 6th week, when two minute white colonies were seen. There was no increase at the end of 12 weeks. In the 18th generation no growth could be seen at the end of 4 weeks.	One minute greyish white colony was seen at the end of the 5th week. A few more appeared in the 6th week. 7th week no change.
G.P.1015 (Inoculated with original material)	7th.	Uniform layer.	Uniform layer in 3 weeks identical with that on pure serum.	No growth was obtained.	At the end of the 2nd week there were small sparsely scattered discrete granular colonies. Over part of the surface a thin grey growth developed in between the colonies. At the end of the 11th week there was this patchy grey layer not increased in thickness and numerous small colonies with grey filmy margins.	In the 2nd week there were a few whitish colonies, increasing slightly up to the 5th week. At the end of the 12th week the surface was almost covered with minute pearly-white colonies.

A culture isolated from Calf 216 resembled the direct culture.

Cultures were also obtained from G.P.'s. 1014 and 1015 inoculated with original material; both of these grew distinctly better than the direct culture and were identical with each other. A culture obtained from Calf 240 inoculated with culture of G.P. 1015 had the same characters as the original G.P. cultures.

Virus B. VII.
Mesenteric Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	12th	Uniform grey layer.	Uniform layer in 3 weeks identical in character and amount with that on serum.	In 3 weeks the surface was covered with a very thin grey pellicle which showed no further change. The growth on 2 per cent. glycerin gelatine was identical in appearance excepting that the pellicle was dotted with opaque white foci.	At the end of the 1st week there was a slight initial grey growth, but no further progress until the 3rd week. At the end of the 4th week there was a grey layer not quite uniform. In 5 weeks it was rather thicker and greyish white in parts, due to small confluent colonies. At the end of 2 months delicate grey colonies had appeared in the thinner patches.	Minute grey colonies were visible in the 3rd week. There had been no change 11 weeks later.

Cultures were also isolated from G. P.'s 1016 and 1017, both inoculated with original material. The cultures were identical with Virus VII. direct, but a rather better growth was obtained with G.P. 1017 on potato.

Virus B. VIII.
Mesenteric Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	15th	A moderately thick layer in 3 weeks.	There was a uniform growth a little less than on serum in the 1st week. In the 3rd week the growth became whiter and raised in parts, but on the whole less than on serum. In 6 weeks numerous small raised colonies appeared on the surface.	Inoculated from a serum tube on to a flask of 2 per cent. glycerin broth. In 14 days there was thin grey translucent pellicle speckled with whitish foci, covering about $\frac{1}{3}$ of the surface. In 4 weeks the surface was covered with a greyish translucent membrane studded with raised opaque white colonies.	In 7 days there was a thin uniform grey ground-glass layer, which increased in thickness slightly until the 3rd week. In the 4th, 5th and 6th weeks there was no change. At the end of 10 weeks the surface was studded with minute whitish colonies.	In 14 days there was a greyish layer composed of minute colonies. In the 4th week it was a little thicker and greyish-white, with no subsequent increase.

A culture isolated from Dog 102, fed with culture of original material, showed no alteration.

Virus B. IX.
Mediastinal Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct.	14th.	Thin uniform grey layer not increasing after the 2nd week.	At the end of 7 days there was a grey layer with white points decidedly better than on serum alone. During the 2nd and 3rd weeks the growth became thicker and the colonies whiter and more raised. At the end of the 4th week there was a greyish-white moist layer, at least twice as thick as on serum alone.	On an asparagin medium the surface was covered with a thin grey membrane with a few whitish points. On 5 per cent. glycerin broth in 14 days the surface was partly covered with fairly thick opaque white islands, which sank a few days later.	There was a slight increase at the end of the 1st week. In the 2nd week there was a uniform grey ground-glass layer. Increase took place until the end of the 6th week, when there was a moderately thick grey granular layer, thicker and whiter in places.	At the end of the 4th week there was a thin greyish white layer on the surface of the potato. This increased very little subsequently.

Strains of this virus have been isolated from 15 different animals, including the chimpanzee, monkey, baboon, dog, cat, pig, goat, mongoose, hedgehog, rabbit and guinea-pig; with one exception, that of Dog 50, these have reproduced the characters of the original virus, the pellicle formed on the surface of broth, though varying in thickness, exhibiting the same marked tendency to sink.

VIRUS B. IX.—*continued.*

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Dog 50 through G.P.1708	3rd	Thick uniform greyish-white layer.	In the 1st week the growth was thicker than on serum. At the end of the 2nd week the layer was thick, creamy and markedly wrinkled. Subsequently the wrinkling became coarser and the colour more yellow.	At the end of 6 weeks there was a thick opaque pellicle, creamy in colour, with a finely granular surface. No tendency to sink.	Growth took place from the outset and in 6 weeks the surface was covered by a thick opaque, yellow, coarsely wrinkled layer.	At the end of the 1st week there was a greyish-white layer. This increased until at the end of 6 weeks there was a thick raised yellow growth with a granular surface.

A culture was also isolated from Dog 50 through G.P. 1707; its cultural characters were identical with those of G.P. 1708.

VIRUS B. X.

MESENTERIC GLAND.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
G.P.1070 (Inoculated with original material)	20th Broth 16th	Moderately thick grey layer with a few small whitish colonies.	At the end of the 1st week there was a layer rather thicker than on serum alone. It increased in thickness until the end of the 4th week when there was a greyish white finely wrinkled layer about twice as thick as on serum without glycerin.	In 14 days the surface was incompletely covered by several uniform grey translucent islands. There was slight thickening subsequently, the pellicle remaining translucent. In 4 weeks the growth had sunk.	In 7 days there was a grey translucent layer covered with small white colonies. The growth increased in thickness until the 4th week, becoming greyish white and opaque in patches with fine wrinkling on the under surface.	In 6 weeks numerous minute white colonies were observed. There was considerable increase until the 10th week when the colonies in part coalesced to form a patch of grey growth, and others became raised and slightly yellow.

A culture was isolated from the supramammary gland through G.P. 1072 and was identical in cultural characters.

VIRUS B. XI.

MEDIASTINAL GLAND.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Rabbit 66 (Inoculated with original material)	11th	In 3 weeks there was a thick uniform grey layer.	At the end of the 1st week there was a greyish white layer with small colonies, thicker than on serum alone. During the 2nd and 3rd weeks the growth became thicker and more raised and in 4 weeks there was a layer covered with greyish white colonies at least twice as thick as on serum alone.	In 14 days the surface was nearly covered by several grey translucent islands. In 28 days the surface was completely covered with a greyish translucent pellicle covered with white raised foci. There was very little further thickening but the growth showed no tendency to sink.	In 7 days there was a grey translucent layer with whitish opaque foci. This increased until the 3rd week when there was a greyish white ground-glass layer with small white colonies.	A thin whitish layer was formed in 3 weeks not increasing subsequently.

A culture was also isolated from Rabbit 65 and yielded similar results.

Virus B. XII.

Lung and Mediastinal Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct (Lung)	16th	Uniform layer of moderate thickness with raised white colonies on the surface.	No growth occurred until the end of the 3rd week when minute colonies could be seen with a lens. During the 4th week numerous small grey colonies became visible. At the end of the 6th week there were numerous flat grey colonies mainly discrete and several raised warty colonies of various sizes.	At the end of 6 weeks there were numerous small grey transparent islands which showed no subsequent change.	No growth had occurred at the end of the 9th week.	In 5 weeks there were a few minute whitish raised colonies. In 10 weeks there were fairly numerous raised whitish colonies less than a pin-head in size with grey filmy margins.
Rabbit 98 (Inoculated with the Mediastinal Gland)	17th	Uniform grey layer of moderate thickness.	Growth took place from the outset in the form of a uniform grey layer which progressed at the same rate as on serum alone. At the end of 4 weeks the growths were identical in character and amount.	On an asparagin medium the surface was covered in 5 weeks by a thin grey pellicle speckled with whitish foci.	A very thin grey layer was formed during the 1st week. Slow but definite progress was made and at the end of the 4th week there was a thin translucent layer with a finely granular surface.	Very numerous minute grey colonies were visible in the 4th week.

A culture was also isolated from G.P. 1159 inoculated with original material; this culture yielded similar results to that isolated direct, with the exception that on glycerin agar a few grey colonies appeared during the 9th week.

Virus B. XIII.

Mediastinal Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Rabbit 116 (Inoculated with the original material)	11th	Thin uniform layer.	There was no growth during the first 2 weeks. At the end of the 3rd week there were fairly numerous discrete whitish colonies. At the end of the 4th week the colonies had increased in size, the largest being about the size of a pin head. Around each colony there was a delicate grey marginal expansion which increased until the 6th week, the colonies however remaining discrete.	At the end of 6 weeks there were several delicate grey speckled islands. The surface eventually became covered with a thin semi-transparent pellicle, partly uniform and partly reticulated.	At the end of the 4th week very numerous minute delicate grey colonies could be seen with a hand-lens. In 6 weeks these had increased in size with raised opaque centres and had coalesced in parts.	At the end of the 9th week there was no growth visible. After 3 months fairly numerous minute grey colonies were seen on the centre of the potato.
G.P.1269 (Inoculated with the original material)	13th	Similar.	Colonies were not visible until the 4th week. They had fused together at the end of 6 weeks, forming a thin grey layer.	In 2 months the surface was almost covered by a fine lace-like pellicle which did not increase subsequently.	No growth until the 9th week when two small white raised colonies were visible.	There was no growth until the 9th week, when two small whitish raised colonies were seen. In 3 months these had become conical and faintly yellow. At the end of 4 months the colonies were warty and numerous minute grey ones had appeared.

Virus B. XIV.

Pleura.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct. ('Grapy' masses from Pleura)	13th.	Uniform growth of moderate thickness at the end of the 3rd week.	At the end of the 1st week the growth was a little thicker than on serum alone. After the 3rd week the growths were about equal in amount, but that on glycerin serum was moister, and the surface was roughened by small, raised, white colonies. At the end of the 5th week the growth was thicker than on serum, and the colonies had become yellowish in colour.	On an asparagin medium the surface was covered in 5 weeks with a very delicate grey pellicle, composed of numerous small islands, which, later, sank to the bottom.	Growth during the 1st week was in the form of a thin, grey layer with small, white nodular colonies. During the next 7 weeks steady increase took place, the colonies becoming more raised. At the end of the 9th week there was a fairly thick, greyish-white growth of unequal thickness, covered with small raised colonies. No tendency towards wrinkling.	Numerous minute grey colonies appeared during the 2nd week. During the 3rd week a few became whiter and slightly raised. At the end of the 5th week there was a grey layer, consisting of very small raised colonies. There was no subsequent change.

A culture, isolated from G.P. 1271, inoculated with original material, covered the surface of broth (5 per cent. glycerin) in five weeks with a thin, uniform, translucent grey pellicle, which showed no tendency to sink. On the solid media the culture resembled that obtained direct.

Virus B. XV.

Mesenteric Gland and Mediastinal Gland.

Strain.	Generation	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct. (Mesenteric Gland)	7th. Agar 16th.	Moderately thick grey layer, showing a few white colonies on the surface in the 3rd week.	No growth occurred until the 4th week, when a few scattered grey colonies appeared. At the end of the 6th week the colonies had developed grey filmy margins, coalescing near the condensation water where the colonies were most numerous.	In 6 weeks the surface was covered with loosely aggregated delicate grey islands, speckled with whitish points. Later, the growth all sank. A growth similar in character was obtained on a 2 per cent. glycerin asparagin medium.	There was no growth until the end of the 4th week, when numerous minute grey colonies could be seen. At the end of the 5th week these had coalesced and their centres had become raised and opaque.	Minute greyish-white colonies were just visible at the end of the 5th week. In 7 weeks they were so numerous as to form a greyish-white layer. In 12 weeks a few colonies had become slightly raised; beyond this there was no increase.
G.P. 1348 (Inoculated with the Mediastinal Gland)	5th, Broth 8th.	Similar.	A few scattered minute colonies were seen at the end of the 3rd week. In the 4th week the colonies were small and greyish-white with grey margins.	On a 2 per cent. asparagin medium in 4 weeks, over half the surface was covered with an almost invisible grey network, most of which, at the end of 6 weeks, had sunk to the bottom.	No growth obtained.	In 5 weeks there was one small raised colony. In 7 weeks these were slightly raised, with spreading margins, and numerous very minute grey ones had appeared. In 3 months there were 4 pinhead-sized colonies with grey filmy margins, and numerous smaller colonies over the surface.

A culture was also isolated from G.P. 1347, inoculated with the original material at the same time as G.P. 1348 and showed identical characters.

VIRUS B. XVI.

BRONCHIAL AND MEDIASTINAL GLANDS.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct. (Bronchial Gland)	10th.	Uniform grey layer of moderate thickness.	Numerous minute grey colonies were visible in the 3rd week. These increased slightly in size up to the 4th week, growth then ceasing.	Inoculated on to 5 per cent. glycerin broth from a serum tube. About one third of the surface was covered with delicate grey islands in a month. One of these islands increased, forming at the end of 3 months a greyish white lace-like pellicle 1½ inches in diameter. On a 5 per cent. glycerin gelatine bottle sown at the same time an almost invisible grey film covered the surface in 11 weeks.	No obvious growth in 11 weeks.	There were minute greyish-white colonies visible at the end of the 3rd week. In 5 weeks these were fairly numerous. Similar colonies and numerous minute grey ones could be seen with a hand-lens. There was further increase until the 9th week; the final growth being a thin grey layer surrounding greyish-white raised colonies.
	12th.	Similar.	At the end of the 1st week there was a thin grey layer equal to that on serum. The layer showed no subsequent thickening but numerous discrete grey colonies appeared in the 2nd week, becoming raised and greyish-white in the 6th.	The surface was covered in 15 days partly with a very thin grey layer and partly with small grey islands. In 4 weeks the growth had sunk to the bottom.	At the end of the 1st week there was a thin grey layer with greyish-white points. The points developed into greyish-white colonies, the initial layer showing very little increase.	No trial.
G.P.1399 (Inoculated from the Mediastinal Gland)	3rd.	Uniform grey layer of moderate thickness.	In one week there was a very thin grey layer increasing until the 3rd week, becoming thicker in parts than on serum. In a second pair of tubes the final growth was rather better than on serum alone.		In 7 days there was a thin translucent grey layer which became a little thicker along the margins in 14 days. At the end of 6 weeks numerous delicate grey colonies had developed, some of which had become slightly raised.	At the end of 4th week there were numerous minute greyish-white colonies. In six weeks they had become more numerous. There was very slight increase in size up to the 3rd month.

Virus B. XVII.

Mediastinal Gland and Lung.

Strain.	Generation.	Serum.	Glycerin Serum	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct (Mediastinal Gland)	8th.	Thin uniform grey layer.	There was no change until the 3rd week when the surface was covered with minute discrete grey colonies. These increased a little in size but remained discrete.	The pellicle from the surface of the condensation water of a serum tube was sown on to 5 per cent. glycerin broth and a 2 per cent. glycerine asparagin medium. No growth occurred on the former: the surface of the latter was covered in 8 weeks with a delicate grey transparent membrane with fine white curved lines.	At the end of 3 months there was practically no growth.	A few minute grey points were seen with a lens at the end of the 3rd week. In 3 months there was no change.
G.P.1411 (Inoculated with the Lung)	3rd	Thin uniform grey layer.	There was a uniform layer equal to that on serum at the end of the 1st week. The growths were practically identical at the end of 6 weeks.	The condensation water pellicle sown on a 5 per cent. glycerin broth produced no growth.	No growth was obtained.	Minute grey points were visible with a hand-lens in 7 weeks. In 9 weeks these had formed definite raised colonies with grey filmy margins.

Virus B. XVIII.

Bronchial Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct.	3rd. Agar 11th. Broth 10th.	Uniform layer of moderate thickness.	Numerous minute grey colonies were visible in the 4th week. Subsequently the colonies developed delicate grey filmy margins which had coalesced at the end of the 6th week.	At the end of the 6th week there was one small grey speckled island. Four weeks later there were two small grey islands speckled with opaque white foci.	There was no growth during the first three weeks. At the end of the 6th week there was a thin grey granular patch of growth formed by coalescing colonies some of which had become raised and whitish.	No growth occurred until the 8th week when two minute colonies appeared. At the end of the 12th week there were three slightly raised creamy colonies and several minute grey ones. In 16 weeks there were fairly numerous discrete colonies ranging from a point to a pinhead in size.
Rabbit 160. (Inoculated with the original material)	5th.	Thin grey glazed layer.	Numerous minute colonies appeared in the 3rd week. These extended superficially and in 6 weeks had coalesced to a large extent forming a thin incomplete layer.		No growth.	In the 5th week a patch of minute grey colonies appeared. In the 7th week one was raised and creamy and in 10 weeks the small colonies had become confluent.

Virus B. XIX.
Bronchial Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	5th. Agar 14th.	Uniform grey layer of moderate thickness with whitish colonies on the surface.	No growth occurred during the first 2 weeks. At the end of the 3rd week the surface was covered with very numerous minute grey colonies coalescing to form almost a complete layer at the end of the 4th week. In 6 weeks there was a complete layer studded with greyish white raised colonies. The amount of growth then appeared greater than on serum alone.	A few small grey islands formed on the surface in 3 weeks.	No growth was obtained until the 4th week, when the surface became covered with very numerous minute grey colonies. In the 5th week these had increased in size but remained exceedingly thin and delicate.	In 4 weeks numerous minute white colonies were visible over the surface. In 6 weeks a few became slightly raised. There was no subsequent change.
G.P.1470 (Inoculated with the original material)	6th.	Similar.	During the first 3 weeks there was no growth. At the end of 6 weeks there were fairly numerous raised greyish-white discrete colonies ranging in size from a point up to nearly a pinhead.	No growth during the first 3 weeks. At the end of 6 weeks there were several small whitish nodules floating on the surface.	No growth during the first 3 weeks. At the end of 6 weeks there was a thin grey layer composed of delicate confluent colonies.	No growth at the end of 6 weeks.

Virus B. XX.
Bronchial Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct.	8th. Broth 14th.	At the end of 3 weeks there was a uniform growth of moderate thickness with a few raised white foci.	At the end of the 1st week there was a uniform grey layer, equal in thickness to that on serum, but with more raised foci on the surface. In the second week the growths were still equal. At the end of the 4th week the growth was distinctly thicker than on serum and was moist and greyish-white.	In 2 weeks the surface was practically covered with a delicate grey pellicle in part composed of islands, with scattered opaque white foci. At the end of the 3rd week the growth had sunk.	In 7 days there was a thin uniform grey layer increasing in thickness slightly up to the end of the 3rd week with the development of a few raised colonies. At the end of the 6th week there was a greyish-white granular layer with a few opaque white colonies forming a small warty patch.	A few whitish points were visible in 14 days. In 5 weeks there were fairly numerous minute greyish white colonies. In 8 weeks there was an increase in the number of the colonies which remained minute, some being visible only with a hand lens.
G.P. 1471. (Inoculated with original material)	5th	Uniform grey layer.	In 7 days there was a uniform grey layer, thinner than on serum: this increased slowly, parts becoming raised greyish-white moist and ribbed, parts remaining quite thin and transparent.	In 18 days there were three or four small grey translucent islands, which subsequently sank.	In 7 days there was a thin uniform ground-glass layer which had attained a moderate thickness at the end of the 3rd week, showing a slight fine wrinkling on the under surface. There was very little subsequent increase.	In 14 days there was a thin grey layer on the centre of the potato, which at the end of the 4th week became greyish-white and slightly raised. Growth then ceased.

A culture was also isolated from G.P. 1472, inoculated at the same time and with the same material as G.P. 1471 : this culture gave practically identical growths on the solid media, but on broth formed a uniform transparent grey pellicle covering the surface at the end of 6 weeks.

Virus B. XXI.

Bronchial Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	5th	Moderately thick uniform grey layer.	A few very minute colonies were visible in 14 days. At the end of the 3rd week there were numerous grey colonies closely aggregated in places. In 6 weeks there was very little further change	No growth obtained. On a second bottle several small speckled islands were formed.	In 8 weeks the only change was a slight thickening along one side of the slope of the inoculated material.	In 4 weeks minute grey points were seen. In 6 weeks discrete greyish-white colonies scattered over the surface were distinctly visible. In 8 weeks there were very numerous small slightly raised greyish-white colonies with grey filmy margins.

Virus B. XXII.

(?) Mediastinal Gland.

Strain.	Generation.	Serum	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
G.P.1508 (Inoculated with the original material)	4th.	Uniform grey layer of moderate thickness.	In the 1st week the layer was not so uniform and rather less than that on serum alone. In the 2nd and 3rd weeks the surface of the growth became whiter in parts with fine wrinkling the amounts of growth being about equal on the two media.	In 12 days there were a few delicate grey islands on the surface and growth was starting along the serum. In 3 weeks the surface was covered with an almost transparent pellicle.	In 7 days there was a thin transparent layer which increased slightly during the next 3 weeks. In 6 weeks there was a thin grey translucent layer varying slightly in thickness.	A slight increase of the inoculated material was noticed in 6 weeks.
A culture raised through G.P. 1509 showed similar characters, very little growth being obtained on potato, but a slightly thicker pellicle on broth.						
Dog 72	3rd.	Moderately thick grey glazed layer.	In the 1st week the layer was thicker than on serum without glycerin and greyish white. At the end of four weeks there was a thick moist greyish white layer with fine wrinkling.	The surface was covered in 4 weeks with a uniform grey translucent skin with whitish foci. There was no further thickening.	In 14 days there was a moderately thick dry greyish white layer. In 4 weeks the surface of the growth showed a fine raised wrinkling. There was no further thickening but subsequently opaque creamy colonies developed.	At the end of the 1st week there was a thin grey layer. From this after several weeks raised irregular warty colonies grew.

Virus B. XXIII.

Lung.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	5th	Thin uniform grey layer in 3 weeks.	In 7 days there was a uniform grey layer distinctly better than that on serum. In 14 days the growth was still better and in 3 weeks there was a moist greyish white layer of unequal thickness apparently double the amount of that on serum. No further change was noted.	The surface of the broth was covered with a number of semi-transparent grey islands not forming a complete pellicle. With the 8th generation the surface was in 3 weeks incompletely covered by a pellicle so delicate as to be almost invisible.	In 7 days there was a thin uniform grey ground-glass layer which increased a little in thickness until the end of the 3rd week. There was then a grey translucent layer slightly thicker and opaque in parts.	The surface was covered in 7 days with a greyish growth composed of minute colonies. These slowly increased in size, a few at the end of the 4th week becoming slightly raised—one attaining the size of a pin-head. At the end of the 10th week there was a greyish white layer with a granular surface.

A culture was also isolated from G.P. 1,534 inoculated with the bronchial gland ; this culture resembled that obtained direct, but on broth formed a uniform translucent pellicle which covered the whole surface and began to sink at the end of 6 weeks.

Virus B. XXIV.

Lung and Bronchial Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct ('Grapy' growths from Lung.)	4th.	Uniform layer of moderate thickness.	There was no growth visible until the end of the 3rd week when very numerous minute discrete colonies were observed with a hand lens. At the end of the 4th week they formed a thin grey layer. There was no further change.	No complete pellicle was formed, the surface of the broth after 6 weeks showing only a few very small islands. Some transparent, others greyish-white and granular.	During the first 6 weeks there was no growth. At the end of the 10th week there were three flat grey colonies — the largest about 2 mm. with raised opaque centres.	In 4 weeks minute grey colonies were seen with a hand lens and one or two visible to the naked eye. In 5 weeks the surface was covered with very numerous greyish-white colonies forming a thin layer. There was no subsequent change.
G.P. 1548 (Inoculated with the Bronchial Gland.)	4th.	Uniform layer of moderate thickness at the end of the 3rd week.	Numerous minute colonies were just visible at the end of the 3rd week. In 4 weeks the surface was covered with numerous grey colonies which at the end of 6 weeks had grown larger and to a great extent coalesced.	At the end of six weeks there were numerous transparent islands and opaque white fragments of growth: the surface was not covered.	No growth visible in 10 weeks.	In 5 weeks there were a few minute transparent grey colonies with greyish-white centres. These colonies did not increase in size.

Cultures of this virus were raised direct from the lung and bronchial gland and from the bronchial gland through G.P. 1548. The culture isolated from the bronchial gland was identical with those described above.

Virus B. XXV

Pleura.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	9th	Thin uniform, grey layer not increasing after the 2nd week.	In the 1st week there was a uniform grey layer thicker than on serum alone. At the end of the 2nd week the growth was greyish-white and slightly ribbed round the margins. At the end of the 4th week the layer was two or three times as thick as on serum alone, with a greyish-white roughened surface.	In 11 days the surface was covered with a semi-opaque grey pellicle, climbing up the sides of the bottle. It subsequently began to break up and showed a tendency to sink without further thickening.	In 7 days the surface was covered by a grey layer of unequal thickness. At the end of the 2nd week it was quite thick and opaque in streaks and showed a tendency to wrinkle, which became more definite at the end of the 3rd week, after which there was no change.	Growth took place from the outset in the form of a thin grey moist layer. This remained unchanged until the 2nd month, when small colonies appeared which subsequently became large, irregular, and yellow in colour.
G.P.1607 (Inoculated with the original material)	5th	Similar	Similar.	In three weeks the surface was covered by a thin grey translucent pellicle which did not thicken.	In the 1st week there was a thin grey layer with small whitish patches. This increased until the end of the 3rd week, being for the most part translucent but thick and opaque in patches.	Thin grey layer with the formation of raised white colonies at the end of 5 weeks.

Virus B. XXVI.

Bronchial Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	7th	Uniform grey layer of moderate thickness.	No growth occurred until the 3rd week, when the surface was covered with minute grey colonies. These coalesced in the 4th week, forming a thin grey layer which did not increase.	In 8 weeks $\frac{3}{4}$ of surface was covered with delicate grey islands speckled with opaque foci and small warty nodules. At the end of the 13th week there was no change except for some increase of the whitish nodules. There was no tendency to sink.	No growth occurred in 7 weeks. In 10 weeks there were 2 raised irregular colonies, the largest about the size of a pinhead. In the second generation after 5 weeks there were several small delicate grey colonies. These spread slightly, remaining thin, with minute opaque centres.	In the 3rd and 4th weeks a few minute white colonies became visible and showed no further increase. In the 2nd generation no growth occurred until the 5th week, when minute white colonies were visible. These increased, and in 3 months were fairly numerous, in size from a point up to 1 mm., with delicate grey margins.

A culture was also isolated through Rabbit 380, which was inoculated with the Original Material; it was identical in characters with the direct culture.

Virus B. XXVII.

Diaphragm.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	4th	Uniform grey layer of moderate thickness.	At the end of the 1st week there was a greyish white layer thicker than on serum alone. In the 2nd week it was about twice as thick, and showed at the end of 6 weeks little further change.	In 10 days there was a uniform grey pellicle covering $\frac{3}{4}$rds of the surface. In 14 days the surface was completely covered with a greyish white translucent pellicle, with small opaque patches, already showing a tendency to sink. The growth subsequently sank without further thickening.	At the end of the 1st week the surface was covered by an unequal grey layer of moderate thickness in parts. The thicker patches increased, and became slightly ribbed at the end of the 4th week. In the 2nd generation there was formed at the end of the 4th week a greyish-white translucent layer which had slowly increased from the outset.	In the 1st and 2nd weeks there was a thin grey layer which showed no subsequent increase in thickness. At the end of the 11th week there were scattered small raised white colonies growing from the grey layer. In the 2nd generation there was formed in the 1st week a thin grey layer, which increased slightly up to the end of the 3rd week.

Virus B. XXVIII.

Thoracic Gland. (?)

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	4th	Uniform grey layer of moderate thickness at the end of 3 weeks.	A uniform layer was formed in the first week a little thicker than on serum. It became at the end of the 4th week nearly twice as thick as on serum remaining uniform.	In 14 days there was a grey translucent island about one inch square. At the end of 5 weeks there were 4 grey islands covering about one-sixth of the surface. There was no further increase. On a second bottle the result was similar.	At the end of 12 days there was a grey ground glass layer over the surface. This increased until the 4th week, becoming finally wrinkled in parts. There was no subsequent increase.	During the first 2 weeks no growth was evident. At the end of 3 months there was a thin grey layer, with yellowish colonies up to a millet seed.

Virus B. XXIX.

Mediastinal Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	4th	Uniform grey layer of moderate thickness.	Growth took place from the outset and was rather thicker than on serum alone at the end of the 2nd week. At the end of the 4th week there was a grey layer not quite uniform and nearly twice as thick as on serum alone.	In 4 weeks the surface was not quite covered by a grey translucent almost uniform pellicle with minute white foci. This remained unchanged and showed no tendency to sink.	In 14 days there was slight increase where the inoculated material was thickest, and a patch of thin grey growth near the condensation water. Six weeks later there had been very little change.	There was a thin greyish moist layer becoming definite at the end of the 3rd week. There was no further thickening, but at the end of the 6th week there were three small raised white colonies.

Virus B. XXX.

Bronchial Gland.

Strain.	Generation.	Serum.	Glycerin Serum.	Glycerin Broth.	Glycerin Agar.	Glycerinated Potato.
Direct	3rd.	Thin uniform grey layer.	Growth was of the same character as on serum and progressed at the same rate until the end of the 3rd week. Subsequently the layer became a little thicker and moister than on serum and showed raised white foci on the surface.	In 4 weeks the surface was covered with a thin grey translucent pellicle on which are scattered irregular whitish foci.	In 8 days there was a slight increase of the inoculated material. There was a slow increase until the end of the 4th week, a very thin and incomplete grey layer being formed.	No growth.

Plate 1.

CULTURES OF TUBERCLE BACILLI ON GLYCERIN AGAR.

Showing the influence of glycerin in increasing cultural luxuriance.

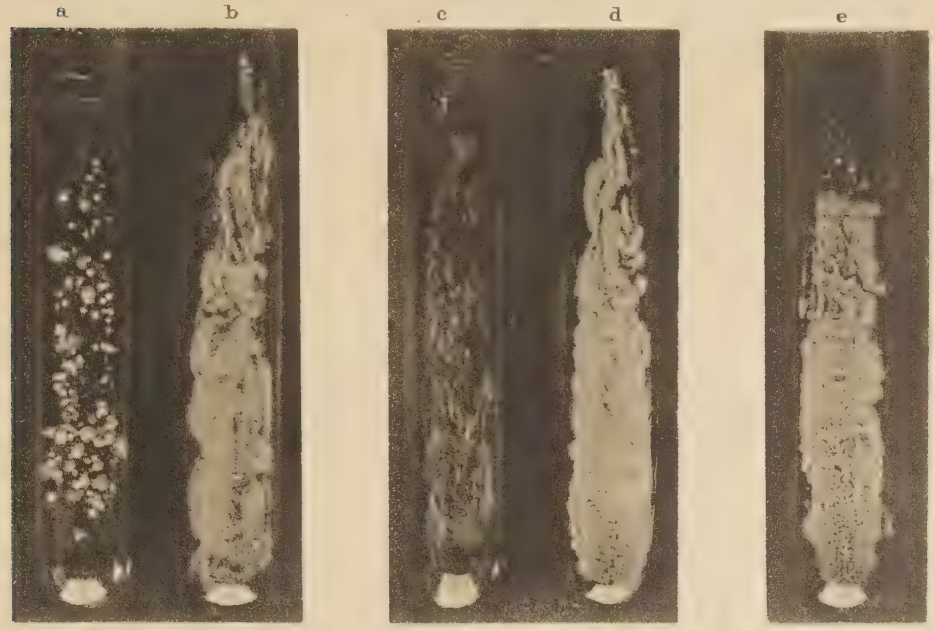


Fig. I. Fig II. Fig. III.

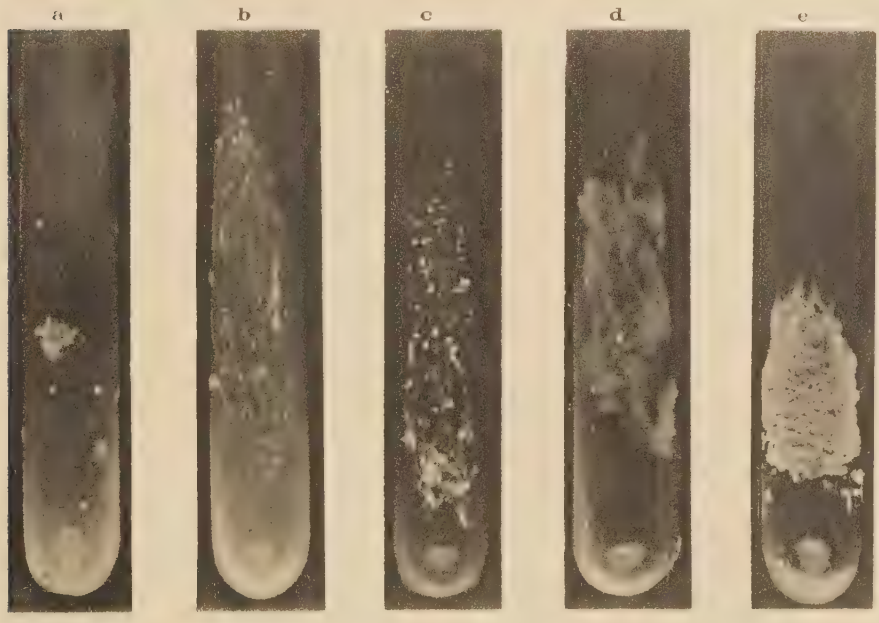
Fig. I. Illustrates the type of growth produced on glycerin agar by bovine bacilli of Class I.
- a. Grown since isolation on pure serum.
- b. Grown for several generations on glycerin serum.

Fig. II. Illustrates the type of growth produced on glycerin agar by bovine bacilli of Class III.
- c. Grown since isolation on pure serum.
- d. Grown for several generations on glycerin serum.

Fig. III. A culture on glycerin agar of an easy growing tubucle bacillus of low virulence, included for comparison

CULTURES OF TUBERCLE BACILLI ON GLYCERIN AGAR.

Showing different types of growth.



a. Virus B.I. Strain derived from Heifer 28. (Prescapular gland) through Guinea-pig 540.
b. Virus B.II. „ „ „ the original material, through the Guinea-pig.
c. Virus B.V. „ „ „ Heifer 80.
d. Virus B.IV. „ „ „ Dog 18. (mesenteric gland)
e. Virus B IX. „ „ „ Dog 50. (Lung)

Plate 2.

a. b. c. d.

CULTURES OF TUBERCLE BACILLI ON GLYCERIN GELATINE.

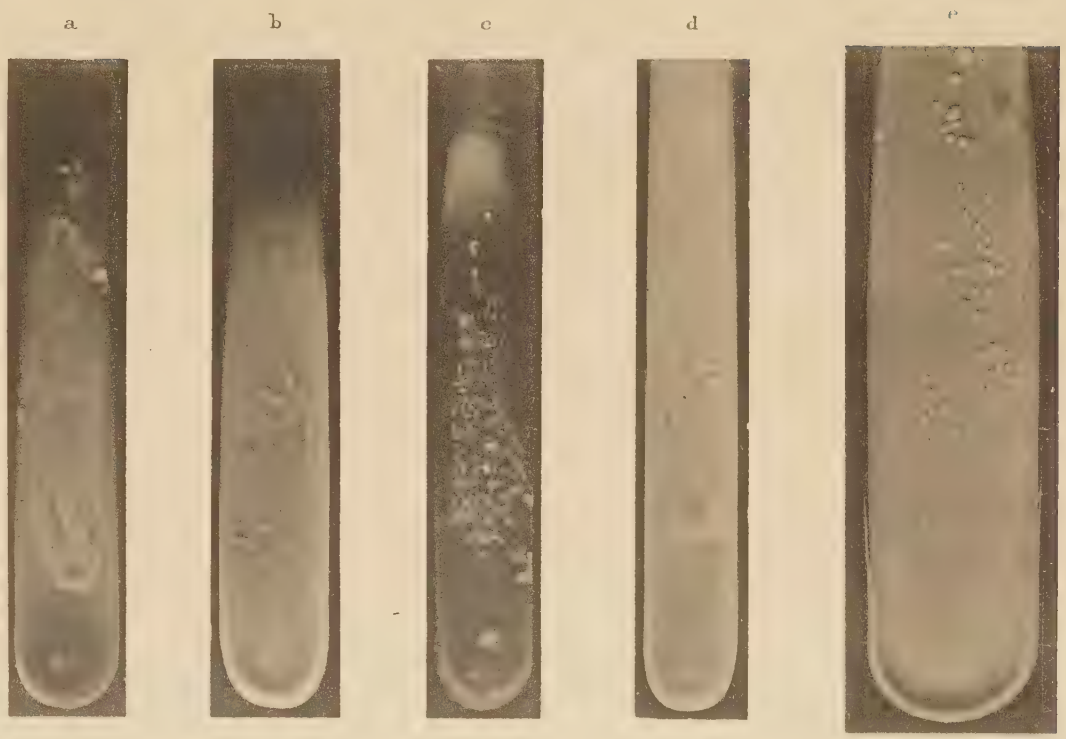
a. Derived from the original material of Virus B IV.

b. „ „ the original material of Virus B XI. through Rabbit 66.

c „ „ the supramammary lymphatic gland of Cow 44 (Virus B. I.)

d „ „ the lung of a Dog 50 fed with 1 milligramme of culture from Virus B. IX.

PRIMARY CULTURES OF BOVINE TUBERCLE BACILLI
ON VARIOUS MEDIA.



a. Culture on Cat serum, 3 months old.
b. Culture on Pig serum, 3 months old.
c Culture on Dog serum agar, 3 months old.
d. Culture on bovine serum + 5% glycerine, 3 months old.
e. Culture on the egg medium, 4 months old.

Plate 3.

THE CULTURAL CHARACTERS OF THE TUBERCLE BACILLI OBTAINED FROM MAN.

Dr. L. COBBETT.

THE CULTURAL CHARACTERS OF THE TUBERCLE BACILLI OBTAINED FROM MAN.

During the last three and a half years tubercle bacilli from sixty cases of human tuberculosis have been cultivated, and many strains raised, some direct from the human tissues, some from animals injected from them, and others again after the strain had passed through a greater or smaller number of animals of various kinds.

Over 160 of these strains have been examined as to their mode of growth on various media, both in primary culture, and in secondary cultures extending over a varying period of time. Many of the strains have been in continuous cultivation since their isolation up to the present time, some for over three years. Many, too, have been grown in large quantities for the injection of animals, and this has offered an opportunity of *measuring* the relative amount of growth on different media.

Cultures have been made on all sorts of media including the serum of many different animals, and egg, as well as serum, agar, broth, and potato to which 5 per cent. of glycerin has been added. Other media also have occasionally been used, such as glycerin-serum-agar, glycerin-egg, glycerin-serum-broth, and broth without any glycerin. The effects of long continued cultivation on serum with and without glycerin have been observed.

When working with cultures of bovine origin at Walpole Farm, I observed that those which were then under observation would not grow upon glycerin-serum, or grew less readily upon that medium than upon serum to which no glycerin had been added. On being transferred to Blythwood Farm and commencing to work with bacilli of human origin, I observed that the majority grew far more luxuriantly on media which contained glycerin than upon those which did not, while a minority grew like the bovine bacilli which I had cultivated at Walpole. The former were only slightly virulent for the ox and rabbit, the latter were highly virulent for these animals. Further experience, however, showed that some of those strains which were highly virulent for these animals, grew rather better on glycerin-serum than upon serum which contained no glycerin, but not so luxuriantly as those which were only slightly virulent for these animals.

Referring now only to tubercle bacilli of human origin, it may be said that different strains grow very variously on a given medium. On serum and egg the differences are not marked; they are far greater on media which contain glycerin. Thus on glycerin-serum there may be a thick nodular continuous cream-coloured growth, or a thin scarcely visible grey film formed of separate points; there may be no growth at all, or anything between those extremes. On glycerin-agar there is often no growth, often a colourless grey film of small points, and frequently again there is a more or less continuous nodular or wrinkled growth of raised cream-coloured colonies. Sometimes, when the colonies are few and widely isolated, they grow into large and wrinkled pyramids. The growth on broth is equally varied. There may be a partial and delicate grey film, or a thick layer which covers the surface, climbs the side of the vessel and becomes wrinkled. Some cultures form a thin delicate reticulated membrane resembling lace, others, large thick islands, the surface of which is roughened by innumerable projections. In some the growth remains very limited and forms a small thick isolated island, which after a long period of apparent quiescence may send out a thin film which rapidly covers the surface. Experiment has shown that a high degree of acidity increases the tendency to the formation of a thick limited patch of growth, and it may well be that the rather sudden changes in the character of growth just described are due to the lessening of the acidity below a certain unfavourable point. This kind of change has been observed in the growing strains which have no tendency to form acid.

The growth of a given strain on a given medium is variable, conditions not entirely under our control apparently playing a dominant part. On liquid media an accidental shake may cause the commencing film to sink, and growth to become arrested. To encourage growth on liquid media various devices have been tried. A small piece of cotton wool in the broth, just sufficient to rise to the surface, has been found to afford a convenient resting place for the bacilli. But the plan which usually succeeds best is to pour a little glycerin-broth into a serum tube which is already growing well, and when floating growth has appeared on this to transfer it to a flask containing the liquid medium.

In consequence of the uncertainty of growth of a given strain on a given medium, in spite of all efforts to obtain constant conditions, no conclusions as to cultural characters can be drawn except after repeated observations, only the best growths obtained with a given strain on a given medium being taken as typical.

No systematic observations covering all the strains grown here have been attempted. I therefore do not propose to submit a detailed report of my observations on cultures, but will briefly summarise the conclusions to which I have been led.

(1) Tubercle bacilli from man fall into two groups, which present fairly distinct characters of growth.

(2) These characters are determined by *quantity* and *rate* of growth on certain culture media. The ultimate quantity appears to me the more significant and useful, but others have thought that the rate of growth was the most distinctive.

(3) The differences in the quantity of growth—as shown by the thickness, colour and other appearances of the film—produced by typical strains of the two groups, is not marked on pure serum, and no classification according to the cultural characters on this medium would be possible. It is on media containing *glycerin* that the difference becomes marked.

It is perhaps most marked on glycerin-agar and glycerin-broth, but those media are apt to be uncertain, and growth on them at the best is slow. Glycerin-serum appears to me to be more certain, and on it results are more quickly obtained.

(4) The two cultural groups into which human tubercle bacilli fall may be defined as including—

1. All those strains the growth of which in early cultures is little, if at all, aided by glycerin; and

2. Those which grow much more luxuriantly on media to which glycerin has been added.

(5) A rough but useful method of making preliminary observations on the cultural characters of a given strain is to sow it on serum and glycerin-serum and to compare the two growths after three weeks. If the growth is decidedly better on glycerin-serum than upon pure serum, it will certainly belong to the second group. If it is better or not markedly inferior on pure serum it will belong to the first group. This test will not apply, as will presently be explained, to old cultures, or to those which have become accustomed to glycerin media.

(6) The unavoidable variations in our culture media, and the variations in characters of growth which may depend upon them, and also perhaps on other factors which

are not under our control, are such that the characters of a given strain cannot be determined without repeated cultivation; and in forming our judgment *only the best growths on a given medium should be considered characteristic.*

(7) It has been shown elsewhere that tubercle bacilli from man, classified according to their *virulence* for certain animals, fall into two principal groups. It is otherwise with the groups into which the bacilli fall, when classified according to their cultural characters.

Cultural characters go, to a certain extent, hand in hand with virulence. The extreme members of each cultural group differ greatly from each other, but there are others where the difference is less marked. Thus while comparative virulence does not seem to vary among the members of each group, cultural characters do. It has even been doubted whether it is reasonable to make two cultural groups, and whether after all the various strains do not form a series in which each presents minimal differences from its neighbours. It is true that examination of the cultural characters on glycerin-agar of a large number of different strains showed that they formed a continuous series in which there was no gap sufficient to justify a division into two groups. But this, I believe, was, to a great extent, explained by the fact that the strains were not strictly comparable in that they had been a very different length of time under cultivation. And it is an easily demonstrable fact that tubercle bacilli gradually adapt themselves to our artificial culture media and grow better upon them after continued cultivation than when first isolated from the animal.

Bacilli which grow little or not at all on glycerin media when freshly isolated from the living animal body acquire the power of growing on these media after long-continued cultivation. And those which grow fairly well on it at first may acquire a luxuriance of growth after several generations. The power of growing, or of growing better on glycerin media, is acquired to some extent by cultivation on media such as serum which contain no glycerin, but the adaptation is carried further if the strain is accustomed to glycerin itself, by being grown on media which contain it. In this way strains of bacilli which belonged originally to the first group come to be indistinguishable from members of the second group; that is, they are indistinguishable by their cultural characters alone; their virulence has not altered.

(8) For these reasons a classification of tubercle bacilli according to their cultural characters can only be made by comparing strains which have been the same length of time under artificial cultivation, and always on the same medium. The shorter the period of cultivation probably the better. If primary cultures could always be used for comparison, I believe that strains with intermediate characters of growth would to a large extent disappear, and each group would contract and become more sharply distinct from the other. It is, of course, too much to ask that cultural characters on such media as glycerin-broth-agar or -potato should be determined in primary cultures, but I am of opinion that early cultures should be exclusively used, that comparison should be made between cultures whose total duration of cultivation is approximately the same. If this were done I admit there would still be a considerable difference between the members of each group, but I believe that the two groups would show themselves to be distinct.

(9) It has been said that cultural characters and virulence go to some extent hand in hand. In the modification experiments, whenever alteration of virulence occurred, there was a corresponding alteration of cultural characters. The original cultures of low virulence grew well on glycerin media, and after they had acquired virulence for the calf, etc. they grew very badly on these media. But on the other hand an alteration of cultural characters may take place without any change of virulence, and virulent strains which would not at first grow on glycerin media were found to have lost no virulence after they had adapted themselves to this medium. Moreover, while the virulent strains originally differed greatly in their capacity to grow on glycerin media no corresponding difference of virulence was observed.

It may, however, be said that the two groups into which human tubercle bacilli are divided by their cultural characters are in the main the same two groups into which they are separated by their comparative virulence for the ox.

After one had gained some experiene, one seldom, if ever, was unable to foretell the comparative virulence of a strain from a cursory study of the characters of its early cultures. It was often on these grounds alone that the decision rested whether or no calves should be injected with 10 mg. doses. Only once was this dose injected unnecessarily, and the mistake was afterwards found to have been due to the use of an unsatisfactory batch of glycerin-serum tubes.

The opinions expressed in the above summary are based not only on the impressions gained day by day and from notes taken at the time, but also on the comparative examination of large numbers of typical cultures on all sorts of media which have been preserved. Early impressions could be checked by the gradual growth of experience, and opinion based on cultures grown a year or more earlier could be corrected by comparing the actual cultures with those grown to-day. Each culture tube was marked by the number of generation, and the total duration of cultivation up to the time of sowing that tube could be ascertained by comparing its date with the date of isolation.

An actual comparison of the relative amount of growth on serum with and without glycerin of certain selected strain may be of interest, though the results are much spoilt by variations in the total duration of artificial cultivation among the strains used.

The method was as follows: When serum cultures were sown for the purpose of getting bacilli with which to inject animals, other cultures were sown on tubes of the same serum to which 5 per cent. of glycerin had been added. The tubes used were large ones with a constricted neck, and the amount of surface exposed by the culture medium in each was 7 in. by more than 1 in. Several tubes of each kind were sown, and if any of them grew less well than their fellows they were rejected. Thus only cultures which were considered satisfactory were measured. At the end of three weeks' incubation all the bacilli from each set of cultures were removed and emulsified by rubbing between ground glass plates and suspended in a known volume of salt solution.

The relative quantity of bacilli in the two suspensions was determined by taking samples of each and centrifuging in capillary tubes, known to be of uniform calibre by careful calibration.

The results are given in the following table.

The serum of adult bovine animals has consistently proved to be a better medium for growing tubercle bacilli than the serum of the calf. The addition of glycerin to the latter makes it a far better medium for the cultivation of bacilli belonging to Group II., but the serum of adult bovine animals is not improved to the same extent. It would seem that it already possesses something which calf serum lacks and which to some extent can be replaced by glycerin. As a consequence, adult serum does not bring out the difference caused by the presence or absence of glycerin in so marked a manner as does calf serum. In the experiments now under consideration calf serum was employed except when otherwise stated. With the serum of one particular cow (No. 9), which was used in several of the experiments, the difference between the amount of growth on serum and glycerin-serum was particularly small.

TABLE SHOWING THE RATIO OF THE QUANTITY OF GROWTH ON SERUM TO THAT OF GLYCERIN-SERUM.

(1) GROUP II.

Virus.	Strain.	Total duration of artificial cultivation.	Generation of culture.	Age of culture in days.	Ratio of amount of growth on :	
					Glycerin-serum.	Serum.
H 26. "K.M." -	Human kidney - - - -	13 months	11th	21	16·0	1
H 45. "F.M." -	Human bronchial gland - -	11 „	9th	21	4·5	1
H 9. "C.T." -	Calf 201 - - - - -	10 „	6th	21	3·5	1
H 18. "T.T." -	Calf 405 - - - - -	8½ „	6th	21	6·0	1
H 15. "I.W." -	Calf 413 - - - - -	8 „	9th	21	4·4	1
H 11. "E.D." -	Calf 309 - - - - -	8 „	11th	21	4·5	1
H 30. "E.M." -	Human mesenteric and bronchial glands.	4½ „	4th	21	3·3	1¶
H 47. "S.B." -	Human hip joint - - -	4 „	5th	21	1·67	1†
H 51. "H.M." -	Human lung - - - -	4 „	5th	21	6·3	1‡
H 50. "P.H." -	„ „ - - - -	3 „	5th	21	2·9	1†
H 44. "D.C." -	Human cervical glands - -	4 „	8th	21	3·8	1
H 43. "F.F." -	Human knee joint - - -	2 „	4th	17	2·6	1
H 41. "A.S." -	„ „ „ - - -	2 „	3rd	21	5·4	1
H 46. "H.W." -	„ „ „ - - -	2 „	6th	21	2·4	1
H 48. "W.P." -	Human lung - - - -	1½ „	3rd	21	1·2	1†

* After growing 5 generations on glycerin-serum. † Serum of Cow 9. ‡ Serum of Cow 259. ¶ Pig serum used.

TABLE SHOWING THE RATIO OF THE QUANTITY OF GROWTH ON SERUM TO THAT OF GLYCERIN-SERUM.

(2) GROUP I.

Virus.	Strain.	Total duration of artificial cultivation.	Generation of culture.	Age of culture in days.	Ratio of amount of growth on :	
					Glycerin-serum.	Serum.
H 7. "C.M." -	Calf 5 - - - - -	2½ years	17th	21	1·3	1†
„ „ -	„ „ - - - - -	2⅝ „	21st	21	4·0	1*
H 14. "F.S." -	Human mesenteric glands -	2 „	19th	20	1·44	1
„ „ -	„ „ „ -	2 „	19th	21	1·	1‡
H 19. "S.W." -	Goat 11 - - - - -	1½ „	16th	21	1·36	1
H 20. "F.L." -	Calf 213 - - - - -	17 months	15th	22	1·88	1
H 31. "L.F." -	Human cervical glands - -	14 „	12th	21	1·9	1
H 28. "C.L." -	Rabbit 102 - - - - -	12 „	13th	21	1·5	1
„ „ -	„ „ - - - - -	12 „	14th	31	1·6	1**
H 16. "J.H." ¶ -	Calf 337 - - - - -	12 „	11th	21	1·2	1†
„ „ -	Calf 317 - - - - -	10 „	10th	21	0·88	1
H 29. "M.F." -	Human cervical glands - -	8 „	10th	22	1·13	1
H 17. "Sp.B." ¶ -	Calf 553 - - - - -	3½ „	4th	21	0·76	1
„ „ -	„ „ - - - - -	3½ „	4th	21	0	N

** After growing for 1 year on glycerin-serum. † Serum of Cow 9. ‡ Serum of Cow 259.
¶ After increase of virulence.

TABLE SHOWING THE RATIO OF THE QUANTITY OF GROWTH ON SERUM TO THAT OF GLYCERIN-SERUM.

(3) MIXTURE OF GROUPS I. AND II.

Virus.	Strain.	Total duration of artificial cultivation.	Generation of culture.	Age of culture in days.	Ratio of amount of growth on :	
					Glycerin-serum.	Serum.
H 8. "S.C." and H 10. "B.S." [1st Mixed Culture]	H 8. "S.C."; Human mesenteric glands.	"S.C." 2½ yrs.	19th	21	5·6	1
	H 10. "B.S." Calf 103.	"B.S." 1½ yrs.	14th			

REPORT ON THE CULTIVATION OF TUBERCLE BACILLI AT BLYTHWOOD.

By Mr. H. J. HUTCHENS (Assistant Investigator).

REPORT ON THE CULTIVATION OF TUBERCLE BACILLI AT BLYTHWOOD FARM.

From November, 1903, to June, 1905, by H. J. Hutchens.

The work here reported was undertaken at Blythwood Farm, while acting as assistant to Dr. Cobbett, with the object of determining the cultural characters of tubercle bacilli derived originally from various tuberculous lesions in the human subject.

At the outset, considerable difficulty was encountered in isolating bacilli in pure culture and in propagating them under conditions of artificial cultivation. The methods of isolation and of cultivation and the preparation of the media selected on which to grow the bacilli in vitro are therefore also described.

In a few instances cultures have been raised by sowing the human material on suitable culture media, but in the vast majority of cases the human material was inoculated into experimental animals, and the lesions subsequently developing in the latter were the immediate source of the cultures.

The different human lesions used for experiment are described as viruses; and cultures obtained either from these lesions or from lesions produced by them in experimental animals as strains of bacilli. The animals from which the different strains were immediately derived are stated in every case.

In all, 141 strains of bacilli derived originally from forty-seven different human sources have been isolated and examined.

The Media used for Cultivation and their Preparation.

The media most commonly used were—

α. Blood serum.

β. Blood serum to which a solution of glycerin had been added.

γ. Glycerin agar.

δ. Glycerin broth.

Preparation of the media.

α. *Blood serum.*—Blood for the preparation of serum was obtained from various animals, viz.—

1. Bull, cow and calf.
2. Horse.
3. Pig.
4. Goat.
5. Dog.
6. Cat.
7. Rabbit.
8. Guinea pig.
9. Monkey.

Blood was collected in suitable sterile vessels and the serum drawn off by means of a sterile bulb and cannula, distributed at once in convenient quantities into sterile test tubes, which were then heated gradually in a sloped position to a temperature of about 75°C. in an atmosphere saturated with water vapour until the serum was so firmly set that it no longer quivered when the tubes were tapped on the thumb-nail. As soon as the tubes had cooled they were carefully and completely sealed with paraffin wax. The tubes were finally incubated for three or four days before being used, in case some error of technique had permitted the entrance of contaminating organisms into one or more tubes.

An iridescence was generally observed on the surface of the serum when it had cooled after coagulation. This did not appear to seriously interfere with the growth of the tubercle bacillus provided that other conditions of the medium were satisfactory. Further experience, however, showed that the formation of an iridescent film could be almost, if not entirely, obviated by raising the temperature very gradually to the point of coagulation, and removing the serum from the inspissator immediately it was firmly set, and before the temperature reached an unnecessarily high degree.

β. *Blood serum with the addition of glycerin.*—Blood serum with the addition of 1·25 per cent. and 5 per cent. of glycerin was very largely used in the work under review. This medium was prepared by adding to ordinary serum before coagulation, in the former case 25 per cent. of its volume of a 5 per cent. solution of glycerin in water, and in the latter case 10 per cent. of a 50 per cent. solution of glycerin in water—the glycerin solutions being, of course, previously sterilized. The medium was then coagulated, sealed and incubated as in the case of blood serum.

γ. *Glycerin Broth.*—The perfectly fresh muscles from the calf or adult bovine animal were finely minced, and tap water added in the proportion of one litre to one pound of meat; 0·5 per cent. sodium chloride added, stirred and allowed to stand overnight. Next morning it was boiled, then strained, and subsequently 2 per cent. Witte's peptone added. It was then boiled again, filtered, and 5 per cent. glycerin added, being finally sterilized in the ordinary manner. The reaction of a broth so prepared was found to be neutral or very faintly acid to litmus, and to phenol-phthalein acid, approximately to the extent of 20 c.c. normal acid per litre.

δ. *Glycerin Agar.*—Was prepared by adding 2 per cent. agar to the glycerin broth prepared as above.

It was usual, especially in the latter part of the work, to seal with paraffin wax all tubes, flasks, and bottles, in which media were stored, and also while they were in the incubator after sowing.

The Selection of the Material from which to isolate Bacilli in Pure Culture.

α. *Human.*—Cultures were raised in several instances directly from human material by scraping the tuberculous tissue and sowing tubes with the material so obtained. Mesenteric and cervical glands have proved particularly useful for this purpose.

β. *Bovine.*—The bovine experimental animals at Blythwood were, in the great majority of cases, inoculated subcutaneously in the neck with material of human origin.

Almost invariably a tuberculous lymphatic gland has been used and generally the prescapular, prepectoral or long posterior mediastinal. Organs such as the lung, liver, spleen and kidney of the bovine, though not often used, have not given good results. Experience early showed that old calcareous and fibrous lesions were also to be avoided, because of the frequency with which cultures sown with such material failed.

γ. *Guinea Pig.*—Any structure affected with the disease was found to readily yield a culture. As a rule a caseous and softened gland, *e.g.* the inguinal, post manubrial, omental or hepatic, or the omentum or spleen or—when the inoculation was intra-peritoneal—a scraping from the peritoneum or the lung have been used. The kidney and liver have not yielded such good results on the relatively few occasions on which they have been utilised.

δ. *Rabbit.*—The omentum, lung, or rib-marrow. Most of the rabbits were inoculated intraperitoneally.

ε. *Monkey.*—A softened and caseous lymphatic gland has been the tissue from which cultures have been sown.

ξ. *Goat.*—The prescapular or long posterior mediastinal gland or the kidney.

η. *Rat.*—Lung and spleen have been used.

When the animal was but slightly affected it was found advisable, in order to prevent disappointment, to sow tubes from several different organs or tissues. This was also done when the lesions present were old and calcareous. Similarly, if only one gland in the bovine or goat was affected better results were obtained by sowing from different parts than all from the same part.

The Technique of raising Primary Cultures.

Various procedures have been adopted for sowing the culture media. Thus, in the case of a softened caseous gland the capsule was sterilised by heat, a small opening made and the pultaceous matter squeezed out drop by drop and smeared over the surface of the medium with a platinum loop.

When the peritoneum was selected, the surface was scraped with a blunt knife and the material so obtained rubbed over the surface of the culture medium.

The omentum was dealt with in a variety of ways; thus, a small piece was sometimes cut off, placed on the surface of the culture medium, and then rubbed over its surface; or, after cutting off a small piece, the fluid and small pieces of tissue obtained by crushing between ground glass plates were rubbed over the medium; or again, if caseous and softened points were present, these were cut into and the material picked out on the point of the knife and used for sowing the tubes.

From the spleen or lung or tuberculous gland which had not softened, it was usual to cut out a small piece, or scrape the tissue with a knife and then sow the tubes, or, having cut out a piece, to crush it between ground glass plates and distribute the crushed tissue with a platinum wire or, if fluid, to pick it up with a piece of glass tubing drawn out to a capillary diameter and spray it over the medium.

After well distributing the material over the surface of the medium the tube was incubated without further interference until growth appeared.

The Development of Primary Cultures.

It has very rarely happened that no growth of tubercle bacilli has occurred when the animal tissue was sown, by one of the methods described, on to the surface of any good serum whatever.

A. *Media used for raising Primary Cultures.*

Coagulated blood serum obtained from various animals with and without the addition of glycerin have been the media commonly used for the purpose of raising primary cultures. Three or four tubes of coagulated blood serum were always used and generally one or two tubes of glycerin serum were sown at the same time. Occasionally a tube of glucose serum or serum agar has also been used. Other media have not been regularly employed for raising primary cultures in these investigations.

B. *Comparison of Values of different Media for Primary Cultures.*

Of the various media used, serum has been the most useful medium upon which to sow primary cultures, for while all tubercle bacilli from whatever source obtained grew more or less readily on serum, bacilli were met with which did not grow at all, or only with great difficulty in primary culture on glycerin serum.

In those cases in which a tube of serum and a tube of glycerin serum were inoculated with material from the same source it was generally found that growth appeared on the serum before it appeared on the glycerin serum.

Glucose serum appeared to be equal to, and perhaps rather better than, serum. And of the various kinds of serum which have been used the most successful results have been obtained with serum from the dog and pig; nevertheless the majority of my cultures have been raised in the first instance on bovine serum, and in my experience a rich golden serum from a milking Jersey cow is as good as any serum obtained.

Glycerin agar has not in my experience proved suitable for raising primary cultures. Serum agar, moreover, has only rarely been satisfactory.

C. *Characters of Growth in Primary Culture.*

These were very variable and were found to depend, in part upon the manner in which the material was sown, and in part upon the number of bacilli present which subsequently multiplied. Other factors influencing the characters of growth in primary culture were the media employed and in the case of serum media the quality of the medium.

When a piece of tissue was merely placed on the medium the bacilli multiplied on the tissue itself and the first evidence that multiplication had occurred was a slight rounding off of the minute ridges and projections and a generally somewhat moist and shiny appearance of the surface of the tissue. Later, minute rounded elevations appeared, and these often formed ultimately large heaped-up cream-coloured nodules or large "crusts."

On the other hand when the surface had been merely rubbed over with scrapings from the tuberculous tissue, or with tissue juices obtained by crushing the material between ground glass plates, or softened pultaceous matter had been sown in a thin layer, or an emulsion sprayed over the surface, the resulting growth had all the characters described later as occurring when subcultures were made on the same media; the only difference observed being that, as a rule, the growth in primary culture was scantier and made its appearance later than in subculture, consequently isolated discrete colonies which often attained considerable size were more commonly seen in the former than in the latter.

A difference between the growth on serum and on glycerin serum was often very well brought out in primary cultures. It was this, when the number of colonies was so small that each remained discrete from its neighbours the number developing on the glycerin serum was in comparable tubes (*i.e.*, tubes prepared from the same batch of serum and inoculated with similar material) often much smaller than on serum and the individual colonies much larger and whiter. This, however, in the case of primary cultures, was found to be a rule which has many exceptions.

In the few cases in which glucose serum has been used it has been noticed, on the other hand, that the number of colonies was very much greater and individuals correspondingly smaller than on serum. Hence, it sometimes happened that a thin ground glass layer formed on the glucose serum while on serum the colonies were discrete but fairly large in number and small in size.

The length of time which elapsed after the tissue had been sown and before growth became apparent to the naked eye varied within wide limits. Sometimes as early as the seventh or eighth day after sowing, frequently at the end of 10 days and very rarely after 3 or 4 days some evidence of growth was plainly visible. In a few cases it has been three or four weeks before the growth had progressed sufficiently far to be visible to the naked eye. In the majority of cases, however, growth made its appearance between the end of the first and the end of the second week.

When pieces of tissue had been sown, whether crushed or not, an early growth not infrequently escaped notice when examined with the naked eye until some experience had been gained in examining primary cultures microscopically. If a microscopical examination be made it could generally be determined at a much earlier period than was possible with the naked eye whether or no growth was taking place. Thus, it was quite usual to find on examining a cover glass preparation that considerable multiplication had occurred even after four days, and by making subcultures at this early period growth was frequently much accelerated. The microscopic appearance which was taken as evidence of growth was the presence of bacilli in clumps instead of occurring singly or in pairs or in relatively quite small clumps as they usually do in tissues.

In some cases with any of the media employed, and particularly in the case of horse serum, the appearance of visible growth was delayed until the third and fourth week. Meanwhile the surface of the medium became covered with a scum. As growth became apparent minute circular areas were seen from which the scum had disappeared. In the centre of each of these areas a minute, practically colourless, speck was found to be present; this was a centre of commencing growth which progressed and assumed the ordinary characters of growth on that medium.

Microscopically the bacilli obtained from primary cultures on serum and glycerine serum were very noticeably shorter and more uniformly stained than those in the animal tissues with which the tubes were inoculated.

D. *Contaminations.*

When sowing primary cultures it was found advisable to inoculate several tubes, and these from different organs or glands, in order to minimise the risk of failing to obtaining a culture. As experience was gained in technique the number of contaminated tubes was considerably reduced, but however much care was bestowed in sowing primary cultures from even recently killed animals my

experience showed that sometimes contaminating organisms were occasionally present in the animal tissue: *e.g.*, culture tubes sown with material from the bovine lung have repeatedly grown organisms other than tubercle; on the other hand the lung of the rabbit has proved one of the most satisfactory tissues with which to sow cultures.

Contaminations generally appeared within forty-eight hours, and were, as a rule, sufficiently obvious; if, however, there was some doubt as to the purity of a culture the condition of the fluid at the bottom of the tube will often assist in settling the difficulty; if the fluid be milky the tube is impure.

But while the majority of contaminations were apparent within forty-eight hours, organisms were sometimes present which were as slow or even slower in producing a recognisable growth than tubercle, and in such a case it has happened that the growth had all the appearances of tubercle, the fluid at the bottom of the tube was clear and every indication of contamination was absent. The only certain method therefore of determining whether a culture was pure or not was to examine it microscopically.

When dealing with the original human material contaminating organisms have occasionally been present which were of such a nature as to be unable to grow under the ordinary conditions of tubercle cultivation, and consequently pure cultures of tubercle bacilli were sometimes raised from material which microscopically was very unpromising.

Pure cultures of tubercle bacilli have however occasionally been recovered from contaminated tubes. The ordinary method of obtaining pure culture by the method of dilution not being applicable, the best chance was found to lie in picking off a trace of growth from various parts of the serum, examining under the microscope, and, if no foreign organisms were present, to sow that particular piece on to a serum tube. When the colonies were discrete then of course recovery was more easily effected.

Subcultures.

When subcultures were made merely for the purpose of perpetuating any strain of bacilli, it was found to be desirable that they should be made from serum on to serum and at intervals of a month to six weeks. The importance of making subcultures from serum on to serum lies in the fact that, as these experiments proved, whatever medium was used the bacilli slowly underwent change in their cultural characters, and this change has been found to be least marked, and to occur most gradually, when serum is used. It has often happened that subcultures have been made from cultures which were much older than six weeks, but experience showed that a risk was then incurred that the bacilli might have died, and this especially if the serum upon which they were last sown had been a poor one.

But when the object of making subcultures was to determine the cultural characters of a particular strain then various other media were used, and those relied upon for this purpose have been:—

Serum.
Glycerin serum.
Glycerin agar.
Glycerin broth.

And as the result of my experience I have come to the conclusion that in order to get reliable results with these "differentiating media" it is absolutely necessary that they should be sown from young vigorous cultures, seven to fourteen days old, growing on a good serum, and as soon after isolation from the animal tissues as possible. And the method I came to adopt as the most desirable when attempting to differentiate strains was to sow three or four serum tubes from the primary culture on serum, and from these to inoculate two or three tubes of serum, glycerin serum, and glycerin agar—if only one tube of each was sown it was found that error was very liable to creep in and give a false impression.

General Statement of the Appearance of Growth of Tubercle Bacilli on each of the four Media—Serum, Glycerin Serum, Glycerin Agar, and Glycerin Broth.

Even with the same strain of bacilli results were very variable, and depended, in addition to the readiness or reluctance of the particular strain under investigation to grow on artificial media, upon the age of the culture from which subcultures were made, the quality of the medium upon which the bacillus was growing, and of that upon which it was sown, the amount of material sown, and the length of time it had been under artificial cultivation. Reliable comparisons, therefore, between different strains were only possible when these disturbing factors had, as far as possible, been excluded.

Serum.—When a good serum has been used as the culture medium no difficulty has been experienced in obtaining a growth with any of the strains of bacilli examined; but at the same time differences in the rate of growth and in the degree of luxuriance ultimately attained have been constantly observed. Further, it has been found that those strains which in general grew with the greatest luxuriance and most rapidly on serum were those which grew also most readily and rapidly and with the greatest luxuriance on other media; and conversely, those whose yield on serum was sparse were those which grew sparingly and slowly and often with difficulty on other media. But though this has been found to be true as a general observation, the differences in the growth on serum were not so constant nor so well marked as the differences found to exist in the growth on glycerin serum, glycerin agar and glycerin broth.

When every precaution had been taken to eliminate as far as possible every disturbing factor it was found that while in some cases growth took place relatively somewhat slowly and the surface became covered eventually with a thin, colourless, almost invisible, dull, uniform, ground-glass layer, in other cases multiplication took place distinctly more rapidly, so that within a week the surface of the medium was covered with a thick layer of growth generally more or less pigmented (if the medium were pigmented) with rounded hemispherical elevations projecting from the general surface of the growth. In these cases, after further incubation the layer of growth had thickened considerably, become more pigmented, the elevations had increased in size and number, and the growth had a tendency, often considerable, to extend on to the glass of the tube, and occasionally a certain degree of wrinkling of the layer was noticed.

These were the two extreme types of growth, but were not by any means the only ones obtained. Every possible intermediate type was met with, filling up the gap between those described, that is, between the least luxuriant, and the most luxuriant. And in all cases the differences were differences in degree only.

When the growth from a serum tube, which often sticks very tightly, was washed off or gently lifted off with a platinum spatula it was found that the surface of the serum was dotted with innumerable minute holes representing points where the bacillus in its growth had actually extended into the surface layer of the medium.

Glycerin Serum.—The appearances resulting from growth on this medium were essentially similar to those already described as seen on serum. The differences in the rate of growth and in the degree of luxuriance attained were, however, more marked than on serum, for while no difficulty was experienced in obtaining a growth on serum with any of the strains, some, those, namely, which exhibited the least luxuriance on serum, grew on glycerin serum only with difficulty and after a delay which was often considerable. On the other hand, those bacilli which grew the most readily and most luxuriantly on serum produced in the same space of time a growth which was in every respect equal to that obtained on serum, and in some cases, especially when the serum was not of the best, the growth on glycerin serum excelled that on serum.

In many cases the growth on glycerin serum was slower in making its appearance than on serum, and in practically every case the growth is whiter on the former than on the latter (when a pigmented serum was used). The layer of growth, too, was frequently more wrinkled in the case of the more luxuriantly growing bacilli when grown on glycerin serum than when grown on serum without the addition of glycerin.

On removing the layer of growth the surface of the medium was frequently seen to be pitted as in the case of serum.

On Glycerin Agar.—In some cases no growth at all has occurred for some weeks, and when the maximum has been finally attained it is very scanty. In such cases the surface did not become uniformly covered, but growth consisted merely of isolated colonies often very few in number, sometimes large, sometimes small, but in either case generally

surrounded by a spreading, often delicately wrinkled, almost colourless halo. These colonies often became considerably heaped up, ridged on the surface and hollowed at the base. If left for three or four months the growth occasionally took a rosette form.

Some strains developed after some delay a very thin, almost colourless layer of growth which, at first uniform, later became irregularly and sparsely studded with small cream-coloured hemispherical nodules.

With other strains again growth commenced without delay, progressed rapidly, and the ultimate yield was considerable, consisting of a thick cream-coloured generally wrinkled layer which was thickly studded with projections from the general surface, these projections resembling "miniature mountain ranges." Moreover, these strains of bacilli had a great tendency to grow beyond the surface of the agar on to the glass sides of the tube.

Intermediate appearances of almost infinite variety have been obtained on this medium.

On Glycerin Broth.—Various methods have been adopted for sowing broth cultures. In most cases the film on the fluid at the bottom of a serum tube has been lifted off with a many-looped platinum wire and floated on to broth. The best results were obtained by transferring this film as soon as it had formed, and in some cases, in order to increase its extent before transferring to broth, a few drops of broth have been added to the fluid already in the serum tube, and the culture replaced in the incubator for a week or so.

In a few cases the film so transferred has at once begun to extend, and has covered the surface of a 6-oz. bottle, laid on its broader face, in two or three weeks or even earlier. But in a majority of cases no extension, or at most a very slight extension, of the film has occurred for some time, perhaps three or four weeks. During this period the pellicle, if the bacilli were alive, thickened and developed very small, white, scattered hemispherical points, and sometimes downgrowths into the depths of the fluid took place. But in these cases once the film had started to extend its area growth over the surface of the liquid medium was generally very rapid, so that the surface was covered not infrequently in so short a space of time as fourteen days. In some cases it has been three or even four months after transferring the film before any increase in area has been observed. It has, however, been frequently noticed that growth ceased before the whole surface was covered, and this has been the case with all strains, whatever their origin.

The resulting pellicle was in some cases very thin and delicate, practically colourless, flat, uniform, and readily broken up into small islands, with but small tendency to climb the sides of the bottle and floating with difficulty.

In other cases the film was somewhat thicker, greyish-white in colour, and generally reticulated.

With many strains of bacilli the pellicle was relatively thick, cream-coloured and opaque, floating much more readily than the thinner films, often thrown into delicate folds and showing a great tendency to climb the sides of the bottles, sometimes to such an extent that the roof was half occupied by the extension of the film. In such cultures small white nodular projections were often seen.

Downgrowths from the under surface of the original material sown occurred indiscriminately, whatever the nature of the resulting pellicle. These stalactites were particularly well marked when there had been much delay before growth began to extend laterally.

The growth on broth has been found to be very capricious, and with the same strain broth has given more variable results than any of the other media described.

Comparison of Values of each of these Media for Comparative Purposes.

While the various strains of tubercle bacilli investigated grew on serum with varying degrees of luxuriance, the medium was found to be of relatively small value for comparative purposes on account of the slight differences which have been found to exist between the most luxuriantly growing strains and those which grow most scantily.

But the differences observed between the various strains on glycerin serum, glycerin agar, and glycerin broth, have been in each case much more marked than on serum.

Comparison of Growths on Serum and on Glycerin Serum.

In these comparisons the same batch of serum has been used in the whole serum and in the glycerin serum tubes.

In comparing the growths produced by the various strains on these two media, it has been found that when a strain was such as to grow only poorly on serum, then its growth on glycerin serum would also be scanty, and in some cases did not occur at all in early sub-culture, and on the other hand, a bacillus which grew luxuriantly on serum grew luxuriantly also on glycerin serum. It has further been observed that in the case of the more luxuriantly growing bacilli, when a poor sample of serum was used, the growth on glycerin serum exceeded in amount the growth on serum, making it appear as though glycerin acted as a substitute for, or supplemented, certain nutritive substances which are absent from or deficient in a poor serum.

With the best samples of serum this difference in the relative amounts of growth of serum and glycerin serum did not occur.

Differentiation of Strains and Viruses.

The characters of growth of tubercle bacilli on serum, glycerin serum, glycerin agar, and glycerin broth have already been considered. Very great difficulty has been experienced in obtaining constant results as regards these characters, but by multiplying, repeating, and controlling the experiments, and by taking every possible care to eliminate as far as possible any disturbance due to factors under control, it has been found that well-marked differences exist between the various strains of different viruses and in a few cases between the different strains of the same viruses.

Some strains of bacilli when sown in primary culture have grown readily on both serum and glycerin serum, and in a relatively short space of time have produced abundant growths on both these media. And in some cases it has been noticed that the amount of growth ultimately resulting on the glycerin serum medium is even more luxuriant than that on the serum medium. Such strains in sub-culture have grown easily, readily, and luxuriantly on serum, glycerin serum, glycerin agar, and glycerin broth. That is to say, that when sown on these media from an early and young subculture growth commenced early, proceeded rapidly, attained its maximum soon and was finally very copious.

Other strains grew well on serum and on glycerin serum both in primary and in subculture, but not quite so rapidly nor so luxuriantly as those already described; and, further, it has been found that these strains when subcultured on to glycerin agar and glycerin broth only commenced to grow after some delay, and it has not infrequently happened that growth was only obtainable on these two last-named media when the strain had been in culture for some weeks, and had passed through several subcultures on serum or glycerin serum. Even when growth was finally obtained, it did not attain the luxuriance of those strains above described.

In the case of some other bacilli, growth has very rarely indeed been obtained on glycerin serum in primary culture, and then traces only, and that after a long period of delay. Such bacilli have been found to grow far better on serum than on any of the other three media under consideration. But even on serum the growth was relatively slow and continued for a long time, while the amount ultimately obtained was small. On glycerin serum it frequently happened that no growth was obtained in the first two or three sub-cultures, and even when it did occur was scanty and very slow in appearing. On glycerin agar it was very rarely indeed that the smallest trace of growth was obtained until these strains had been in culture for some months, and even then it was generally many weeks at the earliest after sowing on the medium before indications of multiplication were visible, and, further, with these bacilli, growth was, as a rule, limited to one or two hemispherical colonies, or a few very small colourless points of growth. On glycerin broth growth was as difficult to obtain as on glycerin agar. When first isolated from the living tissues, bacilli of this character have in my experience consistently failed to grow on glycerin broth, and it was only after repeated sub-culture on serum that a film was ultimately obtained, and even then the film was very thin and delicate.

My experiments have shown that after long-continued cultivation outside the body the growth characters undergo a slow but quite definite change. The longer a strain of bacilli has been kept in artificial culture the more readily and luxuriantly has it grown, and strains which when first isolated were difficult to grow, and on certain media did not grow at all, became so changed as to grow comparatively easily and well on any of the media mentioned.

Conclusions.

From the examination of a very large number of strains of tubercle bacilli derived from many human sources which is summarised in the foregoing report, the following results have been obtained;—

1. In staining properties and microscopic appearances no essential differences have been found in culture between the various strains examined.
2. In cultural characters the only differences which were made out were differences in amount of growth and in time of growth. In all other respects all the strains examined agreed in their cultural characters.
3. The relative capacity of the several strains to grow in artificial culture when first isolated from the tissues of the body varied within limits, for while some grew relatively slowly and scantily, and on some media with great reluctance, others there were which grew more rapidly, more luxuriantly, and with greater readiness, and others, again, with even greater relative rapidity, luxuriance, and ease.
4. The relative capacity to grow outside the body was, however, not constant, for, on the one hand, when any strain of bacilli was kept in artificial culture for some time it was relatively more eugonic than it was at first, and this was equally true of all strains, whatever their initial gonic power. It has also been noticed that bacilli after passage through a rabbit or a goat are slightly more eugonic than after passage through a bovine animal or a guinea-pig. And, on the other hand, in three cases a eugonic bacillus after passage through a series of bovine animals has been converted into a dysgonic bacillus, and cultures obtained from intermediate animals of the passage series was less eugonic than the former and more eugonic than the latter. It has also been found possible to alter gonic power by altering the character of the culture medium. All strains of bacilli grew more rapidly, luxuriantly, and easily on a rich than on a poor serum, and on one that was moist than on one which was dry. Again, for instance, in the case of broth cultures, concentration, dilution, or addition of acid all changed the relative eugony or dysgony of any strain of bacilli.
5. The cultural characters of many of the strains which I have recorded in this report corresponded in every respect to the German "Typus humanus"; others, again, were identical in every detail with their so-called "Typus bovinus." Filling up the gap between the typical typus humanus and the typical typus bovinus a large number of strains of bacilli have come under my observation, which exhibit characters intermediate between the two.

Table showing the different viruses arranged according to the source of the human material and the number of strains of each virus examined.

			No. of strains examined.
Axillary glands:—			
	H 33.	R.T.	1
Bone and gland from a case of multiple osteitis:—			
	H 15.	I.W.	2
Bronchial glands:—			
	H 13.	A.D.	3
	H 21.	G.B.	2
	H 32.	Y.W.	1
	H 35.	C.B.	1
	H 45.	F.M.	1
	H 22.	F.W.	1
Cervical glands:—			
	H 28.	C.L.	8
	H 29.	M.F.	5
	H 31.	L.F.	1
	H 38.	J.M.	1
	H 39.	M.B.	1
	H 44.	D.C.	1
	H 27.	B.D.	1
	H 34.	C.U.	1
Joints:—			
	H 16.	J.H. *knee*	10
	H 9.	C.T. *wrist*	3
	H 43.	F.F. *knee*	1
	H 41.	A.S. „	2
	H 46.	H.W. „	1
	H 47.	S.B. *hip*	1
	H 42.	M.R. *ankle*	1
	H 11.	E.D. *elbow*	2
Kidney:—			
	H 26.	K.M.	2
Lung:—			
	H 36.	M.D.	1
	H 23.	J.P.	4
	H 50.	P.H.	1
	H 48.	W.P.	2
	H 25.	A.T.	2
	H 22.	F.W.	3
Lupus Vulgaris:—			
	H 53.	D.H.	1
Mesenteric glands:—			
	H 10.	B.S.	10
	H 14.	F.S.	11
	H 7.	C.M.	4
	H 20.	F.L.	6
	H 32.	Y.W.	3
	H 19.	S.W.	4
	H 49.	T.C.	1
	H 18.	T.T.	4
	H 8.	S.C.	5
	H 36.	M.D.	1
	H 12.	H.N.	5
Mesenteric and bronchial glands:—			
	H 30.	E.M.	2
Sputum:—			
	H 17.	Sp. B.	7
	H 2.	Sp. A.	9
Testis:—			
	H 40.	J.G.	1

* Cultures originally isolated by Dr. Cobbett.

† Cultures originally isolated in conjunction with Dr. Cobbett.

H 2. Sp. A.

Source of the Human Material.—Mixed Sputum.

The cultures examined were obtained from:—

* G.P. 340 inoculated with an emulsion of the Mesenteric and Hepatic Glands of Heifer 11 - 2.iv.03
* G.P. 434 „ „ „ various Glands of Calf 79 - - - - - - 6.iv.03
* G.P. 534 „ „ „ the Posterior Mediastinal Gland of Calf 93 - - 14.v.03
* G.P. 618 „ „ „ „ Prepectoral Gland of Calf 83 - - - - 12.viii.03
* The Local Lesion of Calf 163 - - - - - - - - - - - - 14.viii.03
* G.P. 651 inoculated with emulsion of tissues of Calf 91 - - - - - - - - 24.viii.03
† G.P. 690 „ „ „ „ Calf 111 - - - - - - - - 9.x.03
† G.P. 664 „ „ „ „ Calf 153 - - - - - - - - 10.x.03
† G.P. 777 „ „ „ „ Pig 3 - - - - - - - - 12.x.03

Characters of growth in Sub-culture.

In all cases the same, viz.:—

Source.	Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
All H 2. Sp. A. - - -	Very good	Very good	Fairly easily growing and moderate yield	Fairly easily obtained and moderately thick.

H 7. C.M.

Mesenteric Glands of a child aged 3 years who died of tuberculous meningitis.

Source of the Cultures.	Sown.
*G.P. 448 from an emulsion of Calf 5 - - - - -	1.iv.03.
*G.P. 552 from supra-mammary gland of Cow 73 - - -	12.vi.03.
*G.P. 646 from anterior mediastinal gland of Calf 103 - -	9.x.03.
*G.P. 708 from prescapular gland of Calf 115 - - -	12.x.03.

Characters of growth in Sub-culture.

In all cases the same, viz.:

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Good; fairly rapid.	As on serum.	Poor: much delayed and increase slow.	Moderate yield; growth obtained fairly easily after being in culture some time.

H 8. S.C.

Mesenteric Glands of a child aged 11 months.

Source of Cultures.	Sown.
The Human Material—	
Calf 305 - - - - - - - - - - -	16.i.04
Prescapular Gland of Calf 303 - - - - -	13.iv.04
Thoracic Gland of Calf 361 - - - - - -	30.v.04
G.P. 1230 from Local Lesion of Calf 275 - - -	28.vi.04

Characters of Growth in Primary Culture.

On serum—luxuriant.
On glycerin serum—luxuriant.

H 8. S.C.—*contd.*

Characters of growth in Sub-cultures.

In all cases the same, viz:—

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Very good.	Very good: thick, white and opaque.

H 9. C.T.

Material removed by operation from a tuberculous wrist of a Man aged 55 years.

Source of Cultures.	Sown.
G. P. 981 from Calf 183 (prescapular gland) - - - - -	12.i.04.
G. P. 996 from Calf 183 (prescapular gland) - - - - -	9.ii.04.
G. P. 1081 from Calf 201 (prescapular and prepectoral glands) -	19.v.04.

Characters of growth in Primary Culture.

Source.	Serum.	Glycerin Serum.	Dextrose Serum.
Calf 183 G. P. 981	Good.	Very good.	—
Calf 183 G. P. 996	Good.	—	—
Calf 201 G. P. 1081.	Good.	Very good.	Good.

H 10. B.S.

Mesenteric Glands from a child $1\frac{9}{12}$ *years.*

Source of Cultures.	Sown.
†G.P. 725 from Calf 35 (prescapular gland) - - - -	8.x.03.
†G.P. 1, from G.P. 771, 744, from G.P. 661, from G.P. 593, from G.P. 386, from original material - - - - -	9.x.03.
G.P. 757 from Calf 113 (prescapular gland) - - - -	31.x.03.
G.P. 798 from Goat 3 (prescapular gland) - - - -	9.xi.03.
G.P. 791 from Calf 191 (prescapular gland) - - - -	11.xi.03.
Heifer 231 (prescapular gland) - - - - - -	16.xii.03.
G.P. 952 from Cow 295 (milk) - - - - - -	21.iii.04.
Monkey 59 (cervical gland) - - - - - - -	16.vii.04.
Calf 387 (liver) - - - - - - - - -	27.vii.04.
Heifer 249 (prepectoral gland) - - - - - -	16.viii.04.

Characters of growth in Primary Culture.

On serum.—The number of colonies developing was generally small and individuals remained discrete. They were nearly colourless, minute and hardly raised above the surface.

On glycerin serum.—Rarely any growth and then only after a long lapse of time, and the yield ultimately very poor.

H 10. B.S.—*contd.*

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Scanty, thin, colourless, and relatively slow-growing.	Very poor, growth much delayed and often not obtained in early sub-cultures. Multiplication much slower than on serum.	Cultures only obtained after being in artificial culture some time. Very slow-growing and ultimate yield scanty.	Not obtained until after being in culture some time. Film very thin, uniform and colourless. * See footnote.

The cultures obtained from the goat grew rather more easily and somewhat more copiously than the other cultures.

* Bacilli obtained from Heifer 231 after being in artificial culture for 15 months covered the surface of glycerin broth contained in a Winchester quart and laid on its side in 14 days.

H 11. E.D.

Synovial Membrane from the elbow joint of a female aged 10 years, removed by operation.

Source of the Cultures.	Sown.
G.P. 918 from prescapular gland of Calf 221	23.xii.03
G.P. 1298 from portal lymphatic gland of Calf 309	23.vii.04.

Characters of growth in Primary Culture.

On serum - - - - - - Good

On glycerin serum - - - Fair in 33 days

Characters of growth in Sub-culture.

In both cases the same, viz. :—

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Copious.	Copious.	Copious.	Thick, white, easily grown and much folded.

H 12. H.N.

Messenteric Glands from a male aged 15 years.

Source of Cultures.	Sown.
*G.P. 577 from original material	21.vi.03
*Calf 123	27.viii.03
G.P. 988 from G.P. 928–930 (prescapular and prepectoral gland of Calf 189)	13.i.04
The thoracic gland of Calf 319	10.iii.04
Calf 363	22.vi.04

Characters of growth in Primary Cultures.

Source.	Serum.	Glycerin Serum.	Serum Agar.
Calf 123 G.P. 988	—	—	Luxuriant.
Calf 319	Luxuriant.	—	—
Calf 363	Very good.	Very good.	—

Characters of growth in Sub-culture.

In all cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Luxuriant.	Luxuriant.	Good, sometimes wrinkled.	Fairly thick, opaque film.

H 13. A.D.

Bronchial Glands and Spleen of a child aged 4 years. Disease probably primary in Bronchial Glands.

Source of the Cultures.	Sown.
Calf 301 (Thoracic Gland)	20.i.04
Prepectoral Gland of Calf 321	23.iii.04.
Spleen of Rat 15 from Thoracic Gland of Calf 301	5.vii.04.

Characters of growth in Primary Culture.

Calf 301. On serum, good in 19 days.
„ 321. On serum, good.
Rat 15. On serum, fair.
On glycerin-serum, nil.

Characters of growth in Sub-culture.

These exhibited differences, viz. :—

Source.	Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Calf 301	Very good.	Very good.	Very good ; generally wrinkled.	Thick white film, often thrown into folds.
Calf 321.	Good : fairly rapid.	As on serum.	Very poor, much delayed and grows slowly.	Thin, colourless and transparent, and obtained with some difficulty.
Rat 15.	Scanty and relatively slow growing.	Very poor : growth delayed and increase slow.	Very scanty: a delay of several weeks occurs before growth begins and then proceeds slowly.	Scanty, very thin, transparent and delicate.

H 14. F.S.

Mesenteric Glands from a Child aged 4½ years.

Source of Cultures.	Sown.
Prescapular gland of Calf 125	1.vii.03.
G.P. 587 from original material	24.vii.03.
G.P. 769 from Heifer 197 (prescapular gland)	15.x.03.
Mesenteric gland of Calf 181	24.x.03.
G.P. 894 from Calf 205 (prescapular gland)	19.xii.03.
G.P. 838 from Heifer 243 (prescapular gland)	21.xii.03.
G.P. 1103 from Cow 235 (uterus)	18.iii.04
G.P. 1108 from Calf 327 (hepatic gland)	22.iii.04.
G.P. 1104 from Cow 235 (uterus)	2.iv.04.
G.P. 1109 from Calf 327 (hepatic gland)	12.iv.04.
G.P. 1106 from Calf 327 (liver)	30.v.04.

H 14. F.S.—*contd.*

Characters of growth in Primary Culture.

Calf 205 G.P. 894 Heifer 243 G.P. 838 Cow 235 G.P. 1103	On serum. Thin, colourless and slow growing.
Calf 327 G.P. 1108 Cow 235 G.P. 1104 Calf 327 G.P. 1109 Calf 327 G.P. 1106	On glycerin serum. In two cases, one or two very small colourless colonies developed after the lapse of a long period of time.

Characters of growth in Sub-cultures.

In all cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Thin, colourless, scanty and relatively slow growing.	Often not occurring in early sub-cultures when obtained only after some lapse of time. Ultimate yield very poor.	Growth very long delayed and very sparse. Only obtained after being in culture some time.	Growth only occurred after being in artificial culture some time and the film then obtained was very thin and colourless.

H 15. I.W.

Fragments of Bone and an Enlarged Gland removed by operation from a case of Multiple Osteitis occurring in a child aged 4 years.

Source of Cultures.	Sown.
(2) Prescapular and prepectoral glands of Calf 413 -	4.viii.04.
(1) Calf 311 (prescapular gland) - - - - -	9.v.04.

Characters of growth in Primary Culture.

On serum a luxuriant growth.

Characters of growth in Sub-culture.

In both cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Very good and rapidly growing. No delay in appearance of growth.	Easily obtained. Extension rapid and film of moderate thickness.

H 16. J.H.

Synovial Membrane from the knee joint of a man aged 38 years removed by operation.

Source of the Cultures.	Sown.
*G.P. 609 from human material - - - - - -	12.viii.03
*Calf 157 - - - - - - - - - - - -	24.viii.03
G.P. 872 from local lesion of Calf 187 - - - - -	23.i.04
Prescapular gland of Calf 273 - - - - - - - -	10.ii.04
Local lesion of Calf 355 - - - - - - - - -	24.iii.04
Calf 337 - - - - - - - - - - - -	30.iv.04
G.P. 1141 from prescapular gland of Calf 355 - - - - -	19.v.04
Calf 317 - - - - - - - - - - - - -	13.vi.04
Prepectoral gland of Bull 423A - - - - - - -	30.vii.04
Bronchial gland of Calf 559 - - - - - - - -	12.xii.04

H 16. J.H.—*contd.*

Characters of growth in Primary Culture.

Source.	Serum.	Glycerin Serum.	Dextrose Serum.
Calf 187 G.P. 872.	Good.	Some growth.	—
„ 273.	Fair growth.	Very good.	–
„ 355	Good.	Good.	
„ 337.	Very good.	Nil.	Good.
„ 355 G.P. 1141.	Very good.	Some growth.	
„ 317.	—	—	Good.
Bull 423A.	Good in 18 days.	—	—
Calf 559.	Very good.	—	

Characters of growth in Sub-culture.

These exhibited differences which are set out below.

Source.	Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
G.P. 609 Calf 157 Calf 187, G.P. 872 Calf 273	Very luxuriant.	Very luxuriant.	Very good; thick, nodular, and wrinkled.	Very good, thick, opaque and folded.
Calf 355 Calf 337 Calf 355, G.P. 1141 Calf 317	Good, fairly rapid.	Good.	Poor, delayed and increase slow.	Moderate yield obtained with some difficulty.
Bull 423A Calf 559	Scanty, relatively slow-growing.	Scanty and slow-growing. Appearance of growth generally much delayed in early sub-culture.	Very scanty and very slow-growing. Growth not obtained in early sub-cultures, and when obtained only after a delay of some weeks.	Very scanty, thin and colourless. Growth obtained only after being in culture some little time.

H 17. Sp. B.

Mixed Sputum.

Source of the Cultures.	Sown.
Prescapular gland of Calf 265	11.ii.04
„ „ Calf 391	25.v.04
G.P. 83 from Calf 277 (prescapular gland)	27.iv.04
G.P. 235 from Calf 339 (prescapular and prepectoral glands)	8.vii.04
Prepectoral gland of Calf 555	19.i.05
Thoracic gland of Calf 555	19.i.05
Long thoracic gland of Calf 531	20.i.05

H 17. Sp. B.—*contd.*

Characters of growth in Primary Culture.

Source.	Serum.	Glycerin Serum.
Calf 265 - - - -	—	Luxuriant. Large varied white hemispherical colonies.
„ 391 - - - -	Copious.	Copious.
„ 277 G.P. 81 - -	—	Good.
„ 339 G.P. 235 - -	Very scanty.	
„ 553 - - - -	Fair growth. 6 weeks.	Nil. 6 weeks.
„ 555 - - - -	Fair growth.	—
„ 531 - - - -	Scanty.	—

Characters of growth in Sub-culture.

These exhibited differences which are set out below :—

Source.	Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Calf 265 -	Very luxuriant.	Very luxuriant.	Very good.	Easily obtained, thick film and often folded.
„ 277 -				
„ 391 -				
„ 339 -				
Calf 555 -	Scanty and relatively slow growing.	Very scanty and slow-growing, appearance of growth delayed.	Very scanty and slow-growing. Delay of several weeks before growth commences.	Scanty and slow-growing. Not obtained in early subcultures.
„ 531 -				

H 18. T.T.

Mesenteric Gland from a child 2 $\frac{5}{12}$ years.

Source of Cultures.	Sown.
G.P. 999 from Calf 131 prescapular gland after passage through G.Ps.	1. ii. 04.
G.P. 1174 „ „ „ „ „ „ -	20. iv. 04.
G.P. 1173 „ „ „ „ „ „ -	27. iv. 04.
Calf 405 „ „ „ „ „ „ -	21. vi. 04.

Characters of growth in Primary Culture.

Source.	Serum.	Glycerin Serum.
Calf 131 G.P. 999 - - -	—	Good.
G.P. 174 - - -	Good.	—
G.P. 173 - - -	Good.	—
Calf 405 - - - - -	—	—

Characters of growth in Sub-culture.

In all cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Good and only slightly delayed.	Fairly thick film.

H 19. S.W.

Mesenteric Glands from a child 2½ years old.

Source of Cultures.	Sown.
Prescapular gland of Heifer 239	3.iii.04.
Thoracic gland of Goat 11	4.iii.04.
G.P. 1118 from Heifer 239 (milk)	22.iii.04.
G.P. 1119 „ „ „	23.iv.04.

Characters of growth in Primary Culture.

Source.	Serum.	Glycerin Serum.
Heifer 239	Very good. 21 days.	Good. 21 days.
Goat 11	Copious. 28 days	
G.P. 1118	Very good.	
G.P. 1119	Copious.	Copious.

Characters of growth in Sub-culture.

In all cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Good : fairly rapid growth.	Good : fairly rapid growth.	Much delayed : very slow growing, and ultimate yield very poor.	Difficult, and only obtained after being in culture some time. Film thick.

The cultures through the goat were rather more eugonic than those from the bovine or guinea-pig.

H 20. F.L.

Mesenteric Gland of a child aged 2 $\frac{10}{12}$ years.

Source of the Cultures.	Sown.
Prepectoral gland of Calf 213	6.iv.04.
Prescapular gland of Calf 253	11.vii.04.
Rabbit 110 from the prescapular gland of Calf 253.	23.ix.04.
Rabbit 112 „ „ „ „ „	22.ix.04.
Lung of Rabbit 111 from prescapular gland of Calf 253	2.viii.04.
G.P. 307 from cerebro-spinal fluid of Calf 403	9.viii.04.

Characters of growth in Primary Culture.

On serum a relatively good growth, though in some cases scanty.

On glycerin serum a good growth was sometimes obtained.

Characters of growth in Sub-culture.

In all cases the same, viz. :—

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Good.	Good.	Poor, generally delayed and increase slow.	Moderate yield, but obtained with great difficulty.

Note.—Cultures from the rabbits were relatively rather more eugonic than from the bovine.

H 21. G.B.

Bronchial Gland of an adolescent aged 16 years.

Source of the Cultures.	Sown.
*Thoracic Gland of Calf 447 - - - - - -	28.ix.04.
G.P. 417 from Calf 447 (Prescapular Gland) - -	23.xi.04.

Characters of growth in Primary Culture.

Source.	Serum.	Serum Agar.
Calf 447 - - - -	Scanty growth	—
Calf 447 G.P. 417 - -	Fair growth	Nil.

Characters of growth in Sub-culture.

Source.	Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Calf 447 - -	Good fairly rapid	About equal to that on Serum	Scanty and delayed	Difficult to grow ; thin film.
Calf 447 G.P. 417	Good	Good	Fair yield and grew with but little delay	Moderately thick film.

H 22. F.W.

Lung and Bronchial Gland from a female aged 16 years.

Source of Cultures.	Sown.
*From the bronchial gland—	
Prescapular gland of Calf 293 - - - - - -	1.iii.04.
†From the Lung—	
Spleen of Calf 399 - - - - - - - - -	7.vi.04.
G.P. 1200 from Calf 291 (prescapular gland) - - -	4.v.04.
Thoracic gland of Calf 457 - - - - - - -	3.viii.04.

Characters of growth in Primary Culture.

Source.	Serum.	Glycerin Serum.	Dextrose Serum.
Calf 293 - - - - -	Good.	Very good.	—
Calf 399 - - - - -	—	—	Good.
Calf 291 G.P. 200 - - -	Good.	—	—
Calf 457 - - - - -	Good.	—	Good.

Characters of growth in Sub-culture.

In all cases the same, viz. :—

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Copious.	Copious.	Copious, easily obtained, rapid growth. Wrinkled.	Easily obtained, rapid extension. Film ; thick, white, opaque, and generally thrown into folds.

H 23. J.P.

Lung from a man aged 41 years.

Source of Cultures.	Sown.
The prepectoral gland of Calf 365	19.v.04.
The prescapular gland of Calf 421	8.viii.04.
The prepectoral gland of Calf 411	9.viii.04.
The prepectoral gland of Calf 431	19.ix.04.

Characters of growth in Primary Culture.

Source.	Serum.	Glycerin Serum.	Dextrose Serum.
Calf 365	—	Good, 25 days.	Good, 25 days.
Calf 421	Poor, decayed.	—	—
Calf 411	Very good.	—	—
Calf 431	Good in 29 days.	—	—

Characters of growth in Sub-culture.
In all cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Very good, easy and rapid.	Film is thick and cream coloured. Growth easily obtained.

H 25. A.T.

Lung of a female aged 30 years.

Source of Cultures.	Sown.
G.P. 1282 from original material	1.vi.04.
Prescapular gland of Calf 417	5.viii.04.

Characters of growth in Primary Culture.

G.P. 1282. On serum - - Copious.
Calf 417. „ „ - - Moderate growth.

Characters of growth in Sub-culture.
In both cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very copious.	Very copious.	Copious.	Very easily obtained, rapid extension, thick and forced.

One of the most easily, luxuriantly and rapidly growing bacilli.

H 26. K.M.

Kidney removed by operation from a female 25 years old.

Source of Cultures.	Sown.
G.P. 1326 from original material	17.viii.04
The prepectoral gland of Calf 437	29.ix.04

H 26. K.M.—*contd.*

Characters of growth in Primary Culture.

On Serum - - Good in 22 and 34 days respectively.

Characters of growth in Sub-culture.

In both cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Good; delay very small.	Easily obtained; thick, white and nodular film.

H 27. B.D.

Cervical Glands of a child aged 11½ years removed by operation.

Source of the Culture.	Sown.
G.P. 1216 inoculated with human material - - - - -	2.vii.04

Characters of growth in Primary Culture.

On serum - - - Luxuriant in 27 days.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Luxuriant.	Luxuriant.	Luxuriant.	Very good; thick, white and wrinkled, and very easily grown.

Note.—One of the most easily and luxuriantly growing bacilli examined.

H 28. C.L.

Cervical Glands of a child aged $1\frac{2}{12}$ years removed by operation.

Source of Cultures.	Sown.
The human cervical glands - - - - - - -	12.v.04.
Rabbit 102 from G.P's. 271–272 inoculated with the human material - - - - - - - -	20.vii.04.
G.P. 341 from Calf 513 (prescapular gland) - - -	6.viii.04.
Prescapular gland of Calf 515 - - - - -	12.viii.04.
Rabbit 123 from Calf 515 - - - - - -	22.ix.04.
G.P. 400 from the milk of Cow 143 - - - -	18.x.04.
Kidney of Goat 23 - - - - - - - - -	16.xi.04.
Cervical glands of Monkey 63 - - - - -	12.xii.04.

Characters of growth in Primary Culture.

In all cases the same, viz. :—

On serum the growth ultimately obtained was relatively scanty, and as a rule somewhat slow in appearing.
On dextrose serum scanty and colourless, and slow growing.
On glycerin serum no growth was obtained.

H 28. C.L.—*contd.*

Characters of growth in Sub-culture.

In all cases the same, viz. : —

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Scanty and slow growing.	Scanty and slow growing. Appearance of growth much delayed in early sub-cultures.	Very scanty and very slow growing. Appearance of growth delayed for some weeks and often not obtained in early sub-cultures.	Scanty growth. Generally difficult to obtain in early sub-cultures.

The cultures from the goat and rabbits were somewhat more eugonic than those from other sources.

H 29. M.F.

Cervical Glands from a child aged 7½ years removed by operation.

Source of the Cultures.	Sown.
G.P. 1329 from the human material - - - - - -	30.vii.04.
G.P. 1334 „ „ „ „ - - - - - -	8.viii.04.
Thoracic gland of Calf 477 - - - - - - -	2.ix.04.
Rabbit 115 from G.Ps. 1328–1329 - - - - - -	22.ix.04.
G.P. 1462 from liver of Calf 663 born of the infected heifer 251 -	28.i.05.

Characters of growth in Primary Culture.

G.P. 1329. Colourless, scanty growth on dextrose serum, visible in 9 days.
— 1334. On serum, growth was visible in 9 days and ultimately became fairly copious.
Calf 447. On serum, a scanty growth in 2 months.
Rabbit 115. On serum, a good growth in 6 weeks.
G.P. 1462. Original cultures raised on serum.

Characters of growth in Sub-culture.

In all cases the same, viz. :—

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Good : fairly rapid.	As on serum.	Slow, delayed and poor.	Moderate yield, and obtained only with some difficulty.

H 30. E.M.

Mesenteric and Bronchial Glands from a child 3$\frac{10}{12}$ years.

Source of Cultures.	Sown.
G.P. 1338 from G.P.'s 284–285 (original material) - - - -	5.viii.04.
G.P. 1337 „ „ „ - - - -	8.viii.04.

Characters of growth in Primary Cultures.

Good in 12 days on serum.

Characters of growth in Sub-culture.

In both cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Luxuriant.	Luxuriant.	Luxuriant.	Very good, rapidly growing, thick, white and wrinkled.

One of the most luxuriantly and rapidly-growing bacilli examined.

H 31. L.F.

Cervical Glands of a child, aged 7 years, removed by operation.

Source of Culture.	Sown.
*Human cervical glands.	24.ix.04.

Character of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Good: fairly rapid.	As on serum.	Poor: growth delayed for some weeks.	Moderately thin, slow growing and capricious.

H 32. Y.W.

Lung, Bronchial Gland and Mesenteric Glands from a child aged $2\frac{9}{12}$ years.

Source of Cultures.	Sown.
1. Mesenteric gland:	
† The human mesenteric glands	1.x.04.
Prescapular gland of Goat 7	19.xi.04.
Thoracic gland of Calf 465	9.xi.04.
2. Bronchial gland:	
Rabbit 166 from the human material	6.i.05.

Characters of growth in Primary Culture.

Source.	Serum.	Serum Agar.	Glycerin Serum.
Original material	Scanty and colourless	—	—
Goat 7	Moderate growth	Very scanty and colourless	—
Calf 465	Very scanty and colourless	Nil 64 days	Scanty: nearly colourless, discrete colonies.
Rabbit 166	Moderate growth	—	—

Characters of growth in Sub-culture.

In all cases the same, viz. :—

Source.	Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
All "Wilson" Cultures	Good and fairly rapid.	Good and fairly rapid.	In early cultures no growth for nearly three months; ultimate yield poor.	Growth delayed, film thin.

Cultures from the goat and rabbit were slightly more eugonic than from the other sources.

H 33. R.T.

Axillary Gland from a child aged 7 years.

Source of Culture.	Sown.
The human axillary gland	17.viii.04.

H 33. R.T.—*contd.*

Characters of growth in Primary Culture.

Very good on serum and on serum agar.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Copious.	Copious.	Copious, thick and white.	Easily obtained and rapidly growing. Film thick.

H 34. C.U.

Cervical Glands. Details unknown.

Source of Culture.	Sown.
* G.P. 236 from original material - - - - -	4.i.03.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Luxuriant.	Luxuriant.	Very good.	Easily obtained, thick, and folded.

H 35. C.B.

Bronchial Gland of a child aged 5½ years.

Source of the Culture.	Sown.
* G.P. 571 inoculated with the human material - - - -	23.vi.03

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Fairly easily growing and moderate yield.	Fairly easily obtained, moderately thick opaque film.

H 36. M.D.

Lung and Mesenteric Glands from a child 4½ years old.

Source of the Culture.	Sown.
The human mesenteric glands - - - - - - - -	11.i.05
G.P. 1571 inoculated with the human lung - - - -	3.iii.05.

Characters of growth in Primary Culture.

Source.	Serum.	Glycerin Serum.
Mesenteric gland direct from the human material - -	Very scanty, 31 days	—
Lung through G.P. 1571 - - - - - - - -	Failed	Very copious, 21 days.

H 36. M.D.—*contd.*

Characters of growth in Sub-culture.

Cultures from the lung and from the mesenteric glands had similar characters, viz.:—

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Luxuriant	Luxuriant	Very good	Very good, thick, white, folded pellicle.

H 38. J.M.

Cervical Glands from a child aged 1 year and 3 months.

Source of Culture.	Sown.
G.P. 793 from original material - - -	9.xi.03.

Characters of growth in Primary Culture.

On Serum - - Good.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Good.	Good.	Fair. Much more copious after being in culture for 12 months.	Film thin but obtained fairly easily after being in culture many months.

H 39. M.B.

Cervical Glands of a child, aged 12 years, removed by operation.

Source of culture.	Date of sowing.
* The human material	22.x.04.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Good.	Good.	Good.	Easily obtained; fairly thick film.

H 40. J.G.

A Testis with Epididymis of a man aged 43 years.

Source of the Culture.	Sown.
*G.P. 1439 from human material - - - - - - - -	9.xii.04

Characters of growth in Primary Culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good	Very good	Good, but rather slow	Fairly thick, and obtained with little difficulty.

H 41. A.S.

Synovial Membrane from a knee-joint of a female aged 39 years.

Source of Culture.	Sown.
†G.P. 1435 from original material - - - - - - - -	7.i.05
†G.P. 1437 „ „ „ - - - - - - - -	18.xii.04

Characters of growth in Primary Culture.

G.P. 1435 on serum - - - Good
G.P. 1437 „ „ - - - Good

Characters of growth in Sub-culture.

In both cases the same :

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Copious.	Copious.	Very good, easy and rapid.	Easily obtained, rapid extension and film of moderate thickness.

H 42. M.R.

Scrapings from a Tuberculous ankle-joint from a female aged 64 years.

Source of Culture.	Sown.
† G.P. 447 from original material.	24.xii.04.

Characters of growth in Primary Culture.

On Serum - - - Good.
On Glycerin Serum - Good.

Characters of growth in Sub-Culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Copious.	Copious.	Copious, easy and rapid.	Growth easily obtained, rapid extension and thick film.

One of the most eugonic cultures in the laboratory.

H 43. F.F.

Synovial Membrane of a knee-joint from a child aged 7 years.

Source of the Culture.	Sown.
†G.P. 1425 inoculated with human material - - -	12.i.05.

Characters of growth in Primary Culture.

A poor growth on serum in 3 weeks.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Good.	Easily obtained, film of moderate thickness.

H 44. D.C.

Cervical Glands of a child aged 4 years removed by operation.

Source of the Culture.	Sown.
The human material - - - - - - - -	8.xi.04

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Very good. Well marked growth in 7 days.	Very good, film thick, fairly easily and rapidly growing.

H 45. F.M.

Bronchial Gland from a child aged 1 year.

Source of Culture.	Sown.
G.P. 1137 from original material - - - - - - -	10.v.04

Characters of growth in Primary Culture.

On Serum—Good in 12 days.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Luxuriant.	Luxuriant.	Luxuriant.	Luxuriant.

One of the most easily, rapidly, and luxuriantly-growing bacilli examined.

H 46. H.W.

Caseous Material from the Patella of a Tuberculous knee-joint.

Source of Culture.	Sown.
G.P. 1444 from human material.	12.i.05.

Character of growth in Primary Culture.

G.P. 1444 { On serum, poor. / On Glycerin serum, nil.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Copious.	Copious.	Growth occurred without any delay, was rapid and very good.	As in the case of agar, film thick.

H 47. S.B.

Pus and fragments of Bone from the Hip-joint of a child aged 6½ years.

Source of Culture.	Sown.
† G.P. 1430 inoculated with the human material - - - -	6.i.05.

Characters of growth in Primary Culture.

On Serum, thick opaque masses of growth in 14 days.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Luxuriant.	Luxuriant.	Very good. Well marked growth in 7 days.	Thick and easily grown : sometimes thrown into folds.

H 48. W.P.

Lung of a man aged 40 years.

Source of Culture.	Sown.
G.P. 1494 from original material - - - - -	10.iv.05.
Rabbit 301 „ „ „ - - - - -	10.iv.05.

Characters of growth in Primary Cultures.

G.P. 1494. On serum, good in 9 days.
Rabbit 301. „ „ copious growth in 22 days.

Characters of growth in Sub-culture.

In both cases the same.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very luxuriant.	Very luxuriant.	Very good, thick, wrinkled, white, with small nodules.	—

H 49. T.C.

Mesenteric Glands of a male aged 18 years.

Source of the Culture.	Sown.
The human mesenteric glands - - - - - - - .	6.iv.05.

Characters of growth in Primary Culture.

Sown on serum, but contaminated so that the characters of the primary growth were obscured. Microscopic evidence of growth at the end of four days.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Grows after some delay, and yields only a moderate growth.	Moderately thick film, fairly easily started, but capricious.

H 50. P.H.

Lung of a male aged 34 years.

Source of Culture.	Sown.
* G.P. 1490 from human material.	27.iii.05.

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very luxuriant.	Very luxuriant.	Very good; thick white, nodular and wrinkled.	—

H 53. D.H.

Scrapings of tissue removed by operation from a case of Lupus Vulgaris, occurring in a girl aged 14 years.

Source of Culture.	Sown.
* G.P. 1482 from human material.	8.iii.05

Characters of growth in Sub-culture.

Serum.	Glycerin Serum.	Glycerin Agar.	Glycerin Broth.
Very good.	Very good.	Good and obtained after but little delay.	—

July 18th, 1905.

H. J. Hutchens.

REPORT ON THE CHANGES IN REACTION PRODUCED IN BROTH BY HUMAN AND BOVINE TUBERCLE BACILLI.

Dr. A. STANLEY GRIFFITH.

ON THE CHANGES IN REACTION PRODUCED IN GLYCERIN BROTH BY HUMAN AND BOVINE TUBERCLE BACILLI.

INTRODUCTION.

In a paper published in 1903* Theobald Smith described a simple method of distinguishing the bovine from the human type of bacilli in broth cultures.

He found that if bovine bacilli are grown on the surface of 3 to 5 per cent. glycerin broth, the acidity of which to begin with is about 2 per cent. the broth becomes steadily less and less acid until finally, when the membrane is complete, an acidity or an alkalinity of about 0·1 to 0·2 per cent. is reached; with human bacilli on the other hand the acidity during the formation of the membrane diminishes at first, falling sometimes as low as 0·3 per cent., then, as the membrane enlarges and thickens, gradually rises again and may reach 2 per cent., but more commonly fluctuates between 0·5 and 1·5 per cent.

Smith claimed that the differences thus brought out established a difference in physiological properties.

This conclusion was based upon the results of the investigation of six strains of tubercle bacilli of human origin and six strains of tubercle bacilli of bovine type; four of the latter were obtained from the bovine, one from a horse and one from a cat; one of the strains of human origin was virulent for calves and rabbits.

Subsequently† three more cultures isolated from the human being were tested after this method; these cultures were found to pursue one of two courses; they either attained a greater than the initial acidity at the end of two to three months, after having first become less acid, or else (as occurred with Human XIII.) the reaction remained after some preliminary fluctuation at an intermediate level of acidity.

Bacilli having the character of human bacilli were found therefore to differ among themselves in the degree of acid production; this phenomenon was constant for the same culture.

In a more recent publication‡ the results of experiments with varying amounts of glycerin are given. With bovine tubercle bacilli the percentage of glycerin had no influence on the final reaction. With slightly virulent human bacilli (except Human XIII.) glycerin up to $\frac{3}{10}$ per cent. did not interfere with a final slightly alkaline reaction, but in broth containing 1 to 2 per cent. the reaction remained slightly acid; in broth containing 3 to 5 per cent. a considerable amount of acid accumulated after several months.

With the human culture mentioned above which produced only a moderate amount of acid, all the broths containing up to 1 per cent. became alkaline; the flask containing 2 per cent. approached and nearly attained the neutral level, but the 3 per cent. broth remained acid.

From these experiments Theobold Smith concluded that in order to bring out the differences at least 3 per cent. glycerin should be employed.

As the method, described in Theobald Smith's first communication on this subject, appeared to supply a valuable means of distinguishing bovine from human tubercle bacilli whenever isolated in pure culture, I was directed by the Commission to repeat Smith's experiments with the different strains of tubercle bacilli which we had isolated from the ox and the human being in order to ascertain whether the same differentiation into two types could be made.

The work in connection with the investigation has been carried out at the Royalcot Laboratory.

* Trans. Assoc. Amer. Phys., Vol. XVIII., 1903, and The Journ. of Med. Research, Vol. XIII., No. 3, 1905.

† Amer. Journ. of the Medical Sciences, Vol. CXXVIII., 1904.

‡ The Journ. of Med. Research, Vol. XIII., No. 4, 1905.

Technique.

The cultures have all been grown in half litre flasks, each containing 200 c.c. of broth.

The broth was prepared from beef in the usual way, and in most of the earlier tests was previously fermented for two days with bacillus coli before adding the peptone, salt and glycerin. The initial acidity of the broth has varied from 10 to 21. Glycerin in the majority of cases was added in the proportion of 2 per cent.; in a few cases flasks containing 2 per cent. and 5 per cent. glycerin have been inoculated at the same time. In all cases chemically pure glycerin has been used.

The samples of broth to be tested were withdrawn from the flasks either by means of a syphon fitted into the neck, or by means of a side tube issuing from the bottom of the flask; the flasks with syphons were put in the incubator in an inclined position on a wooden stand. The glass tube forming the syphon passed to the bottom of the broth through a solid rubber cork in which there was also a short length of glass tubing, plugged with wool, which acted as an air inlet.

Over the external extremity of the syphon tube, as well as over that of the side tube, a short length of semi-pressure tubing was placed. In the distal end of the rubber tube a short glass cannula was inserted; the latter passed through a rubber cork fitted into a small test tube; the rubber tubing was clamped with a spring clamp.

Before withdrawing the test sample sufficient broth was withdrawn to empty the syphon or side tube. After removing the sample the small glass cannula was dipped in 2 per cent. lysol to prevent organisms which might have settled thereon, when exposed to the air, from growing in the broth left behind in the tube external to the clamp.

The flasks with side tubes possess several advantages over those with syphons; they occupy less space in the incubator and are easier to inoculate; there is, besides, no disturbance of the pellicle as is sometimes the case with the syphon, which necessarily passes through the centre of the pellicle.

The method of titration was similar to that usually employed for determining the acidity of broth; 2 c.c. of the broth were diluted to 50 or 60 c.c. with distilled water, and after the addition of 0·5 c.c. of a 0·5 per cent. solution of phenol-phthalein in 50 per cent. alcohol, brought to ebullition; an $\frac{N}{10}$ solution of NaOH was then run in until the liquid had assumed a faint but definite pink colour throughout. In every instance two, and often three or more, separate estimations were made and the average taken.

An attempt was at first made to estimate the amount of growth by a gravimetric method; the broth was filtered through a weighed filter paper and the growth collected: the filter was dried in the incubator for three days and then weighed. This method was found to give unreliable results and was abandoned for the volumetric method; this was carried out in the following way; the growth was collected in a graduated test tube and, after finely subdividing the bacillary masses (with a glass rod), centrifugalised for a definite period; then the level of the column of bacilli was immediately read off.

Material Used.

The material for inoculating the flasks has in most cases been taken from broth cultures raised directly from serum; in a few cases the bacillus has grown for some generations on broth before transference to the flasks, in others the pellicle which forms on the surface of the condensation water in a serum tube has been used.

I wish here to acknowledge my great indebtedness to Dr. Eastwood for allowing me the use of his broth cultures, and for having had in many cases broth cultures specially raised for me; in this way the labour of the investigation has been considerably lessened, and I have had the advantage of always using young vigorous cultures.

DISCUSSION AND INTERPRETATION OF EXPERIMENTS.

The Characteristic Reactions of Bovine and Human Cultures.

The results of the experiments are exhibited in tabular form. Considering first the results obtained with the bovine tubercle bacillus, an inspection of the table shows that all the strains have caused a diminution in the acidity of the broth. In four instances the broth became neutral or nearly neutral to phenol-phthalein. In the others the final reaction varied from +5 to +11·5. The total reduction in acidity, *i.e.*, the difference between the initial and the final reaction has varied from 1·5 to 12·5, None of the broths which began with an initial acidity of about + 20 became neutral to phenol-phthalein, the final reaction in these cases never attaining a lower level than +8·5. In several cases the final reaction of the broth was higher than the lowest recorded reaction ; this return to a higher acidity was in no instance great and did not exceed + 2·5.

The virulent strains of human origin have with one exception also caused a diminution in the acidity of the broth. The greatest diminution occurred in the case of H. 28 "C.L." which yielded the least amount of growth. None of the cultures brought the reaction of the broth to the phenol-phthalein neutral point, the final reaction varying between +1·5 and +13·7 ; in some of the strains the diminution in the acidity was very slight. As in the case of bovine strains the final reaction was on several occasions slightly higher than the lowest recorded reaction.

The exception mentioned above occurred in the case of H.19 "S.W." This strain grew slowly but finally yielded an abundant growth ; the final acidity of the broth was +4·5 degrees higher than the initial.

All the slightly virulent strains first caused a diminution in the acidity of the broth (this diminution was not observed in every case on account of the first tests not being made soon enough) and then an increase, the final acidity in the majority of the cases being higher than the initial. This excess of the final over the initial acidity varied from +0·5 to +17. In four instances the final acidity was slightly less than, and in one the same as the initial acidity.

Reactions with Viruses Exhibiting Experimental Variations.

In several instances both with human and bovine viruses strains of tubercle bacilli have been isolated from experimental animals which have differed in cultural characters and pathogenic effects from the first cultures of the virus isolated ; and in one case where the virus consisted of mixed sputum two different kinds of bacilli have been isolated from animals infected with the original material.

The results with these strains I shall consider separately ; they show that the differences in cultural luxuriance are accompanied by a corresponding difference in reaction.

In the case of H.16 J.H. the slightly virulent culture isolated from Calf 273 on two occasions caused the final acidity of the broth to be higher than the initial. The two virulent strains of this virus which have been tested both reduced the acidity of the broth ; the strain from Calf 559 grew better than that from Bull 423A., and it is interesting to note that the diminution in acidity was not so great.

In the case of H.17 Sp. B., the slightly virulent culture isolated from Calf 265 caused once an increase and once a slight diminution in the final acidity of the broth ; another slightly virulent strain (Calf 339) caused a slight increase in the acidity of the broth. Three virulent strains of this virus have been tested : the cultures from Calf 571 and Calf 555 steadily diminished the acidity of the medium ; the latter grown on an unfermented broth (reaction +17·5) caused a greater diminution in acidity than the former grown on a fermented broth (reaction +10) ; in this case (Calf 555) the diluted broth in the cold became quite pink on the addition of phenol-phthalein. The strain from Rabbit 181 was tested four times, twice on fermented broth and twice on unfermented ; on the fermented broth the final reaction was on both occasions slightly less than the initial ; on the unfermented the final acidity was higher than the intial ; it may be mentioned that the culture used for the fermented broth was of an earlier generation than those used for the unfermented and moreover did not grow quite so abundantly.

Four strains of H.2. Sp. A. have been tested ; one, a slightly virulent culture isolated from Calf 111, rendered the broth on two occasions more acid ; the other three, all virulent cultures, caused a slight reduction in acidity.

In the case of H. 13 "A. D." only one culture, that from Calf 301, has been investigated ; this produced an acid reaction in the broth ; two attempts have been made to grow the culture obtained from Rat 15, but without success ; attention may here be drawn to the luxuriant growth obtained with this strain on glucose broth

In two instances the bovine tubercle bacillus has apparently been transformed by passage through the body of a dog into a bacillus having the cultural characters of a Group II. organism

The culture isolated from Dog 18, Virus B. IV. produced a moderately thick pellicle, but unfortunately before the test was completed the broth became contaminated.

The culture isolated from Dog 50, Virus B. IX. produced an acid reaction in the broth.

Reactions obtained under Conditions not strictly Comparable.

The results of certain tests are not included in the tables because of the exceptional conditions under which they were obtained.

These experiments will be described in detail.

Virus B.I. (Guinea-pig 14) was sown on a flask containing a syphon ; at the end of 133 days very little growth had taken place, so the syphon, being required for another flask, was removed and replaced by a solid rubber cork. After five weeks further incubation, active multiplication suddenly set in, and in a very short time the whole surface of the broth was covered. The final reaction of the broth was 4 per litre more acid than the initial reaction [*See* page 63.]

Four strains of human origin, three slightly virulent and one virulent for calves and rabbits, were grown on a fermented broth which took several days to filter ; when at last filtered clear, the broth was considerably diminished in bulk, and the loss was made up with distilled water before adding the peptone, salt and glycerin.

The broth was evidently unfavourable for the full development of the cultures, and the pellicle formed in each case was thin, grey and translucent, and of about the thickness of ordinary tissue paper ; the acidity of the broth was also in each case steadily reduced and in about equal degrees.

The results are important as showing that when slightly virulent tubercle bacilli produce thin membranes, they do not react differently from virulent strains.

Interpretation of Results.

Reviewing the experiments as a whole it is found that all the strains of tubercle bacilli used in the investigation whether of human or bovine origin, first casused a diminution in the acidity of the broth.

In a few cases this diminution steadily progressed until the broth became neutral, or nearly neutral, to phenol-phthalein.

In other cases after a preliminary diminution in acidity, varying in amount, but never great enough to cause the medium to become neutral to phenol-phthalein, the original acidity of the medium was restored and in many cases surpassed.

Besides these cultures, there are others which have produced all intermediate varieties of reaction ; some have steadily reduced the acidity of the broth to a varying extent, but not to the phenol-phthalein neutral point, others after first diminishing the acidity have shown all degrees of return from the lowest point reached to the initial acidity of the broth. If these results are arranged in numerical order, beginning with the strain which has reduced the acidity to the greatest extent and ending with the strain which has produced the greatest amount of acid, it will be found they form a continuous series in which there is no gap indicating that two wholly distinct kinds of bacilli are being dealt with.

Examined with reference to the virulence of the organisms such a series will show that, although there is no sharp line of demarcation, if a line be drawn at a point where the initial and final reaction are the same, all the strains above the line with few exceptions possess high virulence, whilst all the strains below the line with one exception have only slight virulence for the calf and rabbit.

As will be seen from the descriptions of the characters of the pellicles the great majority of the strains which caused a marked diminution in the acidity of the medium

produced much less bulky membranes than those which increased the acidity of the medium.

Virulent tubercle bacilli, as is well-known, grow with more difficulty and ordinarily produce on broth much thinner pellicles than the slightly virulent human bacillus.

The slightly virulent strains on the other hand grow sometimes rapidly, sometimes slowly, but always on a favourable medium finally produce an opaque, wrinkled, or warty membrane often of considerable density.

In the latter cases the amount of growth was obviously greater than that produced by the majority of the bovine bacilli. It was for this reason that an estimate of the amount of growth was made in order to ascertain whether there was any correspondence between it and the change in reaction.

These estimations (volumetric only) together with the corresponding change in reaction are given in tabular form. The table shows that, although the change in reaction is not strictly in accordance with the amount of growth, generally when the growth has been scanty the medium has become less acid, and when the growth has been abundant more acid.

It has been observed in these experiments, and it has also been pointed out by Theobald Smith, that in the case of the slightly virulent bacillus acid accumulates very rapidly towards the end of the experiment. In several instances it has happened that after more than two months' growth the acidity of the medium has been only about equal to, or even less than, the initial acidity, but when tested after the lapse of a few more weeks there was found to have been a sudden rise in the acidity, although no further increase in the thickness of the pellicle could be definitely said to have taken place.

The rise of acidity does not appear to be throughout the experiment in exact proportion to the amount of growth.

It is clear, therefore, that in estimating the relationship between cultural luxuriance and change in reaction another factor, *i.e.*, duration of growth, besides total amount of growth, must be taken into consideration. The growth of the bovine tubercle bacillus stops much sooner than that of the human bacillus; yet both at first react in the same way and if, as has occurred in three cases, the human bacillus ceases to grow when the pellicle formed is still thin and translucent, the change in reaction is not different from that produced by a virulent bacillus growing at the same rate.

Moreover, when virulent strains grow well and produce moderately thick pellicles the diminution in acidity is not so great as when thin membranes are formed. Were it true that all virulent strains caused an alkaline change only in the broth, those virulent strains which have produced moderately good growths ought to have reduced the acidity of the broth to a far greater extent than those which grew scantily. My results clearly show that this greater effect has not followed, and none of the bacilli of Group I. possessed of high virulence, which have grown well have made the broth neutral to phenolphthalein.

In fact, in many instances, the diminution in acidity has been only a very slight one and in one instance when the growth was specially abundant the final reaction was 4 per litre more acid than the initial.

It seems to me that these experiments at any rate prove conclusively that there are strains of tubercle bacilli possessed of high virulence, which have the power, equally with the slightly virulent bacillus, of producing acid out of glycerin. As regards the virulent strains which have reduced the acidity of the medium to a marked degree it would be impossible to say, from information obtained solely by the method of titration, whether acid was or was not produced during the growth of the bacillus; this could only be determined by exact chemical analysis. My own opinion is that in these cases also, except perhaps when the growth was extremely thin, a certain amount of acid, varying with the amount of growth, was formed, which was masked by a much greater production of alkali. This view is based upon the fact brought out by my experiments that the diminution in acidity is less pronounced when the growth is moderate in amount than when it is very scanty.

Summary and Conclusion.

The investigation has shown that when virulent tubercle bacilli, whether of human or bovine origin, are grown on the surface of glycerin broth the acidity of the medium is gradually diminished, the final acidity being less than the initial; the diminution is variable in amount and has in general been greatest when the growth was scantiest.

On the other hand when slightly virulent tubercle bacilli are grown on the surface of glycerin broth the acidity of the medium is at first diminished; then as the pellicle increases in thickness the acidity gradually rises again, the final acidity being higher than the initial. To this general rule exceptions have occurred—for example, the final acidity with bacilli of Group I. has on one occasion exceeded the initial, whilst that in the case of bacilli of Group II. has sometimes been less than the initial—and the distinction which otherwise might have been drawn between the virulent and slightly virulent viruses, the former reducing, the latter increasing the acidity of the medium, is thereby abolished. Yet if no exceptions had occurred it would be incorrect to assert that the distinction so drawn signified a difference in physiological properties; since we cannot neglect variations in reaction attributable merely to differences in amount of growth.

Although the change in reaction has not in every case been in strict accordance with the amount of growth, some correspondence between the two can be noted.

When an organism grows luxuriantly the final acidity of the broth is usually higher than the initial; when an organism grows scantily the reduction in acidity is more pronounced than when the growth is moderate in amount.

My observations therefore clearly show that glycerin broth does not differentiate tubercle bacilli into two classes, and yield no evidence that there is an essential difference in the action of different strains of tubercle bacilli on glycerin.

Comparison with Theobald Smith's Results.

I am able to confirm Theobald Smith in his general statements that the slightly virulent tubercle bacillus of human origin, when grown upon the surface of glycerin broth, first causes a diminution in the acidity of the medium and then an increase, and that there are strains of tubercle bacilli of bovine origin which steadily diminish the acidity of the broth.

My results do not, however, support the conclusion he draws from his experiments—that the differences he has observed point to a difference in physiological properties.

This conclusion is based upon the investigation of a relatively small number of strains, six human and six bovine; yet if the results as set out in his table be analysed and arranged numerically in a series according to the differences in each case between the initial and final reaction, they form, as Dr. Eastwood has pointed out, an unbroken series in which there is nowhere a gap that would justify the conclusion that two essentially different kinds of organisms were dealt with.

In Theobald Smith's paper no mention is made of the amount or character of the final growth, so that it is not possible to institute exact comparisons between his experiments and mine; the only data given are in the case of a virulent organism, Human XI., which always grew scantily and on three occasions failed completely to cover the surface. It is the experience of most observers that virulent tubercle bacilli grow less readily and generally produce a smaller volume of growth than slightly virulent bacilli.

When two related organisms, such as the colon bacillus and the hog cholera bacillus, growing equally rapidly and vigorously, produce in a liquid medium containing a certain sugar, one an acid reaction, the other an alkaline one, differences in biochemical action may be said to have been demonstrated, but it would be incorrect to assert on similar grounds that physiological differences exist between two strains of tubercle bacilli when one produces a feeble membrane which covers only a part of the surface, while the other produces a membrane of considerable density.

In his most recent paper Theobald Smith recommends that in order to bring out the difference between the two kinds of organisms at least 3 per cent. glycerin should be employed; he found that, whereas the percentage of glycerin (traces up to 5 per cent.) had no influence on the final reaction in the case of the bovine bacillus, in the case of the human bacillus acid was only produced in marked degree when 3 to 5 per cent. glycerin was present.

In my experiments with bacilli of low virulence the final acidity has rarely been lower than the initial and this in spite of the fact that the broth I have used has contained 2 per cent. glycerin only. It is indeed noteworthy that the amount of acid produced by the slightly virulent organisms on 2 per cent. glycerin has in general been very much greater than in Theobald Smith's experiments in which 3 to 5 per cent. glycerin was used.

TABLE I.

SUMMARY OF RESULTS WITH TUBERCLE BACILLI OF BOVINE ORIGIN.

Virus and Strain.	Initial reaction of broth.	Lowest recorded reaction.	Final reaction.	Difference between final and initial reaction.	Characters of pellicle.
B I. Cow 44	+12·5	0·0	Neutral or faintly alkaline.	−12·5	Thin and translucent.
,, ,,	+20	+10·	+11·	− 9·	Thin greyish white and lacelike.
B II. Calf 134	+21·	+10·6	+10·6	−10·4	Partly opaque and white, partly thin and filmy.
,, ,,	+21·	+ 8·	+ 8·5	−12·5	Thin grey and almost transparent, with a small warty island.
B IV. Direct	+11·	+2· or 1·	+2· or 1·	−9 or 10·	Thin grey and translucent.
,, ,,	+17·	+11·	+11·	− 6·	Grey translucent membrane of the thickness of ordinary tissue paper.
B V. Goat 2	+21·	+10·	+10·3	− 9·7	Moderately thick opaque white pellicle.
,, ,,	+13·	+ 5·	+ 5·	− 8·	Opaque white pellicle of moderate thickness.
,, ,,	+12·	+ 6·25	+ 7·	− 5·	Opaque whitish islands coverin not more than half the surface.
B V. Heifer 80	+11·	+ 7·	+ 8·5	− 2·5	Thin grey and translucent.
B VIII. Direct	+17·	—	+13·5	− 6·	Thin grey membrane with opaque whitish patches covering about half the surface.
B IX. G.P. 1069	+21·	+10·2	+11·	−10·	Greyish translucent speckled membrane which sank at the end of the fifth week.
B IX. Lemur 2	+17·5	+11·5	+11·5	− 5·5	Partly opaque white and slightly warty, partly thin and translucent; the whole of the pellicle had sunk at the end of the 38th day.
B IX. Dog 60	+17·5	+15·	+16·	− 1·5	Thin membrane which sank at the end of the 4th week.
B X. G.P. 1070	+20·	+13·5	+11·5	− 8·5	Thin and grey and studded with opaque raised colonies.
B XI. Rabbit 66	+21·	+ 8·	+10·5	−10·5	Partly greyish-white semi-opaque, and studded with raised colonies, partly thin and translucent.
B XIII. Rabbit 116	+10·	+ 1·	+ 3·	− 7·	Very thin.
B XIV. Direct	+17·	+11·	+11·	− 6·	Thin grey membrane with opaque warty patches, covering a little more than half the surface.
B XV. Direct	+10·	+ 4·5	+ 7·	− 3·	Resembled ordinary tissue paper.
B XVIII. Direct	+10·	+ 1·	+ 3·	− 7·	Very thin.

TABLE II.

SUMMARY OF RESULTS WITH TUBERCLE BACILLI OF HUMAN ORIGIN.

(*a*) Virulent Strains.

Virus and Strain.	Initial reaction of broth.	Lowest recorded reaction.	Final reaction.	Difference between initial and final reaction.	Characters of pellicle.
H 7. "C.M." Cow 73	+21·	+ 8·3	+ 9·6	−11·4	Thin translucent membrane containing a few opaque patches.
H 7. "C.M." Cow 73	+20·	+ 9·	+13·7	−63·	Thin grey and translucent with thicker white patches.
H 10. "B.S." Heifer 231	+13·	+ 3·75	+ 3·75	− 9·25	Thin grey and translucent with some opaque white patches.
H 14. "F.S." Calf 125	+13·	+ 3·5	+ 5·	− 8·	Opaque warty islands surrounded by a delicate grey membrane.
H 16. "J.H." ,, 559	+13·	+ 4·5	+ 5·25	− 7·75	Broken up into islands, some thin and grey, others thicker and more opaque.
H 16. "J.H." ,, 423a	+13·	+ 3·75	+ 3·75	− 9·25	Thin and filmy, with three opaque islands.
H 19. "S.W." Heifer 239	+17·5	—	+22·	+ 4·5	Resembled heavy lace.
H 28. "C.L." Direct -	+14·	+ 1·5	+ 1·5	−12·5	Almost transparent except for two small opaque patches.
H 29. "M.F." G.P. 1334	+17·5	+14·	+14·	− 3·5	Partly thick yellow and warty; partly greyish-white and translucent.
H 31. "L.F." Direct -	+14·	+ 6·5	+ 6·5	− 7·75	Surface half covered with moderately thick white pellicle; rest with a few delicate grey islands.
H 32. "Y.W." Direct Mesenteric glands.]	+13·	+ 4·5	+ 7·	− 6·	Thin and grey with a few opaque whitish patches.
H 38. "J.M." G.P. 793	+15·	+ 7·	+ 7·	− 8·	Thin grey and translucent.
H 49. "T.C." Direct -	+13·	+ 9·5	+11·	− 2·	Moderately thick.
H 49. "T.C." Direct -	+13·	+11·	+11·	− 2·	Moderately thick.
H 53. "D.H." G.P. 1482	+20·	+ 8·5	+ 8·5	−11·5	Thin grey and translucent, with thicker white patches.

(*b*) Slightly Virulent Strains

Virus and Strain.	Initial reaction of broth.	Lowest recorded reaction.	Final reaction.	Difference between initial and final reaction.	Characters of pellicle.
H 2. "Sp. A." Calf 111 -	+ 15·	+ 7·	+ 18·	+ 3·	Thick creamy-white and wrinkled.
H 2. "Sp. A." Calf 111 -	+ 20·	+ 16·5	+ 23·5	+ 3·5	Opaque white and wrinkled.
H 8. "S.C." Direct - -	+ 11·5	+ 9·6	+ 18·	+ 6·5	Partly thick opaque and white, partly thinner and translucent.
H 9. "C.T." Calf 183 -	+ 10·5	+ 6·75	+ 10·5	0 0	Moderately thick, greyish-white and wrinkled.
H 11. "E.D." Calf 221 -	+ 11·5	+ 10·3	+ 18·	+ 6·5	Partly thick and opaque white partly thinner and more translucent.
H 12. "H.N." Calf 123 -	+ 17·	—	+ 24·5	+ 7·5	Thick creamy-white and wrinkled.
H 13. "A.D." Calf 301 -	+ 15·	+ 8·5	+ 20·	+ 5·	Thick creamy-white and wrinkled.
H 15. "I.W." Calf 311 -	+ 10·	+ 7·	+ 18·	+ 6·5	Thick white warty growth.
H 16. "J.H." Calf 273 -	+ 11·5	+ 5·	+ 18·	+ 6·5	Thick creamy and wrinkled.
H 17. "Sp.B." Calf 265 -	+ 10·	+ 7·	+ 14·	+ 4·	Moist whitish and slightly wrinkled.
H 17. "Sp.B." Calf 265 -	+ 20·	+ 14·5	+ 17·	− 3·	Greyish-white and wrinkled, finally became broken up into islands.
H 17. "Sp.B." Calf 339 -	+ 18·	- -	+ 20·5	+ 2·5	Thick opaque white slightly creamy and very warty.
H 18. "T.T." Calf 131 -	+ 15·	—	+ 28·	+ 13·	Thick warty and wrinkled.
H 22. "F.W." Calf 293 -	+ 17·	+ 17·	+ 23·	+ 6·	Partly yellowish and warty, partly greyish white and wrinkled.
H 23. "J.P." Calf 365 -	+ 14·	+ 6·5	+ 13·5	0·5	Partly thick and slightly creamy, partly grey and translucent.
H 26. "K.M." G. P. 1326	+ 17·5	+ 14·	+ 21·5	+ 4·	Unequal in thickness, greater part being thick creamy and warty.
H 27. "B.D." G. P. 1216	+ 17·5	—	+ 25·	+ 7·5	For most part thick with warty patches; parts were thin and grey.
H 30. "E.M." G. P. 1337	+ 13·	+ 8·	+ 18·	+ 5·	Thick creamy and wrinkled.
H 34. "C.U." G. P. 236 -	+ 10·	+ 5·5	+ 8·75	− 1·25	Moderately thick opaque white.
H 34. "C.U." G. P. 236 -	+ 11·5	+ 9·5	+ 12·	+ 0·5	Thin and grey with a few whitish patches.
H 36. "M.D." Direct -	+ 19·5	+ 13·	+ 27·	+ 7·5	Thick and creamy-white.
H 37. "O.J." G. P. 1427 -	+ 17·5	+ 13·	+ 20·5	+ 3·	Unequal in thickness but for the most part thick creamy-yellow wrinkled and warty.
H 39. "M.B." Direct -	+ 17·	+ 17·	+ 22·	+ 5·	Moderately thick greyish-white and very wrinkled.
H 41. "A.S." G. P. 1435	+ 17·	+ 18·5	+ 22·	+ 5·	Thick creamy-white and wrinkled.
H 42. "M.R." G. P. 1447	+ 14·	+ 6·5	+ 13·	+ 1·	Moderately thick greyish-white moist and uniform.
H 44. "D.C." Direct -	+ 17·	—	+ 24·	+ 7·	Thick creamy wrinkled and very warty.
H 45. "F.M." G. P. 1137 -	+ 11·	+ 7·	+ 18·	+ 7·	Thick creamy and wrinkled.
H 45. "F.M." G. P. 1137 -	+ 11·	+ 7·	+ 28·	+ 17	Thick creamy and wrinkled.
H 48. "W.P." G. P. 1494	+ 10·	+ 6·75	+ 11·	+ 1·	Moderately thick whitish and slightly wrinkled.
H 50. "P.H." G. P. 1490	+ 10·	+ 6·75	+ 12·	+ 2·	Moderately thick creamy-white and wrinkled.
H 51. "H.M." Direct -	+ 17·	−	+ 22·5	+ 5·5	Moderately thick greyish-white or slightly creamy.
H 52. "T.P." G. P. 1567	+ 17·	+ 16·	+ 18·	+ 1·	Moderately thick creamy and wrinkled.
H 54. "C.W." G. P. 1609	+ 13·	+ 12·	+ 15·	+ 2·	Thick wrinkled and warty.
H 55. "R.D." Direct -	+ 13·	+ 10·	+ 11·5	− 1·5	Moderately thick greyish-white and wrinkled.
H 55. "R.D." Direct -	+ 13·	+ 9·5	+ 13·5	+ 0·5	
H 56. "F.T." G. P. 1682	+ 20·	—	+ 21·	+ 1·	Thick creamy and wrinkled.
H 57. "B.J." - - -	+ 17·	—	+ 30·	+ 13	Thick creamy and very warty.
H 61. "E.C." Direct - (Mesenteric glands.)	+ 20·	—	+ 21·	+ 1·	Moderately thick opaque white islands not completely covering the surface.

TABLE III.

SUMMARY OF RESULTS WITH VIRUSES, DIFFERENT STRAINS OF WHICH HAVE SHOWN DIFFERENCES IN VIRULENCE.

Virus and Strain.	Initial reaction of broth.	Lowest recorded reaction.	Final reaction.	Difference between initial and final reaction.	Characters of pellicle.
H 16. "J.H." Calf 273* -	+ 11·5	+ 7·	+ 20·	+ 8·5	Thick creamy and wrinkled.
H 16. "J.H." Calf 273* -	+ 11·5	+ 5·	+ 18·	+ 6·5	Thick creamy and wrinkled.
H 16. "J.H." Bull 423a.† -	+ 13·	+ 3·75	+ 3·75	− 9·25	Thin and almost transparent with a few thicker islands.
H 16. "J.H." Calf 559† -	+ 13·	+ 4·25	+ 5·25	− 7·75	Composed of islands, mostly grey and translucent a few opaque and white.
H 17. "Sp. B." Calf 265.* -	+ 10·	+ 7·	+ 14·	+ 4·	Moderately thick moist white and slightly wrinkled.
H 17. "Sp. B." Calf 265.* -	+ 20·	+ 14·5	+ 17·	− 3·	Greyish-white and finely wrinkled; moderately thick.
H 17. "Sp. B." Calf 339.* -	+ 18·	—	+ 20·5	+ 1·5	Thick creamy-white warty and slightly wrinkled.
H 17. "Sp. B." Rabbit 181.‡	+ 13·	+ 9·5	+ 9·5	− 3·5	Opaque milky-white islands, which did not completely cover the surface.
H 17. "Sp. B." Rabbit 181.‡	+ 13·	+ 7·	+ 10·	− 3·	One-third opaque and white, rest thin grey and translucent.
H 17. "Sp. B." Rabbit 181.‡	+ 17·5	+ 17·5	+ 23·5	+ 6·	Moderately thick and wrinkled.
H 17. "Sp. B." Rabbit 181.‡	+ 20·	—	+ 21·	+ 1·	Thick wrinkled and slightly warty.
H 17. "Sp. B." Calf 555.† -	+ 17·5	+ 6·	+ 7·	− 10·5	Thin and grey with some slightly thicker patches.
H 17. "Sp. B." Calf 571.† -	+ 10·	+ 4·75	+ 4·75	− 5·25	Ditto.
H 2. "Sp. A." Calf 111* -	+ 20·	+ 16·5	+ 23·5	+ 3·5	Thick opaque white and wrinkled.
H 2. "Sp. A." Calf 111* -	+ 15·	+ 7·	+ 18·	+ 3·	Thick creamy-white and wrinkled.
H 2. "Sp. A." Heifer 11† -	+ 10·	+ 6·5	+ 9·	− 1·	Very thin.
H 2. "Sp. A." Calf 83.† -	+ 12·	+ 5·5	+ 9·5	− 2·5	Thin grey and translucent.
H 2. "Sp. A." Calf 93.† -	+ 13·	+ 9·	+ 11·5	− 1·5	Thin and grey; the pellicle broke up into islands and in three weeks most of these had sunk; a few remained floating and these gradually became opaque and white.
B IX. Dog 50* - - -	+ 20·	+ 13·5	+ 22·	+ 2·	Thick opaque white and granular.

* Slightly virulent for bovines.
† Highly virulent for bovines.
‡ Virulent in early generations, slightly virulent in later.

TABLE IV.

TABLE SHOWING THE RELATION BETWEEN THE CHANGE IN REACTION AND THE AMOUNT OF GROWTH.

Virus and Strain.		Difference between initial and final reaction.	Volume of growth in cubic centimetres.
H 2. "Sp.A."	Calf 555	−10·5	1·3
H 16. "J.H."	Calf 423a	− 9·25	1·25
B I.	Cow 44	− 9°	2·8
B X.	G.P. 1070	− 8·5	2·3
H 38. "J.M."	G.P. 793	− 8°	1·7
H 31. "L.F."	Direct	− 7·5	1·6
H 26. "K.M."	G.P. 1326	− 7·5	1·5
H 7. "C.M."	Cow 73	− 6·3	3·3
B IX.	Lemur 2	− 6°	1·15
B IV.	Direct	− 6°	1·6
B XIV.	Direct	− 6°	1·1
H 17. "Sp.B."	Calf 571	− 5·25	1·5
H 37. "O.J."	G.P. 1427	− 5°	1·6
B VIII.	Direct	− 3·5	1·15
H 29. "M.F."	G.P. 1329	− 3·5	2·8
H 17. "Sp.B."	Calf 265	− 3°	3·
H 49. "T.C."	Direct	− 2°	2·9
H 49. "T.C."	Direct	− 2°	2·6
H 2. "Sp.A."	Calf 93	1·5	2·35
H 23. "J.P."	Calf 365	0·5	3·8
H 52. "T.F."	G.P. 1565	+ 1°	2·8
H 61. "E.C."	Direct (Mesenteric Glands)	+ 1°	2·5
H 56. "F.T."	G.P. 1682	+ 1°	2·3
H 17. "Sp.B."	Rabbit 181	+ 1°	2·9
B IX.	Dog 50	+ 2°	3·6
H 54. "C.W."	G.P. 1609	+ 2°	4·0
H 17. "Sp.B."	Calf 339	+ 2·5	2·9
H 37. "O.J."	G.P. 1427	+ 3°	2·6
H 2. "Sp.A."	Calf 111	+ 3·5	3·3
H 19. "S.W."	Goat 11	+ 4·5	4·3
H 26. "K.M."	G.P. 1326	+ 4°	2·6
H 39. "M.B."	Direct	+ 5°	3·2
H 41. "A.S."	G.P. 1435	+ 5°	3·5
H 51. "H.M."	Direct	+ 5·5	2·8
H 22. "F.W."	Calf 293	+ 6°	2·8
H 17. "Sp.B."	Rabbit 181	+ 6°	3·4
H 44. "D.C."	Direct	+ 7°	3·9
H 27. "B.D."	G.P. 1216	+ 7·5	4·0
H 12. "H.N."	Calf 123	+ 7·5	3·3
H 57. "B.J."		+13°	2·8

DETAILED DESCRIPTION OF THE EXPERIMENTS.

VIRUS B I.—(Cow 44.)

The culture used for the inoculation was taken from broth and was the sixth generation, the last three of which had been grown on broth.

The broth was fermented and contained 2 per cent. glycerin.

During the first 70 days the inoculated material showed no increase in area but slowly increased in thickness becoming warty and nodular; then a grey film began to grow from the margins; this gradually extended and the whole surface was covered on the 91st day, the growth climbing up the sides of the flask; the pellicle subsequently thickened a little and became greyish white and slightly wrinkled.

Initial titration - -	-+12·5
Reaction after 77 days -	-+12
(pellicle slowly extending)	
Reaction after 91 days -	-+ 8
(surface covered)	
Reaction after 96 days -	-+ 2 or 3
,, ,, 106 ,, -	-+ 3
,, ,, 151 ,, -	-broth neutral or faintly alkaline.

VIRUS B I.—(Cow 44.)

The above culture was inoculated at the same time on 5 per cent. glycerin broth.

Growth during the first 4 weeks as opaque white nodules with stalactitic down-growths; in the fifth week a grey marginal film developed around the nodules; at the end of the seventh week the whole surface was covered with a pellicle which climbed up the sides of the flask; the growth was grey and showed whitish striae radiating out from a central group of opaque nodules; the pellicle thickened slightly and was finally greyish white and finely wrinkled.

The broth became nearly neutral to phenol-phthalein after the surface was covered; then a development of moulds stopped the experiment.

(This experiment is not included in the table).

VIRUS B I.—(Cow 44.)

The culture used for the inoculation was the twenty-third generation, the last four of which were grown on glycerin broth.

The broth was not fermented and contained 2 per cent. glycerin.

Growth took place from the outset and in a little more than 3 weeks the surface was covered with a thin greyish-white lace-like membrane; during the next fortnight the pellicle became slightly thicker and whiter, some parts being a little thinner than others. The pellicle then broke up into islands and all further growth ceased, many of the islands becoming moist and falling to the bottom of the flask.

Initial reaction - - -	- + 20
Reaction after 21 days -	- + 12
,, ,, 39 ,, -	- + 10
,, ,, 60 ,, -	- + 10·5
,, ,, 79 ,, -	- + 11

VIRUS B I.—(Original Material, through Guinea-Pig 14.)

The culture used was the twenty-second generation, the last two of which were grown on broth.

The broth was fermented and contained 5 per cent. glycerin.

Examined 133 days after inoculation the inoculated material was seen to have increased in thickness and area only to a very slight extent; there was an appearance, however, around the margins as if growth might take place. The syphon apparatus was removed and the flask stoppered with a solid rubber cork. A month later the small island had not apparently undergone any change. Examined 9 days later the whole surface was found covered with a thin grey pellicle traversed by wavy white lines radiating out from an opaque central patch. Subsequently the membrane thickened slightly and became greyish white.

Initial reaction - - -	- + 12
Reaction after 164 days - -	- + 16
,, ,, 191 ,, - -	- + 16

VIRUS B II.—(Calf 134.)

The culture used for the inoculation was taken from broth and was the seventh generation, the first six of which were grown on pure serum.

The broth was fermented and contained 2 per cent. glycerin.

In ten days the inoculated material had doubled in area; in 39 days there was an irregular greyish white pellicle covering an area equal to that of a two-shilling piece; this slowly increased in area and thickness and in 75 days there was an opaque greyish white patch covering not quite two-thirds of the surface with a grey film spreading out from the margins; the grey film continued to extend but the whole surface was not covered until the 121st day; the filmy growth then sank to the bottom leaving the opaque patch floating.

Initial reaction - - - -	- +21
Reaction after 82 days - -	- +11·2
,, ,, 121 ,, - -	- +10·6

VIRUS B II.—(Calf 134.)

A flask of the same broth, but containing 5 per cent. glycerin, was inoculated at the same time as the above from the same culture.

In 29 days there was a whitish opaque island the size of a sixpenny bit; this slowly increased in thickness, becoming warty on the surface and showing short stalactitic processes on the under surface; around the margins were delicate grey thread-like processes; after 81 days there was very little further increase, but the growth was yellower and more curved. On the 110th day the whole surface was found covered with a thin grey almost transparent pellicle traversed by fine wavy whitish threads radiating outwards from the opaque patch, which was now of a brownish yellow colour thick and warty, and with raised edges.

Initial reaction - - - - -	+21
Reaction after 81 days - - -	+17
„ „ 110 „ - - -	+ 8
(surface covered)	
„ „ 132 „ - - -	+8·5

VIRUS B II.—(Bull 22.)

The culture used for the inoculation was taken from glycerin milk and was the 21st generation, nine of which had been grown on glycerin media.

The broth was not fermented and contained 2 per cent. glycerin.

Growth was very slow; ten weeks after inoculation there were three grey translucent islands, the largest about the size of a shilling, each showing one or two small opaque white thickenings.

Initial reaction - - - -	+ 17·5
Reaction after 94 days - - -	+ 15·75

(This experiment is not included in the table.)

VIRUS B IV.—(Original Material.)

The culture used was taken from broth and was the tenth generation, the last two of which were grown on broth.

The broth was fermented and contained 2 per cent. glycerin.

Forty-four days after there was a series of small opaque nodules with stalactitic downgrowths; the nodules slowly increased in size and on the 57th day were seen to be forming grey marginal films; on the 69th day the whole surface was nearly covered with a thin greyish translucent film through which curved whitish lines radiated from the opaque nodules; there was no subsequent increase in the thickness of the film.

Initial reaction - - - -	+ 11
Reaction after 77 days - -	+ 4
(surface nearly covered).	
Reaction after 96 days - - -	+ 3·5
„ „ 106 „ - - -	+ 2 or 1

VIRUS B IV.—(Original Material.)

The culture used was the 41st generation, the last seven of which had been grown on liquid media.

The broth was unfermented and contained 2 per cent. glycerin.

Growth took place from the outset and in 13 days the whole surface of the broth was covered with a thin uniform grey membrane. At the end of five weeks the membrane was grey translucent finely wrinkled and about the thickness of ordinary tissue paper. Subsequently no further increase in thickness of the pellicle was observed.

Initial reaction - - - - -	+ 17
Reaction after 21 days - - -	+ 12·5
„ „ 72 „ - - -	+ 11

VIRUS B IV.—(Calf 118.)

The culture used for the inoculation was taken from glycerin milk and was the 22nd generation, the last eleven of which had been grown on glycerin media.

The broth was not fermented and contained 2 per cent. glycerin.

Sixteen days after inoculation there was a delicate grey island with whitish thickenings slightly larger than a shilling; this slowly increased and ten weeks after inoculation about a quarter of the surface was covered; the pellicle was grey and translucent and showed opaque white thickenings; there was no further increase in the area of the pellicle and the opaque thickenings acquired a chalky white appearance.

Initial reaction - - - -	+ 17·5
Reaction after 94 days - - -	+ 16·5

(This experiment is not included in the tabular summary of results.)

VIRUS B IV.—(Dog 18.)

The culture used was taken from broth and was the eleventh generation.

The broth was unfermented and contained 2 per cent. glycerin.

Three weeks after inoculation there was an opaque white warty island, measuring 3 by 1½ in. On the fiftieth day two-thirds of the surface was covered with a moderately thick opaque white warty pellicle, whilst on the rest of the surface there were floating some opaque nodules with grey filmy margins. Sixteen days later the broth was found to have become turbid. A smear preparation made from the pellicle showed a few large diplococci scattered about.

Initial reaction - - - - -	+17·5
Reaction after 50 days - - - -	+24
„ „ 66 „ - - - -	+23·

(This experiment is not included in the tabular summary of results.)

VIRUS B V.—(Heifer 80.)

The culture used was the seventh generation, the last two of which had been grown on a liquid gelatine medium.

The broth was fermented and contained 5 per cent. glycerin.

In 29 days the whole surface was covered with a thin uniform greyish translucent growth; subsequently no change was observed in the appearance of the growth.

Initial reaction	-	-	-	- + 11
Reaction after 29 days	-	-	-	- + 7
„ „ 48 „	-	-	-	- + 8
„ „ 77 „	-	-	-	- + 8·5

VIRUS B V.—(Goat 2.)

The culture used was the seventh generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

In 34 days there was an island the size of a two-shilling piece; in 42 days a third and in 47 days the whole of the surface was covered with a pellicle which subsequently became of a moderate thickness; it was whitish and the surface was moist and reticulated. The initial reaction was + 13 and the final + 5.

VIRUS B V.—(Goat 2.)

The culture used was the tenth generation and was the second broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

In 31 days the whole surface was covered, the growth climbing up the sides of the flask; the pellicle was not uniform, part being opaque white and granular on the surface, the rest thin grey and translucent; later the growth became thicker and more uniform; it was opaque white wrinkled and resembled heavy lace; no increase in thickness was observed after the 64th day; on the 110th day scattered pearly white colonies were seen to have developed on the surface

Initial titration	-	-	-	-+21
Reaction after 34 days	-	-	-	-+18
„ „ 49 „	-	-	-	-+12
„ „ 64 „	-	-	-	-+11
„ „ 81 „	-	-	-	-+10
„ „ 110 „	-	-	-	-+15
„ „ 139 „	-	-	-	+13·5
„ „ 142 „	-	-	-	-+10·3

VIRUS B V.—(Goat 2.)

The culture used was the tenth generation and was the second broth subculture.

The broth was fermented and contained 5 per cent. glycerin.

In 31 days there was a thick opaque white pellicle covering not more than a fifth of the surface; this became broken up into islands which increased in area but never coalesced; the islands were opaque white and thick with a pitted surface; not more than half the surface was covered.

Initial reaction	-	-	-	+12
Reaction after 34 days	-	-	-	+11
„ „ 81 „	-	-	-	+ 6·25
„ „ 110 „	-	-	-	+ 7

VIRUS B VIII.—(Original Material.)

The culture used was the 31st generation, the last two of which had been grown on liquid media.

The broth was unfermented and contained 2 per cent. glycerin.

Seven days later there was a grey patch the size of a shilling with whitish thickenings. This slowly extended and on the 25th day about half the surface was covered with a thin grey pellicle containing irregular opaque whitish patches.

Initial reaction	-	-	-	+ 17
Reaction after 105 days	-	-	-	+ 13·5

VIRUS B IX.—(Original Material, through Guinea-Pig 1069.)

The culture used was taken from the condensation water of a serum tube and was the sixth generation.

The broth was fermented and contained 5 per cent. glycerin.

In 12 days a thin grey membrane speckled with opaque white foci covered from $\frac{1}{6}$ to $\frac{1}{5}$ of the surface; in 18 days the whole surface was covered and in 21 days was climbing up the sides of the flask; the pellicle was thin and grey and was closely speckled with small opaque whitish foci; the growth thickened up to the 33rd day, becoming uniform and greyish white; fissures then appeared in the pellicle the broken edges of which were moist and beginning to sink; two days later the whole of the pellicle had sunk to the bottom of the fluid.

Initial reaction	-	-	-	- +21
Reaction after 18 days	-	-	-	- +16·3
„ „ 33 „ (surface covered.)	-	-	-	- +10·2
Reaction after 35 days	-	-	-	- +11

VIRUS B IX.—(Dog 50.)

The culture used was the 2nd generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

Growth was slow. Three weeks after inoculation there was an opaque warty patch, $2\frac{1}{2} \times 1\frac{1}{2}$ inches; in 46 days about half the surface, and in 58 days the whole surface was covered with a thick opaque white granular pellicle (the growth appeared to be composed entirely of granules each about the size of a pin's head.)

Initial reaction	+ 20
Reaction after 60 days	+ 13·5
„ „ 81 „	+ 16·5
„ „ 130 „	+ 22

VIRUS B IX.—(Dog 60.)

The culture used was the third generation; the first two generations were grown on serum, the third on glycerin gelatine.

The broth was unfermented and contained 2 per cent. glycerin.

Growth took place from the outset and in 18 days the whole surface was covered with a grey translucent membrane mottled with irregular whitish thickenings; the growth climbed up the sides of the flask and already showed a tendency to break up and become moist at the edges; on the twenty-eighth day all the growth had sunk to the bottom of the broth.

Initial reaction	+17.5
Reaction after 19 days	+15
„ „ 28 „	+16

VIRUS B IX.—(Lemur 2.)

The culture used was the fourth generation and was taken from the condensation water pellicle of a serum tube.

The broth was not fermented and contained 2 per cent. glycerin.

Growth took place from the outset and in 21 days a quarter of the surface was covered with a pellicle which was partly opaque white and finely warty, and partly thin grey and almost transparent: 9 days later the rest of the surface was covered with a thin grey membrane which climbed up the sides of the flask; on the thirty-fourth day the opaque patch and on the thirty-eighth day the whole of the pellicle had fallen to the bottom of the flask.

Initial reaction	+17.5
Reaction after 34 days	+14
„ „ 38 „	+11.5

VIRUS B X.—(Original Material, through Guinea-Pig 1070.)

The culture used for the inoculation was taken from broth and was the twenty-fifth generation; the first twenty-four were grown on pure serum.

The broth was not fermented and contained 2 per cent. glycerin.

Growth at first was very slow; at the end of 3 weeks there was a thin grey speckled island less than an inch in diameter. Subsequently growth was more rapid and on the forty-sixth day three-quarters and on the 53rd day the whole of the surface was covered with a thin grey membrane closely studded with raised opaque white colonies. Subsequently the growth became thicker and the colonies more raised.

Initial reaction	+ 20
Reaction after 53 days	+ 13·5
„ „ 79	+ 11·5
„ „ 103	+ 11·5

VIRUS B XI.—(Original Material, through Rabbit 66.)

The material for the inoculation was taken from the condensation water of a serum tube and was the fourth generation.

The broth was fermented and contained 5 per cent. glycerin.

In 14 days there was a thin grey patch measuring $1\frac{1}{2} + \frac{1}{2}$ inch; in 35 days the patch measured about 2+1 inches and showed opaque white colonies; it slowly increased in area and in 77 days practically covered the whole surface; the pellicle varied in thickness, the greater part being greyish white and studded with opaque white raised colonies, but parts were quite thin and transparent; the surface became completely covered a few days later, but subsequently there was no further change in the growth with the exception of a slight increase in the size of the colonies.

Initial reaction	+21
Reaction after 77 days	+ 9·5
„ „ 96 „	+ 8
„ „ 119 „	+10·5

VIRUS B XIII.—(Original Material, through Rabbit 116.)

The culture used for the inoculation was taken from broth and was the eleventh generation; the first ten generations were grown on pure serum.

The broth was fermented and contained 2 per cent. glycerin.

In 6 days there was a thin grey island the size of a shilling; this increased rapidly in area and in 14 days the whole surface was covered with an extremely thin grey membrane veined with fine greyish-white tortuous lines; subsequently no definite increase in the thickness of the pellicle was observed.

Initial reaction	+10
Reaction after 20 days	+ 4
„ „ 34 „	+ 3
„ „ 65 „	+ 1
„ „ 99 „	+ 3

VIRUS B XIV.—(Original Material.)

The culture used was the twenty-fourth generation, the last two of which had been grown on liquid media.

The broth was unfermented and contained 2 per cent. glycerin.

Ten days after inoculation there was a grey island the size of a shilling with opaque whitish thickenings; this slowly increased in area and on the 59th day a little more than half the surface was covered with an irregular grey patch containing opaque and slightly warty patches. There was no further increase in growth.

Initial reaction - -	- +17	
Reaction after 108 days	- +11	The broth went pink in the cold on the addition of phenol-phthalein.

VIRUS B XV.—(Original Material.)

The culture used was taken from broth and was the fourth generation, the first three of which were grown on pure serum.

The broth was fermented and contained 2 per cent. glycerin.

In 10 days a thin uniform grey pellicle covered the surface of the broth and climbed up the sides of the flask; the pellicle thickened slightly, then became moist, and at the end of 5 weeks nearly half had fallen to the bottom; the growth that remained on the surface became broken up into islands which increased in area and eventually again covered the whole surface of the broth; the pellicle (incomplete) finally was about the thickness of ordinary tissue paper.

Initial reaction - -	- +10
Reaction after 14 days (surface covered) - -	- + 6·5
,, ,, 38 days - -	- + 4·5
,, ,, 67 ,, - -	- + 6·5
,, ,, 103 ,, - -	- + 7

VIRUS B XVIII.—(Original Material.)

The culture used was taken from broth and was the fifth generation, the first four of which had been grown on pure serum.

The broth was fermented and contained 2 per cent. glycerin.

In 10 days the surface was covered with an extremely thin grey membrane resembling in patches fine lace; at end of 35 days there was very little change; the pellicle was partly thin uniform and almost transparent and partly greyish with delicate lace-like markings; eventually the pellicle became broken up into islands which, however, showed little tendency to sink

Initial reaction	- +10	
Reaction after 14 days (surface covered)	- + 5	
,, ,, 38 days	- + 2	
,, ,, 67 ,,	- + 1	Before boiling the broth became pink on the addition of phenol-phthalein.
,, ,, 103 ,,	- + 3	

VIRUS B XXII.—(Original Material, through G.P. 1508.)

The culture used was the fourth generation and was taken from broth; the first three generations were grown on pure serum.

The broth was unfermented and contained 2 per cent. glycerin.

Growth was very slow; a month after inoculation there was a grey island, $\frac{3}{4}$ in. in diameter, with opaque white warty thickenings from the under surface of which hung short stalactitic down-growths; the island slowly increased in area and in 12 weeks measured about 2 by $1\frac{1}{2}$ in. Growth then ceased. The patch was irregular in outline, unequal in thickness, moist and very warty on the surface, the thicker parts being yellowish in colour.

Initial reaction - - - -	- +17·5
Reaction after 130 days - - -	- +16·

(This experiment is not included in the tabular summary of results.)

VIRUS B XXVIII.—(Original Material.)

The culture used was the fourth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

The inoculated material very slowly increased in thickness and area. Four weeks after inoculation there was a thick opaque whitish warty island 1 in. × $\frac{1}{2}$ in. with moist submerged margins. The island became a little larger, measuring on the 86th day about $2\frac{1}{2}$ in. × $1\frac{1}{2}$ in., and then sank.

Initial reaction - -	×17	
Reaction after 86 days -	×11.5	The broth went pink on the addition of phenol-phthalein before boiling.

(This experiment is not included in the tabular summary of results.)

VIRUS H 2. "Sp. A." (Heifer 11, through Guinea-Pig 340.)

The material was taken from a liquid gelatine medium which had been sown from broth.

The broth used was fermented and contained 2 per cent. glycerin.

In 5 days there was a small grey island with raised whitish foci on the surface; in 16 days the whole surface was covered with an extremely thin grey film with an irregular whitish meshwork; the growth subsequently showed no obvious increase and finally became broken up into islands.

Initial reaction	+ 10
Reaction after 22 days	+ 6·5
,, ,, 39 ,,	+ 7·7
,, ,, 80 ,,	+ 8
,, ,, 88 ,,	+ 9

VIRUS H 2. "Sp. A." (Calf 83, through Guinea-Pig 618.)

The culture was taken from broth.

The broth used was fermented and contained 2 per cent. glycerin.

Sixty-three days later there were three opaque white warty islands, showing stalactic downgrowths; the largest island measured 1¼ in. by 1 in., the others were quite small; 11 days later the surface was almost completely covered with a delicate grey membrane with fine wavy lines radiating out from the opaque patches; on the hundred-and-fifteenth day there was a thin uniform translucent grey pellicle surrounding three opaque patches which had latterly shown no change in character.

Initial reaction	+12
Reaction after 74 days	+ 8·5
,, ,, 115 days	+ 9·5

VIRUS H 2. "Sp. A." (Calf 93, through Guinea-Pig 534.)

The culture used was the twentieth generation and was the first broth subculture.

The broth was not fermented and contained 2 per cent. glycerin.

A week after inoculation there was a thin greyish island, 2½ in. by 1 in., containing opaque whitish foci.

On the sixteenth day the surface of the broth was covered with a grey pellicle speckled with whitish foci; the growth climbed up the sides of the flask.

Five days later the growth began to break up and sink and on the twenty-eight day most had fallen to the bottom of the flask. Several small islands remained floating; these, aggregated loosely together into a patch about the size of a five-shilling piece, gradually increased in thickness, but not in area, and became opaque and white.

Initial reaction	+13
Reaction after 21 days	+ 9
,, ,, 35 ,,	+12
,, ,, 112 ,,	+11·5

VIRUS H 2. "Sp. A." (Calf 111 through Guinea-Pig 690.)

The culture used for the inoculation was taken from milk and was the nineteenth generation; all the previous cultures had been grown on pure serum.

The broth was not fermented and contained 27 per cent. glycerin.

A fortnight after inoculation the surface was nearly covered with a grey translucent membrane mottled with opaque white patches. On the thirty-fifth day the pellicle was thicker and whiter but not wrinkled; from the under surface large flakes of growth were breaking off and falling to the bottom of the fluid. The pellicle finally became thicker more opaque and wrinkled but not warty.

Initial reaction	+ 20
Reaction after 23 days	+ 19
,, ,, 49 ,,	+ 16·5
,, ,, 76 ,,	+ 22
,, ,, 98 ,,	+ 23·5

VIRUS H 2. "Sp. A." (Calf 111, through Guinea-Pig 690.)

The material for the inoculation was taken from broth and was the sixth generation, the first five of which were grown on serum.

The broth was fermented and contained 5 per cent. glycerin.

Growth was very slow; after 71 days there was an opaque island measuring 3 by 1·5 cm.; this slowly increased in area and in 97 days covered half the surface; on the 113th day the whole surface was covered with a thick creamy white wrinkled pellicle which climbed up the sides of the flask; the growth was not quite equal in thickness to that produced by H. 45. "F.M.," and was not so pigmented.

Initial reaction	+ 15
Reaction after 113 days (surface covered).	+ 7
Reaction after 128 days	+ 10·5
,, ,, 142 ,,	+ 13
,, ,, 160 ,,	+ 16
,, ,, 172 ,,	+ 18

VIRUS H 7. "C.M." (Cow 73, through Guinea-Pig 552.)

The culture used for the inoculation was taken from the surface of glycerin milk and was the twentieth generation.

The broth was not fermented and contained 2 per cent. glycerin.

A fortnight after inoculation nearly half the surface was covered with thin grey-speckled islands; on the eighteenth day the whole surface was covered. About a month after inoculation the growth had attained its maximum development; it was not uniform in thickness, the greater part being greyish-white and semi-opaque, the rest thin grey and translucent. Subsequently the pellicle became moist and broke up into islands, many of which sank; at the end of the experiment there was a large amount of deposit.

(The growth closely resembled that obtained with H 53. D.H.)

Initial reaction	+20
Reaction after 28 days	+ 9
,, ,, 56 ,,	+ 9·5
,, ,, 81 ,,	+12·7

VIRUS H 7. "C.M." (Cow 73, through Guinea-Pig 552.)

The culture used was the fifteenth generation and was the first broth subculture.

The broth was fermented and contained 5 per cent. glycerin.

Growth was very slow at first; at the end of 30 days there were three small opaque nodules; these gradually increased in thickness and extent, the largest measuring 52 days after inoculation ¾ in. by ¼ in.; they continued to grow and on the sixty-sixth day a fringe of thin grey growth was seen around each of them. Growth was now rapid and 10 days after the membrane was seen to be beginning to form the surface was covered with a thin grey pellicle which climbed up the sides of the flask, and which subsequently thickened slightly and became slightly wrinkled; the opaque islands meanwhile also increased in thickness and area (largest 1¼ in. by ½ in.) and were moist looking and very warty on the surface.

Initial reaction - - - - -	+21
Reaction after 98 days - - -	+ 8·3
,, ,, 129 ,, - - -	+ 9·6

VIRUS H 8. "S.C." (Original Material.)

The culture used was the eleventh generation and was taken from broth.

The broth was fermented and contained 5 per cent. glycerin.

Growth rather slow in the beginning; in 23 days there was a thick white island measuring 1¾ in. by 1 in.; this increased fairly rapidly in size and in 37 days half the surface was covered with a thick opaque white pellicle, one margin of which was submerged, and half with a thin translucent grey membrane. On the fifty-fifth day more of the opaque pellicle was submerged and the thin membrane had become a little thicker; subsequently there was no appreciable change.

Initial reaction - - - -	+11·5
Reaction after 37 days - - -	+ 9·6
,, ,, 55 ,, - - -	+12
,, ,, 92 ,, - - -	+18

VIRUS H 9. "C.T." (Calf 183, through Guinea-Pig 996.)

The culture used for the inoculation was taken from broth and was the fourth generation, the first three of which were grown on pure serum.

The broth was unfermented and contained 5 per cent. glycerin.

Fifteen days later there was a thin grey pellicle covering about a quarter of the surface; in 23 days three-quarters, and in 35 days the whole of the surface was covered, the growth climbing up the sides of the flask; the pellicle was greyish white opaque and wrinkled and of moderate thickness; slight agitation caused numerous flakes of growth to fall to the bottom of the fluid; subsequently no change in the character of the growth was observed.

Initial reaction - - - -	+10·5
Reaction after 15 days - - -	+ 9
,, ,, 23 ,, - - -	+ 8·25
,, ,, 35 ,, - - -	+ 8
,, ,, 48 ,, - - -	+ 6·75
,, ,, 67 ,, - - -	+10
,, ,, 75 ,, - - -	+11
,, ,, 86 ,, - - -	+10·5
,, ,, 98 ,, - - -	+10·5

VIRUS H 10. "B.S." (Heifer 231.)

The culture used for the inoculation was the eighteenth generation and was the first subculture on broth.

The broth was fermented and contained 2 per cent. glycerin.

After 15 days three-quarters of the surface was covered with an extremely delicate grey lace-like growth; in 25 days the whole surface was covered; the pellicle was thin grey and almost transparent and showed slightly thicker greyish white patches; the pellicle subsequently thickened slightly, the transparent parts becoming translucent and showing a network of curved whitish lines, the greyish white patches becoming opaque and eventually slightly yellow.

Initial reaction - - - -	+13
Reaction after 25 days - - -	+ 6·5
,, ,, 49 ,, - - -	+ 4
,, ,, 80 ,, - - -	+ 4·5
,, ,, 119 ,, - - -	+ 3·75

VIRUS H 11. "E.D." (Calf 221, through Guinea-Pig 918.)

The culture used was the eighth generation and was the first broth culture.

The broth was fermented and contained 5 per cent. glycerin.

In 19 days there was a thick white island 2 in. by 1 in.; 13 days later about a quarter of the surface was covered and at the end of 54 days there was a thick white pellicle over half the surface and over the rest thin grey patches; at the end of 78 days the whole surface was covered partly with a thick white growth and partly with a thinner greyish white membrane, no further change in growth was observed.

Initial reaction - - - -	+ 11·5
Reaction after 54 days - - -	+ 10·6
,, ,, 78 ,, - - -	+ 11·5
,, ,, 102 ,, - - -	+ 18

VIRUS H 13. "A.D." (Calf 301.)

The culture used was taken from broth and was the fifth generation, the first four of which were grown on serum.

The broth was fermented and contained 5 per cent. glycerin.

In 27 days there was a thick opaque pitted pellicle covering half the surface; in 41 days three-quarters and in 55 days the whole surface was covered with a thick creamy white wrinkled pellicle

Initial reaction	+ 15
Reaction after 27 days	+ 8·5
The reaction diminished still further in acidity and then began to get more acid.	
Reaction after 104 days	+ 16
„ „ 109 „	+ 20
„ „ 115 „	+ 20

VIRUS H 16. "J.H." (Calf 273.)

The culture used for the inoculation was taken from broth and was the seventh generation, the first six of which were grown on pure serum.

The broth was fermented and contained 2 per cent. glycerin.

In 3 weeks the whole surface was covered with a greyish white wrinkled pellicle which climbed up the sides of the flask; this slowly increased in thickness and became creamy and more wrinkled.

Initial reaction	+11·5
Reaction after 21 days	+ 5
„ „ 36 „	+ 8
„ „ 47 „	+13
„ „ 57 „	+11
„ „ 75 „	+11·2
„ „ 89 „	+18

At the same time as the 2 per cent. glycerin broth a flask containing 5 per cent. glycerin broth was inoculated.

In 3 weeks there was an irregular pellicle measuring about 1½ by 1 inch; in 27 days about one-fifth and in 36 days four-fifths of the surface was covered; in 57 days the whole surface was covered with a thick creamy growth which was not so wrinkled as on the 2 per cent. broth, and did not climb up the sides of the flask.

Initial titration	+11·5
Reaction after 36 days	+ 7
„ „ 47 „	+13
„ „ 57 „	+10
„ „ 75 „	+12·5
„ „ 89 „	+20

VIRUS H 16. "J.H." (Bull 423 A.)

The culture used was the eleventh generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

Fifteen days later three small delicate grey islands were seen on the surface; these thickened, became opaque and yellowish white and from their margins an extremely fine almost transparent film grew out, which in 80 days had covered about three-quarters of the surface; eventually the whole surface was covered, the thin film increasing in thickness only very slightly.

Initial reaction	+13
Reaction after 79 days	+9·25
„ „ 117 „	+3·75

VIRUS H 16. "J.H." (Calf 559.)

The culture used for the inoculation was the third generation and was the first broth subculture

The broth was fermented and contained 2 per cent. glycerin.

Twenty-five days after inoculation there was a thin grey lace-like membrane covering between one-sixth and one-fifth of the surface; in 38 days three-fourths and in 42 days the whole of the surface was covered; the pellicle was thin grey and translucent and broken up into islands with a dull surface; subsequently many of the islands thickened, became opaque, whitish and warty on the surface.

Initial reaction	+13
Reaction after 42 days	+ 4·5
„ „ 66 „	+ 4·5
„ „ 90 „	+ 5·25

VIRUS H 17. "Sp. B."—(Calf 265.)

The culture used was the thirteenth generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

The inoculated material broke up into small fragments which formed the centres of growth. In 9 days there were numerous small delicate islands which increased rapidly in area and covered the surface in 16 days; the pellicle was thin grey and closely interspersed with fine whitish threads; it subsequently increased to a moderate thickness and was moist whitish and slightly wrinkled; the growth was not uniform, two or three quite thin grey patches being seen.

Initial reaction	+10
Reaction after 16 days	+9
„ „ 34 „	+7
„ „ 57 „	+8
„ „ 98 „	+10
„ „ 110 „	+12
„ „ 125 „	+14

VIRUS H 17. "Sp. B."—(Calf 265.)

The culture used for the inoculation was taken from milk and was the seventeenth generation.

The broth was not fermented and contained 2 per cent. glycerin. A month after the inoculation half the surface was covered with a grey translucent membrane mottled with small opaque raised patches. At the end of 6 weeks the whole of the surface was covered with a greyish-white finely wrinkled pellicle showing one or two warty patches; the growth finally broke up into islands.

Initial reaction	+ 20
Reaction after 42 days	+ 14·5
„ „ 69 „	+ 17·5
„ „ 91 „	+ 19

VIRUS H 17. "Sp. B."—(Calf 339, through Guinea-pig 1235.)

The culture used was the seventeenth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

At the end of 40 days about half the surface was covered with an opaque greyish white pellicle with a reticulated surface. Finally the whole surface of the broth became covered with a thick opaque white faintly creamy pellicle with a slightly wrinkled very warty surface.

Initial reaction	+ 18
Reaction after 93 days	+ 20·5

VIRUS H 17. "Sp. B."—(Rabbit 181.)

Culture fourth generation from broth.

The culture was used to inoculate two flasks of fermented broth, one containing 2 per cent., the other 5 per cent. glycerin.

On the 2 per cent. glycerin broth in 24 days there was a number of greyish white islands covering about a quarter of the surface; in 40 days the greater part of the surface was covered with opaque milky white islands; these subsequently, beyond a slight increase in thickness, showed no change and never coalesced.

Initial reaction	+ 13
Reaction after 40 days	+ 12
,, ,, 104 ,,	+ 9·5

On the 5 per cent. glycerin the inoculated material showed a slight increase in thickness and area; in 52 days there was a thick white patch, irregular in outline, covering between one-sixth and one-fifth of the surface, and at the end of 82 days about a third of the surface was covered; examined at the end of 102 days a thin grey film was seen to have grown out from the margins; this slowly expanded, but never completely covered the remaining surface.

Initial reaction	+ 13
Reaction after 102 days	+ 7
,, ,, 124 ,,	+ 9
,, ,, 135 ,,	+ 10

VIRUS H 17. "Sp. B."—(Rabbit 181.)

The culture used was the thirteenth generation and was the first broth subculture.

The broth was not fermented and contained 2 per cent. glycerin.

Three weeks after inoculation there was an irregular unequally thick island which covered about a sixth of the surface; the island was for the most part opaque and white, the margins being thin and grey. Growth proceeded very slowly; on the forty-ninth day only about half the surface was covered, and on the eighty-fifth day not more than three-quarters; the whole surface was never completely covered; the pellicle, finally, was moderately thick opaque and stiff and had a wavy or wrinkled surface; the pellicle along the top of the waves, or ridges, was cracked.

Initial reaction	+ 17·5
Reaction after 49 days	+ 17·5
,, ,, 85 days	+ 19·25
,, ,, 109 days	+ 23·5

VIRUS H 17. "Sp. B."—(Rabbit 181.)

The culture was the nineteenth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

Growth from the outset was very slow; eighteen days after inoculation there was an opaque white island the size of a half-crown piece; this gradually extended and finally the whole surface was covered with a thick wrinkled slightly warty pellicle of a whitish colour with a faint yellow tinge.

Initial reaction	+ 20
Reaction after 71 days	+ 21

VIRUS H 17. "Sp. B." (Calf 555.)

The culture used was the twelfth generation and was the first broth subculture.

The broth was not fermented and contained 2 per cent. glycerin.

Fourteen days after inoculation the surface of the broth was completely covered with a thin grey membrane which climbed up the sides of the flask: the pellicle was not uniformly thin but showed slightly opaque greyish white patches. There was very little subsequent increase in the thickness of the pellicle which towards the end of the experiment broke up into islands and formed a small amount of deposit at the bottom of the flask.

Initial reaction	+ 17·5	
Reaction after 14 days	+ 11	
,, ,, 29 ,,	+ 6	
,, ,, 50 ,,	+ 6	The broth became faintly pink on the addition of phenol phthalein in the cold.
,, ,, 85 ,,	+ 7	

VIRUS H 17. "Sp. B." (Calf 555.)

The culture used was taken from broth and was the fifth generation.

The broth was fermented and contained 2 per cent. glycerin.

Nine days after inoculation there were a few small greyish white lace-like islands; in 23 days about half the surface was covered with small islands, majority greyish white with a glazed surface, but some grey and translucent; in 39 days the whole surface was covered; the pellicle was not uniform and consisted of opaque greyish-white patches joined together by a thin almost transparent membrane; the opaque patches later showed a scaly surface and upturned edges.

Initial reaction	+ 10
Reaction after 39 days	+ 5·25
,, ,, 57 ,,	+ 9
,, ,, 98 ,,	+ 8·5

VIRUS H 17. "Sp. B." (Calf 571.)

The culture used was taken from broth and was the fifth generation.

The broth was fermented and contained 2 per cent glycerin.

Twenty-three days after small very thin greyish lace-like islands were seen scattered over the surface: these increased in area and covered the surface at about the thirty-fifth day: the majority of the islands remained thin and grey, but a few thickened up slightly and became opaque greyish white.

Initial reaction - - - -	+10
Reaction after 56 days - -	+ 7
,, ,, 94 ,, - -	+ 4·75

VIRUS H 12. "H.N." (Calf 123.)

The culture used was the sixteenth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

Growth at first was very slow; at the end of the twenty-fourth day there was an opaque white slightly warty patch, the size of a shilling piece, dark-coloured around the margins and on the under surface. Examined on the fifty-second day the whole surface was found covered with a greyish white semi-opaque finely wrinkled pellicle in the midst of which there was a thick opaque dark-coloured warty patch; the pellicle subsequently became thicker, more wrinkled and creamy white in colour. A few days before the termination of the experiment the broth became slightly cloudy; no contaminating organisms were found in microscopical preparations of the pellicle and subcultures from the broth on serum remained sterile.

Initial reaction - - - - -	+17
Reaction after 54 days - - -	+20
(surface covered.)	
Reaction after 70 days - - -	+20·7
,, ,, 99 ,, - - -	+24·5
(broth cloudy.)	

VIRUS H 12. "H.N." (Calf 123.)

The culture used was the sixteenth generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

During the first five weeks there was merely a slight increase in thickness of the inoculated material. On the thirty-ninth day growth was observed around the margins of the small opaque island and 14 days later the whole surface was covered with an extremely thin almost transparent pellicle. The pellicle slowly thickened and became slightly wrinkled but remained translucent and of about the thickness of ordinary tissue paper. A few scattered opaque white colonies developed on the surface and attained the size of pins' heads.

Initial titration - - - - -	+15
Reaction after 54 days - - -	+15
(surface covered.)	
Reaction after 70 days - - -	+11·5
,, ,, 100 ,, - - -	+8
,, ,, 126 ,, - - -	+6·5

VIRUS H 14. "F.S." (Calf 125.)

The culture used was the thirteenth generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

Twenty-two days later the inoculated material had increased in area to that of a shilling piece; 6 days after a small grey island was seen besides the larger one; the two islands increased in area and thickness and on the forty-fifth day began to send out thin grey expansions from the margins; in 9 days the whole surface was nearly covered; the pellicle then was partly composed of thick opaque whitish patches, nodular and irregular on the surface and partly of a delicate grey membrane traversed by slender whitish curved threads; there was no subsequent increase noted.

Initial reaction - - - - -	+13
Reaction after 59 days - - -	+ 3·5
,, ,, 88 ,, - - -	+ 5·0

VIRUS H 15. "I.W." (Calf 311.)

The culture was the fifth generation and was taken from broth.

The broth was fermented and contained 5 per cent. glycerin.

In 27 days there was an irregular thick pellicle covering about a sixth of the surface; in 39 days three-quarters, and in 53 the whole of the surface was covered with a thick white growth which became very warty on the surface.

Initial reaction - - - -	+10
Reaation after 63 days - -	+ 7
,, ,, 73 ,, - -	+10·3
,, ,, 82 ,, - -	+11·3
,, ,, 98 ,, - -	+18·

VIRUS H 18. "T.T." (Calf 405.)

The culture used was the fourth generation and was taken from broth; the first three generations were grown on serum.

The broth was fermented and contained 5 per cent. glycerin.

Twenty-five days later there was an island measuring 2 cm. by 1 cm.; in 56 days there was a thick opaque creamy pellicle covering between a fifth and a quarter of the surface; in 75 days half the surface was covered, the growth now being very warty; on the eighty-sixth day three-quarters of the surface was covered and a thin grey membrane was spreading out from the margins; on the one hundred and fourth day the whole surface was practically covered; the thin part then thickened up, the pellicle finally being thick wrinkled and warty on the surface.

Initial reaction- - -	+ 15
Reaction after 104 days -	+ 17·6
,, ,, 124 ,, -	+ 28 broth (acid to litmus.)

VIRUS H 19. "S.W."—(Heifer 239.)

The culture used was the sixteenth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

A fortnight after inoculation there was a small opaque island, about the diameter of a sixpenny piece, unequal in thickness and very irregular on the surface. This very slowly increased in area and at the end of three months was about the size of a half-crown piece; it was very thick, opaque and greyish white and had a warty irregular surface. About this time whitish processes began to grow from one margin; these formed a network and when a small patch had formed became detached from the opaque island.

The network expanded very slowly and on the 198th day nearly the whole surface of the broth was covered with a lace-like network composed of moderately thick white trabeculæ; these trabeculæ were very brittle, the slightest jar of the flask causing pieces to break off and sink; eventually the whole surface was covered and the interstices of the network became filled in with an extremely thin grey membrane. In the meantime the original island gradually became moist around the edges and finally sank to the bottom of the fluid.

Initial reaction - - - - -	+ 17·5
Reaction after 198 days - - -	+ 23·5
,, ,, 203 ,, - - -	+ 23
,, ,, 276 ,, - - -	+ 22

VIRUS H 22. "F.W."—(Calf 293.)

The culture used was taken from broth and was the fifteenth generation, the first fourteen of which had been grown on pure serum.

The broth was not fermented and contained 2 per cent. glycerin.

Twenty-four days later there was an opaque white island measuring 1½ inch by 1 inch; this island slowly expanded and in about sixty days the whole surface was covered; the pellicle was partly thick opaque white and warty, partly thin greyish white and wrinkled; it subsequently increased in thickness and became more wrinkled, the warty part acquiring a creamy tinge.

Initial reaction - - - - -	+17
Reaction after 55 days - - -	+17·5
,, ,, 70 ,, - - -	+18
,, ,, 97 ,, - - -	+23

VIRUS H 23. "J.P."—(Calf 365.)

The culture used for the inoculation was taken from glycerin milk, and was the tenth generation; eight of the generations were grown on pure serum and one (the ninth) on glycerin broth.

The broth was fermented, and contained 2 per cent. glycerin.

In seventeen days there was a greyish-white island 1 inch in diameter; in twenty-eight days a third of the surface was covered with an opaque whitish granular pellicle, whilst the rest of the surface was incompletely covered with a thin grey membrane; the opaque part of the pellicle increased in thickness and in area, covering eventually about half the surface, and acquired a slight creamy tinge; the grey membrane became translucent, then broke up, parts sinking to the bottom of the fluid.

Initial titration - - - - -	+14
After 42 days - - - - -	+ 6·5
,, 66 ,, - - - - -	+ 9
,, 101 ,, - - - - -	+14
,, 115 ,, - - - - -	+13·5

VIRUS H 26. "K.M."—(Original material, through Guinea-Pig 1326.)

The culture used for the inoculation was taken from broth and was the fourteenth generation, the first thirteen of which had been grown on pure serum.

The broth was fermented and contained 2 per cent. glycerin.

Twenty-four days after inoculation there was a thin, greyish-white island, the size of a threepenny piece, and several small fragments; around the margins of these a delicate transparent growth developed, and on the fifty-second day the whole surface was covered with a thin grey translucent moist pellicle showing a few small opaque whitish patches; the pellicle increased very little in thickness, and became broken up into islands, many of which sank to the bottom of the fluid.

Initial reaction - - -	+15
Reaction after 55 days - - -	+14·75
,, ,, 74 ,, - - -	+ 9·75
,, ,, 91 ,, - - -	+ 7·5

VIRUS H 26. "K.M."—(Original material, through Guinea Pig 1326.)

This flask was sown from the same broth culture as the above.

The broth was not fermented and contained 2 per cent. glycerin; there was a small amount of muscle sugar present.

At the end of fourteen days there were two irregular opaque warty islands, with thin grey margins, covering about a fifth of the surface. In twenty-eight days five-sixths of the surface and in forty-two days the whole surface was covered; the pellicle was unequal in thickness, the greater part being creamy wrinkled and opaque, the rest greyish-white and translucent. The pellicle subsequently thickened slightly, the more opaque parts becoming distinctly warty.

Initial reaction - - -	+17·5
Reaction after 32 days - - -	+15·5
,, ,, 44 ,, - - -	+14
,, ,, 71 ,, - - -	+22
,, ,, 95 ,, - - -	+21·5

VIRUS H 27. "B.D."—(Original material, through Guinea-Pig 1216.)

The culture used for the inoculation was the twelfth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

In a fortnight a third, and in a month the whole surface was covered with a greyish translucent finely wrinkled growth containing here and there thicker opaque warty patches. The pellicle increased in thickness, but not uniformly, parts remaining thin grey and translucent. The pellicle finally became broken up into islands and there was a large amount of flaky deposit.

Initial reaction	+ 17·5
Reaction after 29 days	+ 19·5
,, ,, 50 ,,	+ 24
,, ,, 94 ,,	+ 25

VIRUS H 28. "C.L."—(Original material.)

The culture used was the tenth generation and was the first subculture on broth.

The broth was fermented and contained 2 per cent. glycerin.

In 35 days the whole surface was covered with a very delicate almost transparent film, showing two small thicker patches; fine whitish threads developed in the thin membrane, and the two opaque patches increased in thickness, one finally becoming yellowish and slightly wrinkled, the other remaining whitish.

Initial reaction	+14
Reaction after 52 days	+ 5·25
,, ,, 89 ,,	+ 1·5

VIRUS H 29. "M.F."—(Original material, through Guinea-Pig 1334.)

The culture used for the inoculation was taken from broth and was the thirteenth generation.

The broth was unfermented and contained 2 per cent. glycerin.

14 days after inoculation there was a whitish island, with thin grey margins, measuring about 2 by 1 in.; this island slowly increased in area and in 56 days covered about half the surface; the greater part was thick, creamy, irregular and warty on the surface; the margins were thin and greyish and traversed by wavy white lines. The thin grey marginal growth continued to extend but did not completely cover the surface until about 90 days after inoculation. The pellicle was finally composed partly of a thick opaque yellow very warty patch and partly of a greyish white translucent membrane traversed by thick yellow curved trabeculae standing up above the surface as prominent ridges.

Initial reaction	+17·5
Reaction after 65 days	+14
,, ,, 95 ,,	+14
,, ,, 109 ,,	+14

VIRUS H. 30. "E.M."—(Original material, through Guinea-Pig 1337.)

The culture used for the inoculation was taken from broth and was the ninth generation; the first eight generations were grown on pure serum.

The broth was fermented and contained 2 per cent. glycerin.

In 6 days there was a grey island about the size of a half-crown piece; this slowly increased in area and on the twenty-eighth day the whole surface was covered with a uniform moderately thick greyish-white wrinkled membrane which climbed up the sides of the flask. Subsequently the growth became thicker, more wrinkled and creamy white.

Initial reaction	+ 13
Reaction after 28 days (surface covered.)	+ 8
Reaction after 47 days	+ 14
,, ,, 60 ,,	+ 17·5
,, ,, 82 ,,	+ 18

VIRUS H. 31. "L.F."—(Original material.)

The culture used for the inoculation was the fourteenth generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

Three weeks after inoculation there was an opaque white wrinkled island, 2 in. by 1 in.; on the thirty-third day half the surface was covered with an opaque, moderately thick white growth, with a honeycombed surface; around the margins of this delicate grey growth appeared, which in a short time broke up and covered the rest of the surface with small islands; subsequently no increase in the thickness or the area of the pellicle was observed.

Initial reaction	+ 14
Reaction after 109 days	+ 6·5
,, ,, 145 ,,	+ 6·5

VIRUS H. 32. "Y.W."—(Original material—Mesenteric Gland.)

The culture used was the sixth generation and was the first broth subculture.

The broth was fermented and contained 5 per cent. glycerin.

In 23 days there was a thin grey layer containing opaque whitish patches covering half the surface; 6 days later the growth had extended over the whole surface; there was very little subsequent thickening of the pellicle.

Initial reaction	+13
Reaction after 31 days- (surface covered.)	+ 4·5
Reaction after 52 days-	+ 5·8
,, ,, 98 ,,	+ 7

VIRUS H. 34.—"C.U." (Original material, through Guinea-Pig 236.)

The culture used was the fourteenth generation and was the first broth subculture.

The broth was fermented, and contained 5 per cent. glycerin.

In 8 days the inoculated material had increased to about twice the size; in 19 days there was a thin grey membrane showing a few whitish foci, covering a little over a quarter of the surface; this continued to expand, and in 35 days the whole surface was covered; it was thin greyish and uniform, except for a few small whitish patches; subsequently no further increase in thickness was observed.

Initial reaction	+11·5
Reaction after 32 days (surface covered)	+ 9·5
„ „ 51 days	+ 9·5
„ „ 78 „	+10
„ „ 102 „	+12

VIRUS H. 34.—"C.U." (Original material, through Guinea-Pig 236.)

The culture used was the tenth generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

In 17 days the whole surface was covered; the pellicle was partly grey and translucent, with a whitish lace-like network, and partly greyish-white and opaque, with a finely granular surface; on the thirty-fifth day it was moderately thick white uniform and slightly wrinkled; subsequently the pellicle became slightly creamy, but there was no obvious increase in thickness.

Initial titration	+10
Reaction after 17 days (surface covered)	+ 5·5
„ „ 35 days	+ 7·75
„ „ 74 „	+ 9·5
„ „ 95 „	+ 9·3
„ „ 101 „	+ 8·75

VIRUS H. 36.—"M.D." (Original material.)

The culture used was the seventh generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

During 21 days no increase in the area of the inoculated material was observed; on the thirty-sixth day there was a greyish-white patch, with a warty surface, the size of a shilling; this slowly increased in area and on the seventieth day there was a pellicle covering three-quarters of the surface; the pellicle was partly whitish and opaque and partly grey and translucent; on the seventy-eighth day the whole surface was covered; the growth then became thicker and on the hundred-and-sixth day was thick uniform creamy white and smooth on the surface.

Initial reaction	+19·5
Reaction after 70 days	+15
„ „ 106 „	+13
„ „ 117 „	+17
„ „ 138 „	+20·5
„ „ 152 „	+27

VIRUS H 37.—"O.J." (Original material, through Guinea-Pig 1427.)

The culture used was taken from broth and was the fifteenth generation, fourteen of which had been grown on pure serum.

The broth was fermented and contained 2 per cent. glycerin.

During the first 28 days there was no growth; on the forty-third day slight increase in thickness and area of the inoculated material had occurred; on the fifty-sixth day there was a small opaque white island with a delicate, almost transparent margin; fourteen days later the surface was covered with a very thin grey moist translucent pellicle; this thickened slightly and broke up into islands, which showed a marked tendency to become moist at the edges and sink.

Initial reaction	+15
Reaction after 67 days	+12
„ „ 91 „	+10

VIRUS H. 37. "O.J."—(Original material, through Guinea-Pig 1427.)

This flask was sown from the same broth culture as the above.

The broth was not fermented and contained 2 per cent. glycerin and a small amount of muscle sugar.

In fourteen days there was a creamy yellow warty island covering about one-fifth of the surface. In about 30 days the whole surface was covered; the pellicle was for the most part thick creamy-yellow wrinkled and warty, but parts were quite thin greyish-white and translucent; there was slight subsequent increase, the pellicle remaining unequal in thickness.

Initial reaction	+17.5
Reaction after 29 days	+13
„ „ 44 „	+16
„ „ 71 „	+19
„ „ 95 „	+20·5

VIRUS H 38. "J.M."—(Original material, through Guinea-Pig 793.)

The culture used for the inoculation was the eighteenth generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

Ten days after inoculation there were two small grey islands, each the size of a threepenny bit and several small fragments; 14 days later the surface was almost covered with an extremely thin film surrounding a few small opaque whitish patches. The pellicle thickened slightly and on the thirty-ninth day was moist, greyish white and translucent (resembling tissue paper). Cracks then appeared, the edges became moist and on the sixty-eighth day most of the growth had sunk to the bottom.

Initial reaction	+15
Reaction after 39 days	+10
„ „ 46 „	+ 9
„ „ 68	+ 7

VIRUS H. 39. "M.B."—(Original material.)

The culture used was the thirteenth generation and was the first broth subculture.

The broth was not fermented and contained 2 per cent glycerin.

In 17 days there was a thick milky white island measuring about 2 in. by ½ in.; this slowly increased in area and from the margins a thin grey membrane began to spread; at the end of 45 days the whole surface was covered, a fifth of the pellicle being opaque white and warty, whilst the rest was thin grey and translucent; the pellicle increased to a moderate thickness and was finally uniform greyish white and very wrinkled.

Initial reaction - - - -	+17
Reaction after 48 days - - -	+17
,, ,, 63 ,, - - -	+22
,, ,, 92 ,, - - -	+22

VIRUS H 41. "A.S."—(Original material, through Guinea-Pig 1435.)

The culture used was the eleventh generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

At the end of 17 days there was an irregular opaque white island, with thin grey margins, measuring 2in. by 1in.; in 40 days the whole surface was covered; the pellicle was not uniform, being partly thick, opaque and white and partly thin grey and translucent; it subsequently increased in thickness and was finally uniformly thick creamy white and wrinkled.

Initial reaction - - - -	+17
Reaction after 39 days - - -	+15·5
,, ,, 61 ,, - - -	+19·5
,, ,, 90 ,, - - -	+22

VIRUS H 42. "M.R."—(Original material, through Guinea-Pig 1447.)

The culture used was taken from broth and was the seventh generation; the first six of which were grown on pure serum.

The broth was fermented and contained 2 per cent. glycerin.

In 10 days there was a greyish patch covering about a third of the surface; in 17 days the whole surface was covered with a uniform greyish white pellicle; this became wrinkled and a little more opaque, but did not climb up the sides of the flask. After the thirty-fifth day no change in the character of the pellicle was observed except that towards the end of the experiment the surface had become moist and shiny.

Initial reaction - - - -	+ 14
Reaction after 17 days (surface covered.) - - -	+ 6·5
Reaction after 30 days - - -	+ 9
,, ,, 52 ,, - - -	+ 13
,, ,, 67 ,, - - -	+ 13·75
,, ,, 106 ,, - - -	+ 13

VIRUS H 44. "D.C."—(Original material.)

The culture used was the thirteenth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

In three weeks three-quarters of the surface were covered with a pellicle most of which was opaque, white and slightly warty whilst parts were quite thin and grey. In 4 weeks the whole surface was covered and the growth climbed up the sides of the flask. The pellicle was finally thick creamy wrinkled and very warty. After the last titration but one the broth became cloudy; no organisms were found in microscopical preparations.

Initial reaction - - - -	+ 17
Reaction after 23 days - - -	+ 21
,, ,, 39 ,, - - -	+ 25
,, ,, 52 ,, - - -	+ 24

VIRUS H 45. "F.M."—(Original material, through Guinea-Pig 1137.)

The culture was taken from broth and was the seventh generation.

Three flasks of fermented broth were sown; two of the flasks contained 2 per cent. and 5 per cent. glycerin respectively; the third was without glycerin.

On the latter no growth was obtained.

On the 2 per cent. glycerin broth during the first 16 days there was only very slight growth; 8 days later there was a pellicle covering a third of the surface, and on the thirty-first day the whole surface was covered with a greyish white uniform wrinkled growth which spread up the sides of the flask; the pellicle subsequently became much thicker and acquired a creamy yellow colour.

Initial reaction - - - -	+11
Reaction after 24 days - - -	+10
,, ,, 31 ,, - - -	+ 7
,, ,, 67 ,, - - -	+10·5
,, ,, 77 ,, - - -	+16·5
,, ,, 88 ,, - - -	+20
,, ,, 102 ,, - - -	+18

VIRUS H 45. "F.M." (Original material, through Guinea-Pig 1137.)

On 5 per cent. glycerin broth growth was much slower than on the 2 per cent., at the end of 57 days there was an island measuring only 2½ in. by 1 in.; the island then began to enlarge, and on the sixty-seventh day four-fifths of the surface was covered; the pellicle was unequally thick, some parts being quite filmy; the whole surface was speedily covered and the pellicle became thick wrinkled and creamy, but did not climb up the sides of the flask.

Initial reaction - - - -	+11
Reaction after 67 days - - -	+ 7
,, ,, 77 ,, - - -	+ 7·5
,, ,, 88 ,, - - -	+10
,, ,, 102 ,, - - -	+16
,, ,, 120 ,, - - -	+28

VIRUS H 48. "W.P."—(Original material, through Guinea-Pig 1494.)

The culture used was the fourth generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glycerin.

Twenty days later there were three opaque-white warty islands which by the 28th day covered about a sixth of the surface and showed thin grey marginal growth; the latter slowly extended and covered the surface in about 42 days; the complete pellicle consisted of three opaque white warty islands surrounded by a thin grey membrane showing fine whitish threads radiating out from the opaque patches; the whole pellicle thickened, the membranous part becoming opaque, whitish and finely wrinkled.

Initial reaction				- +10
Reaction after 35 days				- + 6·75
,,	,, 71	,,		- + 8
,,	,, 88	,,		- +12
,,	,, 101	,,		- +10
,,	,, 120	,,		- +11

VIRUS H 49. "T.C."—(Original material.)

The culture used was the tenth generation and was the first broth subculture.

The broth was not fermented, and contained 2 per cent. glycerin. A fortnight after inoculation there was a small irregular grey island with opaque whitish thickenings.

On the twenty-eighth day half the surface was covered, the pellicle was opaque whitish and of medium thickness, with parts quite thin and grey.

Growth continued, and appeared to cease about the eighth week; the surface was not completely covered.

The pellicle finally was opaque, rather broken up and of medium, but not uniform thickness; the surface was wavy, moist, and scaly looking.

Initial reaction				- +13
Reaction after 36 days				- +11
,,	,, 56	,,		- + 9·5
,,	,, 77	,,		- +10·5
,,	,, 91	,,		- +10·5
,,	,, 105	,,		- +11

VIRUS H 49. "T.C."—(Original material.)

The culture used for the inoculation was the tenth generation, and was the first broth subculture.

The broth was not fermented, and contained 2 per cent. glycerin.

Fourteen days after inoculation there were three or four small grey islands with whitish thickenings. On the twenty-eighth day three-quarters of the surface was covered with a somewhat broken-up pellicle, not uniformly thick; the greater part was opaque and greyish-white, but parts were quite thin and translucent.

In five weeks the whole surface was covered, but the growth did not climb up the sides of the flask; the pellicle was broken up, and was wavy and parchment-like; the space between the opaque parts was filled with thin, grey growth. The pellicle showed no further increase in thickness, but opaque raised colonies developed on the surface which attained the diameter of a pin's head

Initial reaction				- +13
Reaction after 35 days				- +11
,,	,, 63	,,		- +11
,,	,, 112	,,		- +11

VIRUS H 50. "P.H."—(Original material, through Guinea-Pig 1490.)

The culture used was the fifth generation and was the first broth subculture.

The broth was fermented, and contained 2 per cent. glycerin.

In 12 days there were several greyish-white speckled islands; in 20 days the whole surface was covered with a complete white pellicle; it was grey, with an opaque meshwork of whitish lines, mostly radiating out from slightly raised foci; the pellicle increased to a moderate thickness and was creamy-white and very wrinkled.

Initial reaction				- +10
Reaction after 20 days				- + 6·75
,,	,, 35	,,		- + 8
,,	,, 71	,,		- + 8
,,	,, 88	,,		- +11
,,	,, 101	,,		- +10·5
,,	,, 120	,,		- +12

VIRUS H 51. "H.M."—(Original material.)

The culture used was the seventh generation, the last two of which were grown on broth.

The broth was unfermented and contained 2 per cent. glycerin.

Growth was at first rather slow; at the end of 38 days there was a thick warty island, measuring 3 inches by a little over 1 inch in area; growth was then more rapid and on the sixtieth day the whole surface was covered with a thick opaque greyish-white, or slightly creamy pellicle with a honey-combed or pitted surface.

Initial reaction				- + 17
Reaction after 66 days				- + 20·5
,,	,, 74	,,		- + 22·5

VIRUS H 52. "T.F."—(Original material, through Guinea-Pig 1567.)

The culture used was the fourth generation and was taken from broth; the first three generations were grown on pure serum.

The broth was not fermented and contained 2 per cent. glycerin.

In 17 days a fifth, and in thirty days the whole of the surface was covered with a uniform greyish-white pellicle which climbed up the sides of the flask. The pellicle increased to a moderate thickness, becoming wrinkled and slightly creamy; there was no increase after the forty-fifth day.

Initial titration				- +17
Reaction after 33 days				- +16
,,	,, 60	,,		- +20
,,	,, 84	,,		- +18

VIRUS H 53. "D.H."—(Original material, through Guinea-Pig 1482.)

The culture used for the inoculation was taken from the surface of glycerin milk and was the twelfth generation.

The broth was not fermented, and contained 2 per cent. glycerin.

A fortnight after the inoculation the whole surface was covered with a thin, translucent, grey pellicle, of about the thickness of tissue paper; it was finely wrinkled, and climbed up the sides of the flask. A month after the inoculation the pellicle was found to have become thicker and whiter in patches, the rest of the membrane remaining thin and translucent. Subsequently there was no further growth, and the pellicle broke up into islands, some of which sank; the experiment was terminated on the 56th day on account of a small mould colony appearing in the depth of the broth.

Initial titration	+ 20
Reaction after 14 days	+ 13
„ „ 28 „	+ 9·5
„ „ 56 „	+ 8·5

(This experiment is not included in the tabular summary of results.)

VIRUS H 54. "C.W."—(Original material, through Guinea-Pig 1609.)

The culture used was the sixth generation and was the first broth subculture.

The broth was not fermented and contained 2 per cent. glycerin.

Growth took place from the outset, but was very slow. At the end of a month about a quarter of the surface was covered with an opaque creamy warty patch. At the end of 6 weeks two-thirds and at the end of 8 weeks practically the whole of the surface was covered. The pellicle was for the most part thick, creamy and warty, but parts, chiefly around the margins, were thinner, greyish white, and more wrinkled.

Initial reaction	+ 13
Reaction after 56 days	+ 12
„ „ 77 days	+ 12·5
„ „ 105 days	+ 15

VIRUS H 55. "R.D."—(Original material—Mesenteric Gland.)

The culture used was the fifth generation and was the first broth subculture.

The broth was not fermented and contained 2 per cent. glycerin.

Seven days after inoculation there was a small grey island speckled with opaque white foci. This slowly expanded, and in three weeks half the surface was covered. At the end of 5 weeks the whole surface was covered, and the growth had begun to climb up the sides of the flask. In the seventh week there was a moderately thick, greyish-white, slightly wrinkled membrane showing minute raised white foci scattered over the surface; at one part there was a small warty patch. There was no subsequent increase in the thickness of the pellicle, but the surface became studded with raised opaque colonies.

Initial reaction	+ 13
Reaction after 29 days	+ 11
„ „ 49 days	+ 10
„ „ 98 days	+ 12
„ „ 191	+ 11·5

VIRUS H 55. "R.D."—(Original material—Bronchial Gland.)

The culture used was the fifth generation and was the first broth subculture.

The broth was not fermented and contained 2 per cent. glycerin.

Seven days after inoculation there was an irregular moist whitish island, the size of a shilling. The island slowly increased in area, and at the end of 5 weeks covered about two-thirds of the surface; the growth was moderately thick, opaque, and in parts quite warty. The growth gradually extended, and the surface became almost, though not completely, covered; the pellicle was not uniform in thickness, parts being denser and more warty than others; at the margin there was a small patch, which was quite thin and grey. Subsequently there was a slight general increase in thickness of the pellicle, which acquired a creamy tinge.

Initial reaction	+ 13
Reaction after 34 days	+ 11·75
„ „ 49 days	+ 9·5
„ „ 77 days	+ 13·5
„ „ 98 days	+ 13·5
„ „ 119 days	+ 13·5

VIRUS H 56. "F.T."—(Original material, through Guinea-Pig 1682.)

The culture used was the eleventh generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

Thirty-two days after inoculation there was an opaque greyish-white moderately thick pellicle covering nearly the whole surface. The pellicle soon covered the surface and then began to climb up the sides of the flask; the final growth was thick, creamy-yellow and very wrinkled.

Initial reaction	+ 20
Reaction after 85 days	+ 21

VIRUS H 56. "F.T."—(Original material, through Guinea-Pig 1682.)

This flask was sown at the same time as the above and with the same culture.

The same broth was used but without the addition of glycerin.

Thirty-two days after inoculation there was an irregular island not larger than a two-shilling piece; it was unequal in thickness, the thicker parts having a reticulated surface. There was no further increase in the area of the pellicle.

Initial reaction	+ 20	
Reaction after 85 days	+ 12	The broth became pink on the addition of phenol-phthalein in the cold.

VIRUS H 57. 'B.J."

The culture used was the ninth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

Forty days after inoculation the whole surface was covered with an opaque, greyish, finely granular pellicle, with a slightly yellow tinge; the pellicle gradually thickened and became creamy in colour and very warty, but not wrinkled.

Initial reaction - - - - -	+ 17
Reaction after 93 days - - - -	+ 30

VIRUS H 61. "E.C."—(Original material—Mesenteric Gland.)

The culture used was the fifth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glycerin.

Thirty-two days later about a third of the surface was covered with moderately thick opaque whitish islands, with a reticulated surface. These slowly increased in area but never completely covered the surface.

Initial reaction - - - - -	+ 20
Reaction after 85 days - - - -	+ 21

ADDITIONAL OBSERVATIONS ON THE CHANGES IN REACTION PRODUCED IN GLYCERIN BROTH BY HUMAN AND BOVINE TUBERCLE BACILLI.

After the experiments detailed above were completed an additional series of experiments was undertaken with the object of controlling the previous results.

Twelve strains of bovine origin and sixteen strains of human origin, of which five were virulent for the calf and rabbit, have been investigated.

In order to have the experimental conditions in this series perfectly uniform throughout, the same batch of broth was used; chemically pure glycerin was added to it in the proportion of 3 per cent.

The broth proved to be an extremely favourable medium, and all the cultures grew with great vigour, some of the virulent strains producing pellicles which were almost equal to, and in many cases apparently greater in thickness than, those produced by some of the slightly virulent strains.

After sowing, the flasks were not disturbed until at least two months had elapsed; samples were then withdrawn and tested. In four instances the growths produced by bovine bacilli had, before the end of six weeks, sunk to the bottom of the fluid.

In each case the final amount of growth was estimated volumetrically.

The results have been tabulated and arranged in a numerical series beginning with the strain which reduced the acidity to the greatest extent, and ending with the ones which produced the greatest amount of acid.

The series clearly shows that different strains of tubercle bacilli cannot be differentiated into two classes by means of this test; as in the first experiments, the differences between the initial and final reactions form an unbroken series, in which there is no gap which would justify such a distinction.

With only six strains of tubercle bacilli was the reaction of the broth finally more acid than at the start; this increase varied from +0·5 to +8; the strains causing this increase in acidity have only slight virulence for the calf and rabbit.

In all the other cases the final acidity was less than the initial, but in no case did the broth become neutral to phenol-phthalein, the difference between the initial and final reactions varying from 1 to 9·5. Five of these strains have been shown to possess only slight virulence for the calf and rabbit, whilst the rest are highly virulent.

It is therefore not even possible to draw a line at the point where the initial and final reactions are the same, and say that all the strains which reduce the acidity in any degree are virulent, and that all the strains which increase the acidity are slightly virulent.

It may next be enquired how far the change in reaction has accorded with the amount of growth.

An inspection of the column which gives the volumetric estimations shows that the amount of growth produced by the three strains which caused the greatest reduction in acidity is much less than that produced by the three which formed the most acid. But with these the correspondence ceases; in fact the remaining estimations seem to show that the majority of the more virulent bacilli are incapable of producing with the same or an even greater bulk of growth as much acid as the less virulent bacilli.*

The questions therefore arise—do these differences indicate an essential difference in biochemical action, or are they differences in degree only?

In the case of the slightly virulent organism, it seems quite clear that two chemical processes are at work in the broth—one a reduction in acidity which predominates at first, the other a production of acid which becomes more and more marked as the experiment continues, and which can be referred to some splitting up of the glycerin.

The action of the virulent tubercle bacillus is not so readily explained; Theobald Smith is of the opinion that bovine tubercle bacilli, although they utilise glycerin, do not form acid out of it, and are capable, therefore, of producing an alkaline change only in the medium. If this be so, the change in the alkaline direction ought to bear some proportion to the amount of growth, and to be greater when the growth is luxuriant than when it is scanty, as it is when there is no glycerin in the broth, or when glucose is substituted for glycerin. On examining the list it will be noticed that the diminution in acidity brought about by the virulent tubercle bacilli is in all cases comparatively a slight one, and is in many cases less with large amounts of growth than it is with small ones.

It is difficult to explain these results except on the hypothesis that the virulent tubercle bacilli act on, and produce acid out of, the glycerin in the same way as, though to a less marked extent than the slightly virulent bacillus.

The more saprophytic bacilli no doubt from the outset attack and decompose the glycerin with the formation of acid more readily and in greater degree than the less saprophytic organisms, and it is for this reason that the initial diminution in acidity is never great with the slightly virulent bacillus.

Although the majority of the virulent strains have reduced the acidity of the broth to a greater extent than the slightly virulent, yet it is possible to select out of the series virulent strains and slightly virulent strains which have yielded identical results (*e.g.*, Virus B. XI., and H. 16 J.H. Calf 273, H. 54 C.W. and H. 38 J.M.) or strains in which the differences are so slight as to be negligible, (*e.g.*, Virus B. XI. and H. 50 P.H.).

The differences observed, therefore, in the action of different strains of tubercle bacilli on glycerin broth are, in my opinion, differences in degree and not in kind, and are attributable to variations in saprophytic power which have been shown to exist on other media.

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* It must be pointed out, in connection with the amount of growth, that the volumetric estimations have not in all cases given results quite consistent with the naked eye characters of the growth, *e. g.*, Virus B. XIV. described as being thin, grey and translucent yielded a greater volume of growth than H. 52, T.F., which was moderately thick and creamy-white, and other examples could be found.

TABLE V.

SUMMARY OF THE RESULTS OF THE ADDITIONAL EXPERIMENTS WITH GLYCERIN BROTH.

Virus.	Generation.	Duration of Experiment.	Final Reaction.	Difference between Initial and Final Reaction.	Volume of Growth.	Character of Final Growth.
Virus B IX. (direct)	33rd	64 days	+ 5·5	−9·5	2·1 cg.	Thin grey translucent membrane which sank in the third week.
Virus B VIII. (direct)	34th	64 „	+ 6·5	−8·5	2·6 „	Thin greyish-white membrane which at the end of five weeks had sunk to the bottom of the broth.
Virus B V (G.-P. 711)	25th	64 „	+ 7·	−8·	2·4 „	Greyish-white finely-wrinkled membrane which began to sink soon after it had covered the surface.
Virus B XIV. (direct)	28th	64 „	+ 8·	−7·	3·4 „	Thin grey translucent membrane which soon became moist and sank.
Virus B IV. (direct)	47th	64 „	+ 9·	−6·	3· „	Somewhat opaque greyish white reticulated membrane.
H 49. "T. C." (Calf 787)	12th	71 „	+10·	−5·	3·35 „	Moist greyish-white somewhat translucent membrane of medium thickness.
Virus B V. (G.-P. 711)	25th	64 „	+10·5	−4·5	2·55 „	Moist greyish-white pellicle with a somewhat granular surface.
Virus B I. (Cow 44)	36th	84 „	+10·5	−4·5	3·3 „	Moderately thick opaque whitish pellicle.
H 32. "Y. W." (direct—mesenteric glands)	24th	85 „	+10·5	−4·5	3·1 „	Opaque white moist pellicle.
Virus B IV. (Calf 118)	32nd	84 „	+11·	−4·	3·2 „	Opaque white pellicle of moderate thickness with a slightly warty surface.
Virus B II. (Calf 134)	—	67 „	+11	−4·	2·7 „	Moist opaque white pellicle, not very thick.
H 49. "T. C." (Calf 797)	13th	82 „	+11	−4·	2·2 „	Greyish translucent slightly wrinkled membrane.
Virus B X. (G.-P. 1070)	30th	64 „	+11·5	−3·5	3·0 „	Greyish-white slightly wrinkled membrane containing some opaque white patches.
Virus B XI. (Rabbit 66)	22nd	84 „	+12·	−3·	3·5 „	Moderately thick opaque white moist pellicle.
H 16. "J. H." (Calf 273)	31st	79 „	+12·	−3·	3·2 „	Greyish somewhat translucent wrinkled membrane with a yellowish tinge.
H 49. "T. C." (direct)	19th	74 „	+12·5	−2·5	2·3 „	Grey translucent membrane, closely studded with raised opaque white colonies.
Virus B XI. (Rabbit 66)	27th	84 „	+13·	−2·	3·3 „	Moderately thick opaque white moist pellicle.
H 17. "Sp. B." (Rabbit 181)	22nd	64 „	+13·5	−1·5	2·5 „	Pellicle partly thin grey translucent and wrinkled, partly opaque and granular.
H 54. "C. W." (G.-P. 1609)	15th	66 „	+13·5	−1·5	2·65 „	Pellicle composed of small granules which readily separated from each other on shaking the flask.
H 46. "H. W." (G.-P. 1444)	20th	64 „	+14·	−1·	2· „	Greyish-white somewhat translucent membrane with innumerable raised pearly granules on the surface.
H 50. "P. H." (G.-P. 1490)	20th	64 „	+14	−1·	3·2 „	Opaque white wrinkled membrane with a slight creamy tinge.
H 38. "J. M." (G.-P. 798)	28th	71 „	+14·	−1·	2·75 „	Moderately thick opaque white slightly warty membrane.
H 2. "Sp. A." (Calf 111)	26th	68 „	+15·5	+0·5	3·7 „	Pellicle partly uniform and white, partly creamy wrinkled and warty.
H 52. "T. F." (G.-P. 1567)	25th	71 „	+16·	+1·	2·8 „	Moderately thick creamy-white membrane.
H 58. "F. G." (direct)	7th	74 „	+16·	+1·	2·65 „	Opaque white membrane partly creamy and warty.
H 57. "B. J."	10th	71 „	+18·2	+3·2	6·35 „	Thick creamy wrinkled and warty pellicle.
H 17. "Sp. B." (Calf 265)	24th	66 „	+23·	+8·	3·5 „	Moderately thick greyish-white membrane, parts of which were warty.
H 45. "F. M." (G.-P. 1137)	30th	85 „	+23·	+8·	4· „	Thick creamy-white wrinkled pellicle.

THE CHANGES IN REACTION PRODUCED IN GLUCOSE BROTH BY HUMAN AND BOVINE TUBERCLE BACILLI.

Eleven strains of tubercle bacilli, two of bovine origin and nine of human origin, have been tested on broth containing 1 to 2 per cent. glucose; two of the strains of human origin were virulent, six were slightly virulent, and one (H 53 "D.H.") had intermediate virulence for bovines.

The method of testing the change in reaction has been the same as that employed for the glycerin broth.

Glucose is a very satisfactory substitute for glycerin; all the cultures have grown well and the pellicles formed have been similar in character to those on glycerin broth; in the case of H. 13 "A.D." Rat 15, a strain which grows with great difficulty on glycerin media, a very luxuriant growth was obtained, almost equal to the best obtained with slightly virulent bacilli.

Four different samples of broth were used, with an initial reaction varying from +7 to +22.

The results of the tests are given in Table VI.

In all cases the acidity of the broth was gradually diminished, the virulent strains reacting in precisely the same way as the slightly virulent; the greatest diminution in acidity occurred with those strains which produced the most luxuriant growths.

These experiments with glucose broth, therefore, do not afford results which would justify the division of tubercle bacilli into two classes.

TABLE VI.

SUMMARY OF THE RESULTS ON GLUCOSE BROTH.

Virus.	Initial reaction of broth.	Lowest recorded reaction.	Final reaction.	Difference between initial and final reaction.	Character of Pellicle.	Amount of growth in cubic centimetres.
Virus B V. G-P. 711 - -	+7	+ 2·5	+3	−4	Thin grey and translucent	
Virus B XIV. direct - -	+7	+ 3	+3	−4	Thin grey and translucent	
H. 45. "F.M." G-P. 1137 -	+7	+ 1	+1	−6	Opaque creamy-white and wrinkled	
H. 43 "F.F." direct - -	+7	+ 2	+2	−5	Creamy-white pellicle, surface not completely covered	
H. 11. "E.D." Calf 221 -	+10	+ 5·5	+5·5	−4·5	Partly creamy, partly translucent and greyish white	2·3
H. 34. "C.U." G-P. 236 -	+10	+ 5	+5	−5	Slightly wrinkled moderately thick	1·9
H. 2. "Sp. A." Calf 83 -	+12	+ 6	+6	−6	Partly thick and creamy, partly thin and grey	
H. 17 "Sp. B." Calf 265 -	+12	+ 4·5	+4·5	−7·5	Creamy white and moderately thick	
H. 46. "H.W." G.P. 1444 -	+22	+13·5	12	−10	Very thick wrinkled and yellow	3·7
H. 13. "A.D." Rat 15 - -	+22	+14	+14	−8	More than half thick wrinkled and creamy; rest thin grey and translucent	3·1
H. 53. "D.H." G-P. 1482 -	+22	+11	+11	−11	Greyish-white wrinkled membrane with a slight creamy tinge.	3·0

DETAILED DESCRIPTION OF THE EXPERIMENTS WITH GLUCOSE BROTH.

VIRUS H 11. "E.D."—(Calf 221, through Guinea-Pig 918.)

The culture used for the inoculation was the twelfth generation, and was the first broth subculture.

The broth was fermented, and contained 1 per cent. glucose.

Sixteen days after inoculation three-fourths of the surface were covered with a pellicle, unequal in thickness and broken up into islands. About a fortnight later the whole surface was covered; the growth was for the most part opaque, creamy yellow, and very warty; the rest was thinner and greyish white. Subsequently no change in the character of the pellicle was noted.

Initial reaction				-	+10
Reaction after	19	days		-	+ 6
,, ,,	32	,,		-	+ 7
,, ,,	70	,,		-	+ 5·5
,, ,,	102	,,		-	+ 5·5

The broth in the cold became deeply pink on the addition of phenol phthalein, the colour being more pronounced than in the case of H 34. C.U.

VIRUS H 2. "Sp. A."—(Calf 83, through Guinea-Pig 618.)

The culture used for the inoculation was the first broth subculture.

The broth was fermented, and contained 1 per cent. glucose.

Growth at first was slow; 18 days after inoculation there was an opaque whitish warty nodule, 5 m.m. in diameter; this fourteen days later measured 1 c.m. in diameter, and was thick, opaque and showed stalactitic processes on the under surface.

The island slowly expanded, and on the forty-fifth day a third of the surface was covered with an opaque, creamy-white patch with a honey-combed surface; around the free margin of this patch delicate grey growth began to form; this spread over the surface to some extent, then broke up into small islands; growth then ceased.

Initial reaction				-	+12
Reaction after	50	days		-	+ 6
,, ,,	91	,,		-	+ 6·5
,, ,,	99	,,		-	+ 6

The broth gave a deep pink colouration with phenol phthalein in the cold.

VIRUS H 17. "Sp. B."—(Calf 265.)

The culture used was the thirteenth generation and was the first broth subculture.

The broth was fermented and contained 2 per cent. glucose.

In 9 days the whole surface was covered with a thin grey membrane traversed by fine wavy lines. The membrane increased rapidly in thickness and climbed up the sides of the flask. Five weeks after inoculation it was moderately thick, opaque, uniform and wrinkled, and creamy white in colour. There was very little subsequent increase in thickness and finally all the growth sank to the bottom of the fluid.

Initial reaction			-	-	-	- + 12
Reaction after	14	days	-	-	-	- + 10
,, ,,	35	,,	-	-	-	- + 8·2
,, ,,	55	,,	-	-	-	- + 7
,, ,,	84	,,	-	-	-	- + 4·5

VIRUS H 34 "C.U."—(Original material, through Guinea-Pig 236.)

The culture used was the tenth generation and was the first broth subculture.

The broth was fermented and contained 1 per cent. glucose.

In a fortnight the whole surface of the broth was covered with a uniform thin greyish-white pellicle speckled with opaque, white foci. The pellicle increased to a moderate thickness, and 5 weeks after inoculation was opaque white, or faintly creamy, and slightly wrinkled.

Later the pellicle broke up into islands and some of the growth sank.

Initial reaction			-	-	-+	10
Reaction after	17	days	-		-+	5·5
,, ,,	33	,,	-		-+	6
,, ,,	72	,,	-		-+	5·5
,, ,,	103	,,	-		-+	5

The broth before boiling gave a deep pink colour with phenol phthalein.

VIRUS B. V.—(Original material, through Guinea-Pig 711.)

The culture used was the eighth generation, the last two of which had been grown on broth.

The broth was fermented and contained 1·25 per cent. glucose.

Growth took place from the outset, and in 16 days the whole surface was covered with a thin grey membrane, traversed by many fine wavy lines. The membrane became finely wrinkled but increased very little in thickness; the wavy lines, however, grew thicker, whiter, and more opaque.

Initial reaction			-	-	-	- + 7
Reaction after	16	days	-	-	-	- + 6
,, ,,	32	,,	-	-	-	- + 2·5
,, ,,	78	,,	-	-	-	- + 3

VIRUS B. XIV.—(Original material, through Guinea-Pig 1271.)

The culture used was the fifth generation and was the first broth subculture.

The broth was fermented and contained 1·25 per cent glucose.

In 3 weeks the whole surface was covered with a thin grey uniform lace-like membrane, dotted over the surface of which were numerous small white foci.

The membrane thickened slightly, and was finally translucent, finely wrinkled, and dotted on the surface with minute whitish foci.

Initial reaction	- - - -	+ 7
Reaction after 21 days	- - -	+ 7
„ „ 42 „	- - -	+ 4·5
„ „ 78 „	- - -	+ 3

H 45. "F.M."—(Original material, through Guinea-Pig 1137.)

The culture used was the eighteenth generation and was the first broth subculture.

The broth was fermented and contained 1·25 per cent. glucose.

Growth took place from the outset, and in a fortnight the whole surface was covered with a thin grey uniform membrane; this rapidly increased in thickness, and at the end of three weeks was opaque, creamy white, and wrinkled.

Initial titration	- - - - -	+7
Reaction after 15 days	- - -	+9
„ „ 21 days	- - -	+6·5
„ „ 38 days	- - -	+5·5
„ „ 62 days	- - -	+1
„ „ 79 days	- - -	+1

H 43. "F.F."—(Original Material.)

The culture used was the sixth generation and was the first broth subculture.

The broth was fermented, and contained 1·25 per cent. glucose.

Eleven days after inoculation there was a whitish island the size of a two-shilling piece; this gradually extended, and in 8 weeks about three-quarters of the surface was covered. The pellicle was greyish-white and was partly wrinkled, and partly smooth and studded with raised nodules. The pellicle subsequently became thicker and acquired a creamy colour, but the surface was not completely covered; the nodules increased in size, some attaining a diameter of 2 and 3 millimetres.

Initial reaction	- - - - -	+7
Reaction after 53 days	- - -	+5·3
„ „ 66 days	- - -	+3·5
„ „ 79 days	- - -	+2

H 46. "H.W." (Original material, through Guinea-Pig 1444).

The culture used for the inoculation was taken from milk and was the twelfth generation.

The broth was not fermented and contained 2 per cent. glucose.

Three weeks after the inoculation half the surface was covered with a moderately thick greyish-white wrinkled pellicle, with a thicker yellowish patch in the centre; a week later the whole surface was covered and the growth had a slightly creamy tinge. The pellicle was finally thick, very wrinkled, and deep yellow in colour.

Initial reaction	- - - - -	+22
Reaction after 28 days	- - -	+19
„ „ 42 days	- - -	+13·5
„ „ 70 days	- - -	+12·5
„ „ 99 days	- - -	+12

H 13. "A.D."—(Rat 15.)

The culture used was the sixteenth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glucose.

Twenty days later there was a small patch consisting of raised opaque yellow nodules. The patch increased slowly in area and the nodules became larger; in the meantime on the sixty-first day a grey marginal film began to form; this gradually extended and at last the whole surface was covered. At the end of the experiment more than half of the pellicle was thick wrinkled and creamy, whilst the rest was thin grey and translucent and showed scattered raised foci.

Initial reaction	- - - - -	+22
Reaction after 69 days	- - -	+21
„ „ 154 days	- - -	+14

H 53. "D.H." (Original material, through Guinea-Pig 1482).

The culture used was the twelfth generation and was the first broth subculture.

The broth was unfermented and contained 2 per cent. glucose.

Growth was very slow. On the sixty-first day there were three small yellow nodules, one of which was surrounded by a grey film. One hundred and one days after inoculation the whole surface was covered with a grey translucent finely wrinkled membrane showing in the centre a yellow warty patch. The membrane increased in thickness and become more wrinkled, the summits of the wrinkles having a yellowish tinge.

Initial reaction	- - - - -	+22
Reaction after 101 days	- - -	+16
„ „ 125 days	- - -	+11·5
„ „ 139 days	- - -	+11

PRELIMINARY NOTE ON THE CHANGES IN REACTION PRODUCED IN GLYCERINATED LITMUS MILK BY HUMAN AND BOVINE TUBERCLE BACILLI.

The method adopted in the foregoing experiments for studying the chemical changes produced in a liquid medium by the tubercle bacillus has the disadvantage that exact information of the variations in reaction is gained only at relatively long intervals.

In order, therefore, to be able to watch the succession of changes in reaction from day to day, milk coloured with litmus was substituted for broth.

Milk was chosen as the basis of the medium, in preference to broth, on account of the readiness with which slight changes in the colour of the litmus can be detected in it. The medium was prepared in the following way: fresh cow's milk was taken and allowed to stand for twenty-four hours, or more, in a cool place to allow the cream to separate; the milk was then syphoned from beneath the cream and coloured with blue litmus solution; after the addition of 2 per cent. glycerin, the fluid was run into six-ounce medicine bottles, about thirty cubic centimetres being placed in each bottle, and fractionally sterilized; at the end of the sterilization the milk was bluish with a faint purplish tinge. After inoculation the bottles were sealed with paraffin.

On this milk medium tubercle bacilli grow very well and form pellicles indistinguishable in character from those produced by the same bacilli on glycerin broth.

The sequence of changes produced in the colour of the medium during the growth of the bacillus is very characteristic. At first as the inoculated material slowly increases in area the milk acquires a deeper blue tint; then, as the pellicle expands and thickens, the original colour of the medium is regained and gradually changed to a bright red; and finally the milk clots.

This type of reaction is exhibited by all the slightly virulent organisms, when luxuriant growths are obtained; occasionally when the growth has failed to cover the surface, or is thinner than usual, the milk has become red, but has remained unclotted.

Virulent tubercle bacilli up to a certain stage bring about exactly the same changes in the colour of the medium; the colour is at first changed into a more intense blue and then gradually to pink or red; none of the virulent tubercle bacilli has, however, clotted the milk, but in the case of H 49 "T.C.," there was towards the end of the experiment a slight deposition of casein.

With some virulent bacilli the deep blue colour produced at the outset has remained permanent; this has occurred when there has been only a slight increase of the inoculated material or when the membrane formed, though complete, has been delicate and transparent.

On many occasions the litmus, after turning pink, has been completely decolourised, the milk becoming perfectly white; when the bottles in which this change has taken place are removed from the incubator and allowed to stand at room temperature, first the pink colour slowly returns and finally the original colour is restored, wholly or partially according to the amount of growth which had been produced on the surface; control bottles of milk placed under the same conditions underwent no change. These phenomena, which appear to depend upon oxygenation, have been observed with slightly virulent bacilli as well as with virulent bacilli, but most frequently with the latter. In no instance when the colour of the milk has been discharged has clotting taken place; the clots so far produced have all been red.

On litmus milk without glycerin in one case a slightly virulent human bacillus grew with moderate luxuriance and the milk became slightly decolourised and acquired a faint pinkish tinge; in all other cases growth has been scanty and the milk has remained very blue.

Further experiments with this medium are being made: *e.g.*, the influence on the reaction of varying percentages of glycerin and of some of the sugars.

The results so far obtained however show that the medium does not differentiate tubercle bacilli into two classes and in this respect they are in agreement with those obtained by the use of glycerin broth.

MODIFICATION EXPERIMENTS WITH TUBERCLE BACILLI OF BOVINE ORIGIN.

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MODIFICATION EXPERIMENTS WITH TUBERCLE BACILLI OF BOVINE ORIGIN.

It has been shown that different strains of bovine bacilli vary within limits in their capacity for growth on media containing glycerin, but it has been impossible to demonstrate any corresponding variation in virulence. Experiments have been carried out to ascertain whether these properties, cultural characters and virulence, are stable or capable of being altered artificially on culture media or by animal passage.

MODIFICATION BY CULTURE.

On pure serum tubercle bacilli of bovine origin grow better in later than in early generations. Prolonged cultivation on this medium does not in our opinion alter the characteristic type of reaction towards media containing glycerin. With viruses of Class I. sub-cultivated on serum for over two years and then grown on solid glycerin media there is the same initial delay and formation of discrete colonies, but the colonies produced are more numerous, and the final amount of growth is generally greater; and this is the case with all viruses, that the amount of growth ultimately produced on all varieties of media, glycerinated or otherwise, accords with the general increase in saprophytic power.

By sub-culture on glycerin media all viruses grow with increased luxuriance on solid glycerin media, and after a number of sub-cultures varying with different strains a type of growth is finally produced which is practically the same for all viruses. This is an opaque warty or slightly wrinkled layer distinctly better than that obtained with the most easy growing viruses of Class III. The change is most marked in those strains which grow originally with the greatest difficulty. These cultures which have been modified by repeated subculture on solid media containing glycerin show no corresponding increase in capacity to grow on glycerin broth. On the other hand by repeated transferences from broth to broth a greater vigour of growth on this medium can be established.

The increased capacity to grow on glycerin media is retained by the cultures after their passage through animals (calf, rabbit, and guinea-pig), and is manifested by the readiness with which they grow in primary culture on glycerin serum and glycerinated potato.

The cultures so recovered show no greater luxuriance as a result of their residence in the animal body, and even after successive passages through animals, the cultures intervening being maintained purely on glycerin media, do not grow better than the same cultures modified by residence on glycerin media alone.

The influence of glycerin produces no diminution in the virulence of the bovine bacilli for rabbits and calves. This has been shown by comparing the virulence of cultures which have been kept on glycerin serum for periods up to nine months with that of the same cultures which have been kept on serum alone.

MODIFICATION BY ANIMAL PASSAGE.

CHIMPANZEE PASSAGES.

Two Viruses, B. IV. and B. IX., are being passed through two series of chimpanzees; these viruses were chosen as representatives of classes of bovine bacilli which showed respectively the greatest and the least reluctance to grow on glycerin media.

Passage with Virus B. IV.

The first animal in this series (No. 8) was fed with 107 c.c. of milk of Cow 172 [Table IV. A.] containing 10,000,000 tubercle bacilli and died 144 days later of broncho-pneumonia of pneumo-coccal origin. The tuberculous disease was limited to the intestines and the nearest glands. Cultures were isolated from a colic and a mesenteric gland.

The second animal of the series (No. 10) was fed with 0·1 mg. of the culture derived from the colic gland of Chimpanzee 8; the culture used was in the fifth generation, 21 days old, and had been in cultivation for a total period of 117 days. This chimpanzee died in 113 days and showed general tuberculosis with extensive ulceration of the intestinal mucous membrane and great enlargement and caseation of the mesenteric glands. Cultures were isolated from a mesenteric gland and from the lung through a guinea-pig.

The third animal of the series (No. 16) was fed with 0·01 mg. of the culture derived from the mesenteric gland of Chimpanzee 10; the culture used was 21 days old, the third generation, and had been in cultivation for a total period of 60 days. This animal died in 91 days of general tuberculosis, the mesenteric and retro-peritoneal glands being much enlarged and extensively caseated. Cultures were isolated from the spleen and a portal gland.

A fourth chimpanzee, No. 22, was inoculated subcutaneously with 0·001 mg. of the culture isolated from the spleen and died in 87 days of general tuberculosis.

The cultures derived from each of the first three animals, namely, from the mesenteric and colic glands of Chimpanzee 8, from the mesenteric glands and lung of Chimpanzee 10, and from the spleen of Chimpanzee 16, have been tested on the differential media employed in the classification of the bovine viruses. All the strains showed cultural characters identical with those of the culture obtained from the original material of Virus B. IV., producing for example discrete colonies on glycerin serum after the initial delay characteristic of the viruses in Class I.

Cultures from each of the three Chimpanzees, 8, 10 and 16, have been inoculated into rabbits. The results show that there has been no alteration in virulence. (*See* Table 1, page 94.)

Passage with Virus B. IX.

The first chimpanzee in this series (No. 14) was fed with 1·1 mg. of culture derived from the original material of Virus B. IX.; the culture used was 20 days old, twentieth generation, and had been in cultivation for a total period of 447 days. This animal died in 63 days of general tuberculosis, the intestines and mesenteric glands being

severely affected. Cultures were isolated from a cervical, a mesenteric, and an ileo-colic gland, and there was no change either in cultural characters or virulence for rabbits and guinea-pigs.

The second chimpanzee in the series (No. 18) was fed with 0·01 mg. of the culture derived from the ileo-colic gland of Chimpanzee 14; the culture used was the fourth generation, 22 days old, and had been in cultivation for a total period of 91 days. This culture was primarily raised on glycerin serum, and was grown on glycerin media until the time of its inoculation. The chimpanzee died in 116 days of general tuberculosis, and cultures were isolated from the spleen. The culture shows no change in cultural characters as a result of its passage through the animal body; there is, however, a slightly increased capacity to grow on glycerin media due to its residence on glycerin media before its inoculation. Its virulence for rabbits is unchanged.

Cultures from other Chimpanzees.

Cultures have also been recovered from two other chimpanzees; one, No. 4, inoculated subcutaneously with 1 mg. of culture from the original material of Virus B. IV. died in 56 days of general tuberculosis; the other, No. 6 fed for a week with tuberculous milk (Virus B. IV.) and killed 100 days after the experiment began also showed general tuberculosis. The cultures showed no change in cultural characters or in their virulence for rabbits.

BABOON AND MONKEY PASSAGES.

Six passage experiments are being carried out on monkeys and baboons with three viruses, one with Virus B. IV., two with Virus B. I., and three with Virus B. IX.

Cultures of Virus B. I. recovered from two rhesus monkeys, Nos. 80 and 82, which were killed, when dying of general tuberculosis, in 165 and 176 days respectively, showed no alteration in cultural characters or virulence for rabbits.

Baboon Passage.—Virus B. I.

The first animal (No. 8) was fed with milk of Cow 64 (Table I. A) containing one million tubercle bacilli. It was killed 97 days later and showed general tuberculosis, the infection spreading from the tonsils. A rabbit inoculated intraperitoneally with an emulsion made from a cervical gland died in 88 days of general tuberculosis.

The second baboon (No. 64) was fed with 0·1 mg. of culture, which had been in cultivation 533 days, derived from the cervical gland of Baboon 8. This animal died in 101 days of severe general tuberculosis, and cultures were obtained from the lung and a cervical gland.

The culture isolated from Baboon 8 showed a distinct increase in luxuriance on the differential media, growing almost as well as the bovine viruses in Class III.

The culture obtained from the lung of Baboon 64 showed a further increase in luxuriance, behaving in the same way as a bovine culture which has been modified by repeated sub-cultivation on glycerin media. Its virulence for rabbits, however, is equal to that of any of the bovine viruses.

Baboon Passage.—Virus B. IV.

The first baboon, No. 22, in this series was fed for 30 days with tuberculous milk of Cow 172 (Table IV. A) and died of general tuberculosis 90 days after the experiment began. A culture was isolated from a cervical gland, and after it had been in cultivation for a total period of 292 days, was given by the mouth in a dose of 0·1 mg. to Baboon 62. This baboon died in 136 days of general tuberculosis and cultures were isolated from the spleen and the lung. These strains as well as that from Baboon 22 were identical in every respect with the culture derived from the original material of Virus B. IV., and there was no alteration in virulence as a result of the passage.

It is interesting to note the stability of the cultural characters and virulence of this virus after prolonged cultivation on artificial media and passage through the bodies of chimpanzees and baboons.

Rhesus Monkey. Baboon Passage.—Virus B. I.

A rhesus monkey, No. 76, was fed with 0·01 mg. of culture derived from Cow 44 (Table I. A.) and was killed when dying 228 days later; the post-mortem examination showed general tuberculosis. A culture was isolated from the spleen and this, after it had been in cultivation for a total period of 310 days, was given by the mouth in a dose of 0·1 mg. to Baboon 66, the second animal of the series.

The baboon died in 133 days of general tuberculosis, and cultures were isolated from the spleen and a cervical gland.

The cultures obtained from both animals were unchanged in their characters on artificial media, and in their virulence for rabbits and guinea-pigs.

Rhesus Monkey. Passage.—Virus B. IX.

The first animal of this series (No. 110) fed with 1·0 mg. of culture from the original material of Virus IX., was killed when dying 80 days after inoculation: the post-mortem examination showed general tuberculosis of a severe type. A culture was isolated from an iliac gland and inoculated subcutaneously into two monkeys, No. 114 receiving a dose of 1·0 mg. and No. 116 a dose of 0·01 mg.

Both animals died of acute miliary tuberculosis in 34 and 36 days respectively, and cultures were isolated in each case from an axillary gland.

The culture from Monkey 116 was used to feed Monkeys 130 and 132, the former receiving 0·01 mg., the latter 0·0001 mg. These animals died in 86 and 134 days respectively of general tuberculosis.

The culture isolated from each of the three animals (Nos. 110, 116 and 132) in the passage showed no change in cultural characters or in virulence for rabbits.

Mangabey Monkey and Baboon Passage.

Virus B. IX.

A Mangabey monkey was fed with 1·0 mg. of culture derived from the original material of Virus B. IX.; it died in 80 days of general tuberculosis.

A culture was isolated from a lumbar gland, and after it had been in cultivation for a total period of 49 days was used to feed a mandrill baboon.

This animal, No. 68, received a dose of 0·1 mg. and died in 46 days of cysticercosis (taenia saginata). The only visible sign of tuberculosis was an enlarged and caseous retro-pharyngeal gland; a guinea-pig, however, inoculated with an emulsion of the spleen died of general tuberculosis.

A culture was made from the retro-pharyngeal gland and used to feed Monkeys 126 and 128, the former receiving a dose of 0·01 mg., the latter a dose of 0·0001 mg. Monkey 126 died in 120 days of general tuberculosis; Monkey 128 was killed in 169 days, and showed no tuberculosis.

The cultures isolated from each animal in the series showed no change in cultural characters and that from the last animal (No. 126) was virulent for rabbits.

Lemur Passage.

Virus B. IX.

The first lemur, No. 2, was fed with 1·0 mg. of culture from the original material of Virus B. IX., and when killed after 46 days was found to have general tuberculosis. A culture was isolated from a mesenteric gland and inoculated subcutaneously into Lemurs 12 and 10, the former receiving 1·0 mg., the latter 0·1 mg. Both animals died prematurely in 23 and 30 days respectively. In Lemur 12 there was a local lesion, caseation of the nearest glands and early tuberculosis of the spleen.

In Lemur 10 the disease was rather more advanced, a few minute tubercles being visible in the lungs, spleen and liver. The culture obtained from Lemur 2 was unchanged in cultural characters and virulence for rabbits.

EXPERIMENTS WITH DOGS.

Dogs have been inoculated, subcutaneously, intraperitoneally, and by feeding, with the object of testing their susceptibility towards the bovine bacillus. The experiments have shown that the dog possesses a high degree of resistance. In six instances cultures have been recovered and investigated to ascertain whether residence in this refractory species had produced any modification in cultural characters or virulence. Each case will be considered separately.

Case 1.

Dog 26 was inoculated intraperitoneally with an emulsion containing 10·15 million bacilli, made from the tuberculous organs of Guinea-Pigs 1148 and 1150. (Milk of Cow 172, Virus B. IV.); it was killed when very ill after 71 days and showed general tuberculosis. A culture obtained from a ventral mediastinal gland was identical with that derived from the original material of Virus B. IV.

Case 2.

Dog 72 was inoculated intraperitoneally with 1·0 mg. of culture obtained from a guinea-pig inoculated with the original material of Virus B. XXII., and was killed, in good health, 156 days later.

The post-mortem examination showed a few yellow foci in the liver, numerous tubercles of a doubtful nature in the kidneys, and several small fibrous tubercles and two or three small grey nodules in the lungs. There was no tuberculosis of the peritoneum and no tubercle bacilli were demonstrated in any of the lesions except in one of the grey nodules in the lungs, from which a culture was isolated.

This culture grows distinctly better than the culture inoculated, and is now classed amongst the most easy-growing bovine viruses; its virulence for rabbits, however, is unchanged.

The culture was inoculated subcutaneously into another dog, No. 126, in a dose of 10 mg. This animal was killed 99 days after inoculation and showed an ulcer at the site of inoculation, slight tuberculosis of the inguinal and bronchial glands, numerous tubercles in the liver and three translucent tubercles in the lungs. A culture isolated from the liver is still under investigation.

Case 3.

Dog 18 was fed with an emulsion of guinea-pig organs containing 10·15 million tubercle bacilli (Virus B. IV.) and showed after 135 days tuberculosis of two mesenteric glands and a few doubtful tubercles in the lung.

The culture isolated from a mesenteric gland grows much more luxuriantly than Virus B. IV. direct (for characters, *see* page 8 of Culture Report), and indeed better than any of the more easy growing bovine viruses. The culture is not virulent for rabbits and a young dog fed with 50 mg. and killed after 77 days showed slight tuberculosis of the pharyngeal, mesenteric and bronchial glands and two tubercles in the lungs.

Case 4.

Dog 50 was fed with 1·0 mg. of culture of Virus B. IX. and was killed 111 days after; the post-mortem examination showed no disease of the alimentary tract or glands in connection with it, but there was tuberculosis of one lobe of the lung. Two guinea-pigs were inoculated from the lung and a culture was isolated from each.

Both these strains grew more luxuriantly than the original culture of Virus B. IX. and even better than that obtained from Dog 18, corresponding in characters with the slightly virulent bacilli of human origin. A calf inoculated subcutaneously with 50 mg. of culture and killed after 91 days showed a local lesion, caseation of the nearest glands and a few tubercles in the lungs and some of the lymphatic glands. A second calf inoculated subcutaneously with an emulsion (containing 83·7 million tubercle bacilli) made from the prescapular gland of the first calf showed, when killed after 64 days, a local lesion only. The results in rabbits also demonstrated the slightly virulent nature of the culture.

Two adult dogs were inoculated each with 10 mg., one subcutaneously, the other by feeding to ascertain the degree of virulence of the culture for dogs. The dogs were killed 99 and 98 days after inoculation respectively. The former showed a small healing ulcer at the seat of inoculation and two grey tubercles in the lung; in the latter there was a single tubercle in the lung and no sign of tuberculosis elsewhere.

Case 5.

Dog 60 was fed with the same dose of the same culture as Dog 50. It was killed 112 days after, and showed tuberculosis limited to the pharyngeal, mesenteric, and ileo-colic glands.

The culture recovered from a mesenteric gland showed no definite increase in luxuriance and was virulent for rabbits.

This culture was obtained in the usual way by scraping the whole surface of a primary tube. In addition a number of single colonies from one primary culture have been separately investigated with regard to their virulence for rabbits, one colony having shown on glycerin agar a distinct increase in luxuriance; the rabbits were inoculated subcutaneously, each with the growth from a single glycerin agar tube in the third generation, and all developed general tuberculosis of the usual type.

A dog, No. 84, was fed with 50 mg. of the culture obtained in the ordinary way; when killed 73 days later it showed a few miliary tubercles in the lungs and no disease elsewhere. A culture isolated direct from one of the tubercles possesses the same cultural characters and virulence as that from the original virus.

A young dog, No. 130, was inoculated subcutaneously with 48 mg. of culture derived from the single colony, No. 3, which had shown a slightly increased power of growth; this animal died in 85 days of acute general tuberculosis, the inoculation having been partly intraperitoneal; a culture isolated from the liver is still under investigation.

Case 6.

Dog 102, an adult animal, was fed with 100 mg. of culture derived from the original material of Virus B. VIII.; it was killed in 72 days and showed one nodule in the lung and no disease elsewhere; a culture isolated direct from the nodule resembled that inoculated.

Additional Experiments.

The results in two cases, Dog 18 and Dog 50, namely, the isolation of cultures entirely different from those inoculated, suggested the possibility that the bovine bacillus could be modified in cultural characters and virulence by residence in the body of the dog. In order to exclude natural tuberculosis as a source of error the method of inserting into the peritoneal cavity a collodion capsule containing culture was adopted.

In the first experiment the culture used was from the original material of Virus B. IV. The Dog No. 54 was killed after 142 days; the collodion part of the capsule was found ruptured in the centre of a tuberculous tumour in the gastro-splenic omentum and the glass part free in the peritoneal cavity. There was tuberculous disease of the peritoneum, spleen, and kidneys.

Cultures were recovered from the glass part of the capsule and from the tumour. Both strains were found to be identical with the original culture, 12 colonies from a primary culture from the capsule, being individually tested on serum and glycerin serum.

The growth from six of the serum tubes containing 32 mg. was used to feed Dog 82, which was killed 91 days later, and found to be free from tuberculosis.

Dog 92 was fed with 25 mg. of the growth from the corresponding glycerin serum tubes after 68 days incubation. This animal was killed after 64 days and showed tuberculosis of one submaxillary gland, and twelve caseous nodules in the lungs. A culture isolated direct from the lung showed no alteration in character, beyond that due to its residence on glycerin media, and no change in virulence.

In the second experiment the capsule contained the culture isolated from Chimpanzee 8 (Virus B. IV.). The Dog, No. 56, was ill and emaciated when killed. The post-mortem examination showed tuberculosis of the peritoneum and miliary tubercles in the lungs, the capsule, as in the previous experiment, being ruptured and surrounded by a tuberculous tumour. Cultures recovered from the tumour, and from the lung through a guinea-pig, showed no change.

The culture from the tumour was inoculated subcutaneously in a dose of 10 mg. into Dog 74. In this animal, killed after 63 days, there was a local lesion, a tuberculous cavity in the region of the axilla and miliary tuberculosis of the lungs and kidneys. Cultures obtained

from the cavity and from a bronchial gland through a guinea-pig were identical with that from the original material of Virus B. IV.

The culture isolated from the bronchial gland was used to feed Dog 116, in a dose of 10 mg.; the dog was killed after 98 days and showed general tuberculosis of a rather more severe type than that so far produced in dogs by feeding. Cultures have been isolated from a mesenteric gland and three separate lung nodules. Those obtained from the mesenteric gland and two of the lung nodules are identical with that from the original material of Virus B. IV.; the culture from the third lung nodule grows distinctly better and is equal to any in Class III. The virulence of these cultures has not yet been tested.

PIG PASSAGE.

Virus B. I. has been passed through a series of four pigs in succession, all of which developed general tuberculosis. The total period of residence in the animal body was 551 days.

The first animal was fed with tuberculous milk of Cow 40 and an emulsion made from its mesenteric glands was inoculated subcutaneously into two pigs, No. 10 and No. 2.

From the lesions of one of these, No. 12, two more pigs, No. 14 and 16, were inoculated subcutaneously.

The passage was continued with culture obtained from Pig 16; this was inoculated subcutaneously into Pigs 96 and 98 in doses of 0·1 mg. and 1 mg. respectively. Cultures were isolated from all the pigs, and showed in their reactions on the differential media the characters of the original virus.

The virulence of the culture from the last pig of the series was unaltered.

BOVINE PASSAGES.

Three viruses, namely B. I., B. II. and B. IV., have each been passed through a series of bovines, the number of animals being six in the first, three in the second and three in the third series, and the duration of residence 494, 489 and 693 days respectively. There was no evidence of any alteration in virulence, and culture isolated from the different animals in each series were identical with those from the original viruses.

GUINEA-PIG PASSAGE.

Virus B. II. has been passed through a series of 24 guinea-pigs. The first guinea-pig was inoculated with an emulsion from the original material and each of the others with an emulsion of a tuberculous organ of the preceding animal.

The total period of residence in the guinea-pig's body was 3 years 166 days.

Cultures were isolated from three of the guinea-pigs, including the last of the series.

The cultural characters of the three strains were precisely the same as those of other strains of this virus.

This passage experiment shows clearly that residence in the body of the guinea-pig does not alter the characters of the bovine tubercle bacillus.

SUMMARY OF PASSAGE EXPERIMENTS.

Chimpanzees, baboons and monkeys, although peculiarly susceptible to the action of the bovine tubercle bacillus, were chosen for passage experiments on account of their blood relationship to man. In order to approximate as nearly as possible to a natural mode of infection, the bacilli were inoculated by feeding with culture or with milk containing tubercle bacilli. The passages were continued by culture isolated from the preceding animal in order to encourage the more saprophytic bacilli to increase, and with the same object in several cases the cultures were grown on media containing glycerin.

In the chimpanzee passage with Virus B. IV. three animals were used, the total period of residence in the animal body being 348 days and in culture 138 days.

In the chimpanzee passage with Virus B. IX. two animals were used, the total period of residence in the animal body being 179 days and in culture 538 days.

In the baboon and monkey passages the periods during which the bacilli were in the animal body varied from 198 to 361 days.

With one exception the culture recovered from the last animal in each series showed no change in cultural characters and in all instances the virulence for rabbits remained unaltered. The exceptional case occurred in a passage experiment with Virus B. I. through two baboons, the total period of residence of the culture in the animal body being 198 days. The virus primarily used belonged to the most difficult growing class of bovine bacilli, while that recovered from the last animal grew rather better than any of the most easy growing bovine viruses.

The dog exhibits a high degree of resistance to the action of the bovine tubercle bacillus when introduced into the body by subcutaneous inoculation or by feeding. In five instances cultures have been recovered from dogs experimentally inoculated.

Two dogs were fed with Virus B. IV. and Virus B. IX. respectively and were killed after 135 and 111 days; in each case the culture isolated grew more luxuriantly than any bovine virus and was slightly virulent. Moreover, when reinoculated into other dogs they were found to be incapable of causing more than minimal lesions. Three dogs were inoculated with Viruses B. IV. VIII. and XXII. A culture isolated from the latter killed after 156 days showed increased luxuriance without change in virulence, while in the others there was not the least alteration.

The results in the first two cases may be explained on two hypotheses—either the lesions were not due to the bacilli experimentally introduced, or the bacilli introduced were modified by residence in the animal body.

In order to test the latter hypothesis, Virus B. IV. was passed through two series of dogs and Virus B. IX. through one series, the periods in which the bacilli were in contact with the tissues being 206, 237 and 185 days. The results were negative, the cultures isolated from the last animal in each series showing not the least change in cultural characters or virulence.

The passage experiments with pigs (551 days), oxen (498, 489 and 693 days) and guinea-pigs (1,261 days) produced no change of cultural characters or virulence in the viruses used.

Conclusions.

These passage experiments, so far as they have gone, show that the bovine tubercle bacillus is very retentive of its special cultural characters and high virulence.

The results with dogs are suggestive, and the passage experiments are being continued in order to ascertain whether the dog can modify the bovine tubercle bacillus.

A. Stanley Griffith.

F. Griffith.

SYNOPSIS OF PASSAGE EXPERIMENTS.

Virus.	Species of Animal.	Number of Animals in the Passage.	Duration of the Passage.	Result of the Passage on	
				Characters of Culture.	Virulence.
B. I. - -	Ox	6	494 days	No change	No change
B. II. - -	Ox	3	489 days	„	„
B. IV. - -	Ox	3	693 days	„	„
B. IV. -	Chimpanzee	3	348 days	No change	No change
B. IX. - -	Chimpanzee	2	179 days	„	„
B. I. - -	Baboon	2	198 days	Grows more luxuriantly.	„
B. I. - -	Rhesus Monkey and Baboon.	2	361 days	No change	„
B. IV. - -	Baboon	2	226 days	„	„
B. IX. - -	Rhesus Monkey	3	250 days	„	„
B. IX. - -	Rhesus Monkey and Baboon.	3	256 days	„	„
B. IX. - -	Lemur	2	76 days	„	„
B. I. - -	Pig	4	551 days	No change	No change
B. II. - -	Guinea-pig	24	1,261 days	No change	No change
B. IV. - -	Dog	2	206 days	No change	No change
B. IV. - -	Dog	3	237 days	„	„
B. IX. - -	Dog	2	185 days	„	„
B. IV. - -	Dog (26)	1	71 days	„	—
B. VIII. -	Dog (102)	1	72 days	„	—
B. XXII. -	Dog (72)	1	156 day	Grows more luxuriantly, Class I. to Class III.	No change
B. IV. - -	Dog (18)	1	135 days	Grow better than any bovine virus.	And are only slightly virulent for calves and rabbits.
B. IX. - -	Dog (50)	1	111 days		

TABLE 1.

CHIMPANZEE PASSAGE.

WITH

VIRUS B. IV.

(July 29, 1903.)

CHIMPANZEE 8.

Fed on October 28, 1904, with 107cc. milk of Cow 172 (Table IV. A.) containing ten million tubercle bacilli.

Died of broncho-pneumonia March 21, 1905, 144 days after.

P.M.—Tuberculosis limited to intestines and nearest glands.

CULTURE
derived from a colic gland.

RABBITS

Inoculated with culture from original material.

Number.	Dose.	Method.	Duration of Life.	Result.
6 rabbits	0·1 mg.	Intrav.	D. 21 days (average)	G.T.
117	0·1 mg.	Intrap.	D. 40 days	G.T.
87	0·1 mg.	Subcut.	D. 105 „	G.T.
88	0·1 mg.	„	D. 72 „	G.T.
133	1·0 mg.	„	D. 112 „	G.T.

CHIMPANZEE 10.

Fed on July 16, 1905, with 0·1 mg. of culture from Chimpanzee 8.

Died, November 6, 1905, 113 days later.

P.M.—General tuberculosis.

CULTURE
derived from a mesenteric gland.

RABBITS

Inoculated Nov. 6, 1906, with culture from Chimpanzee 8.

Number.	Dose.	Method.	Duration of Life.	Result.
320	0·1 mg.	Intrav.	D. 16 days	G.T.
321	1·0 mg.	Intrap.	D. 14 „	G.T.
324	0·1 mg.	Subcut.	D. 64 „	G.T.
322	1·0 mg.	„	D. 53 „	G.T.

CHIMPANZEE 16.

Fed on January 5, 1906, with 0·01 mg. of culture from Chimpanzee 10.

Died, April 6, 1906, 91 days later.

P.M.—General tuberculosis.

CULTURE
derived from spleen.

RABBITS

Inoculated Jan. 5, 1906, with culture from Chimpanzee 10.

Number.	Dose.	Method.	Duration of Life.	Result.
658	0·1 mg.	Intrav.	D. 20 days	G.T.
664	0·01 mg.	Intrap.	D. 41 „	G.T.
662	0·1 mg.	„	D. 32 „	G.T.
660	1·0 mg.	„	D. 18 „	G.T.
663	0·01 mg.	Subcut.	D. 100 „	G.T.
661	0·1 mg.	„	D. 85 „	G.T.
659	1·0 mg.	„	D. 96 „	G.T.

CHIMPANZEE 22.

Inoculated subcutaneously on July 4, 1906, with 0·001 mg. of culture from Chimpanzee 16.

Died, September 29, 1906, 87 days later.

P.M.—General tuberculosis.

CULTURE
derived from bronchial gland.

RABBITS.

Inoculated Aug. 4, 1906, with culture from Chimpanzee 16.

Number.	Dose.	Method.	Duration of Life.	Result.
948	1·0 mg.	Intrav.	D. 14 days	G.T.
949	0·1 mg.	„	D. 16 „	G.T.
950	1·0 mg.	Intrap.	D. 9 „	G.T.
951	0·1 mg.	„	D. 42 „	G.T.
954	0·001 mg.	„	K. 54 „	G.T.
955	0·001 mg.	„	D. 45 „	G.T.
952	1·0 mg.	Subcut.	K. 54 „	G.T.
953	0·1 mg.	„	K. 54 „	G.T. (Slight)

CHIMPANZEE 24.

Inoculated subcutaneously on November 21, 1906, with 0·00001 mg. of culture from Chimpanzee 22.

TABLE 2.

CHIMPANZEE PASSAGE.

VIRUS B. IX.

(June 21st, 1904.)

CHIMPANZEE 14.

Fed on September 11th, 1905, with about 1·1 mg. of culture derived from the original material of Virus B. IX.

Died, November 13th, 1905. 63 days later.

P.M.—General tuberculosis; intestines and mesenteric glands severely affected.

RABBITS

Inoculated with culture from the original material.

Number.	Dose.	Method.	Duration of Life.	Result.
212	0·1 mg.	Intrav.	D. 18 days	G.T.
(4 rabbits)	1·0 mg.	Intrap.	D. 22 days (average).	G.T.
211	0·1 mg.	Intrap.	D. 30 days	G.T.
(3 rabbits)	1·0 mg.	Subcut.	D. 69 days (average).	G.T.

CULTURE
derived from an ileo-colic gland.

CHIMPANZEE 18.

Fed on February 12th, 1906, with 0·01 mg. of culture from Chimpanzee 14.

Died, June 8th, 1906. 116 days after.

P.M.—General tuberculosis.

RABBITS.

Inoculated February 12, 1906, with culture from Chimpanzee 14.

Number.	Dose.	Method.	Duration of Life.	Result.
742	1·0 mg.	Subcut.	D. 25 days	S.T.
743	0·1 mg.	Subcut.	D. 103 days	G.T.
744	0·01 mg.	Subcut.	D. 132 days	G.T.
745	0·001 mg.	Subcut.	D. 151 days	G.T.

CULTURE
derived from spleen.

RABBITS.

Inoculated July 14, 1906, with culture from Chimpanzee 18.

Number.	Dose.	Method.	Duration of Life.	Result.
993	1·0 mg.	Intrav.	D. 16 days	G.T.
994	0·1 mg.	Intrav.	D. 18 days	G.T.
996	1·0 mg.	Intrap.	D. 17 days	G.T.
995	0·1 mg.	Intrap.	D. 30 days	G.T.
997	1·0 mg.	Subcut.	K. 44 days	Early G.T.
998	0·1 mg.	Subcut.	K. 44 days	

TABLE 3.

BABOON PASSAGE.

VIRUS B. I.

BABOON 8.

Fed on January 22, 1904, with milk of Cow 64 containing one million tubercle bacilli.

Killed, April 28, 1904, 97 days later.

P.M.—General tuberculosis.

RABBITS

Inoculated with culture obtained from Cows 40 and 44.

Number.	Dose.	Method.	Duration of Life.	Result.
(7 rabbits)	0·1 mg.	Intrav.	22 days (average)	G.T.
137	1·0 mg.	Intrap.	31 days	G.T.
112	0·1 mg.	Intrap.	31 days	G.T.
136	1·0 mg.	Subcut.	82 days	G.T.
3 rabbits	0·1 mg.	Subcut.	95 days (average)	G.T.

CULTURE
derived from cervical gland

RABBIT.

Inoculated with emulsion from cervical gland.

Number.	Dose.	Method.	Duration of Life.	Result.
48	not estimated.	Intrap.	D. 88 days	G.T.

BABOON 64.

Fed on October 13, 1905, with 0·1 mg. of culture from Baboon 8.

Died, January 22, 1906, 101 days later.

P.M.—General tuberculosis.

CULTURE
derived from lung

RABBITS.

Inoculated on May 1, 1906, with culture from Baboon 64.

Number.	Dose.	Method.	Duration of Life.	Result.
857	0·1 mg.	Intrav.	D. 21 days	G.T.
858	1·0 mg.	Intrap.	D. 15 days	G.T.
859	0·1 mg.	Intrap.	D. 16 days	G.T.
860	1·0 mg.	Subcut.	K. 91 days (Dying)	G.T.

TABLE 4.

BABOON PASSAGE.

VIRUS B. IV.

(July 29, 1903.)

BABOON 22.

Fed with tuberculous milk of Cow 172 from September 9, 1904, to October 4, 1904.

Died December 8, 1904, 90 days after the experiment began.

P.M.—General tuberculosis.

CULTURE

derived from a cervical gland.

BABOON 62.

Fed on September 26, 1905, with 0·1 mg. of culture from Baboon 22.

Died, February, 1906, 136 days later.

P.M.—General tuberculosis.

CULTURE

derived from the lung.

RABBITS.

For rabbits inoculated with culture from the original material *see* Table 1.

RABBITS.

Inoculated on May 11, 1906, with culture from Baboon 62.

Number.	Dose.	Method.	Duration of Life.	Result.
874	0·1 mg.	Intrav.	D. 19 days	G.T.
873	0·1 mg.	Intrap.	D. 16 days	G.T.
872	1·0 mg.	Subcut.	K. 108 days	G.T. (slight)

TABLE 5.

RHESUS MONKEY AND BABOON PASSAGE.

VIRUS B. I.

MONKEY 76.

Fed on April 7, 1904, with 0·01 mg. of culture obtained from the supramammary lymphatic gland of Cow 44.

Killed, when dying, November 21, 1905, 228 days later.

P.M.—General tuberculosis.

CULTURE
obtained from the spleen.

RABBITS.

For rabbits inoculated with bovine strains of this virus *see* Table 4.

BABOON 66.

Fed on September 27, 1905, with 0·1 mg. of culture from Monkey 76.

Died, February 7, 1906, 133 days after.

P.M.—General tuberculosis.

CULTURE
derived from spleen.

RABBITS.

Inoculated on February 20, 1905, with culture from Monkey 76.

Number.	Dose.	Method.	Duration of Life.	Result.
189	0·1 mg.	Intrav.	D. 24 days	G.T.
192	1·0 mg.	„	D. 16 „	G.T.
190	1·0 mg.	Intrap.	D. 23 „	G.T.
191	1·0 mg.	„	D. 32 „	G.T.
193	1·0 mg.	Subcut.	D. 47 „	G.T.
194	1·0 mg.	„	D. 63 „	G.T.

RABBITS.

Inoculated on July 6, 1906, with culture from Baboon 66.

Number.	Dose.	Method.	Duration of Life.	Result.
984	1·0 mg.	Intrav.	D. 14 days	G.T.
985	0·1 mg.	„	D. 26 „	G.T.
987	1·0 mg.	Intrap.	D. 12 „	G.T.
986	0·1 mg.	„	D. 1 day	Coccidiosis.

TABLE 6.

RHESUS MONKEY PASSAGE.

VIRUS B. IX.

(June 21, 1904.)

MONKEY 110.

Fed on July 10, 1905, with 1·0 mg. of culture derived from the original material of Virus B IX.

Killed, when dying, September 28, 1905, 80 days later.

P.M.—General tuberculosis.

RABBITS.

For rabbits inoculated from original material *see* Table 2.

CULTURE
derived from an iliac gland.

MONKEY 116.

Inoculated subcutaneously on November 22, 1905, with 1·0 mg. of culture from Monkey 110.

Died, December 28, 1905, 36 days later.

P.M.—Miliary tuberculosis.

MONKEY 114.

Inoculated subcutaneously on November 22, 1905, with 0·01 mg. of culture from Monkey 110.

Died, December 26, 1905, 34 days later.

P.M.—Miliary tuberculosis.

RABBITS.

Inoculated with culture from Monkey 110.

Number.	Dose.	Method.	Duration of Life.	Result.
521	1·0 mg.	Intrap.	D. 22 days	G.T.
522	0·01 mg.	Intrap.	D. 37 „	G.T.

CULTURE
derived from an axillary gland.

MONKEY 130.

Fed on January 24, 1906, with 0·01 mg. of culture from Monkey 116.

Killed when ill, April 20, 1906, 86 days later.

P.M.—General tuberculosis.

MONKEY 132.

Fed on January 24, 1906, with 0·0001 mg. of culture from Monkey 116.

Died, June 7, 1906, 134 days later.

P.M.—General tuberculosis.

RABBITS.

Inoculated on January 24, 1906, with culture from Monkey 116.

Number.	Dose.	Method.	Duration of Life.	Result.
691	1·0 mg.	Subcut.	D. 100 days	G.T.
692	0·01 mg.	„	D. 20 „	S.T.
593	0·0001 mg.	„	D. 100 „	G.T.

MONKEY 152.

Inoculated subcutaneously on June 7, 1906, with a small quantity of emulsion made from the spleen of Monkey 132.

Died, September 26, 1906, 111 days after inoculation.

P.M.—General tuberculosis.

RABBIT.

Inoculated intraperitoneally with an emulsion made from a splenic lymphatic gland.

Number.	Duration of Life.	Result.
1039	D. 75 days	G.T.

RABBITS.

Inoculated on July 27, 1906, with culture derived from the bronchial gland of Monkey 132.

Number.	Dose.	Method.	Duration of Life.	Result.
1022	1·0 mg.	Intrav.	D. 5 days	—
1019	0·1 mg.	Intrav.	D. 16 days	G.T.
1020	0·1 mg.	Intrav.	D. 21 days	G.T.
1021	1·0 mg.	Intrap.	D. 23 days	G.T.
1023	0·1 mg.	Intrap.	D. 5 days	—

TABLE 7.

MONKEY AND BABOON PASSAGE.

VIRUS B. IX.

(June 21, 1904.)

MANGABEY MONKEY 4.

Fed on June 2, 1905, with 1·0 mg. of culture derived from the original material of Virus IX.

Killed, August 25, 1905, 84 days later.

P.M.—General tuberculosis.

CULTURE
derived from a lumbar gland.

MANDRILL BABOON 68.

Fed on October 13, 1905, with 0·1 mg. of culture from Mangabey 4.

Died, November 28, 1905, 46 days later.

P.M.—Tuberculosis limited to a retro-pharyngeal gland.

CULTURE
derived from the pharyngeal gland.

MONKEY 126.

Fed on January 24, 1906, with 0·01 mg. of culture from Baboon 68.

Died, May 24, 1906, 126 days later.

P.M.—General tuberculosis.

CULTURE.
derived from spleen.

MONKEY 128.

Fed on January 24, 1906, with 0·0001 mg. of culture from Baboon 68.

Killed, July 12, 1906, 169 days later.

P.M.—No tuberculosis.

RABBITS.

For rabbits inoculated with culture from the original material *see* Table 2.

RABBITS.

Inoculated on January 24, 1906, with culture from Baboon 68.

Number.	Dose.	Method.	Duration of Life.	Result.
688	1·0 mg.	Subcut.	D. 77 days	G.T.
689	0·01 mg.	„	D. 129 days	G.T.
690	0·0001 mg.	„	K. 187 days	G.T.

RABBITS.

Inoculated on July 27, 1906, with culture from Monkey 126.

Number.	Dose.	Method.	Duration of Life.	Result.
1016	1·0 mg.	Intrav.	D. 15 days	G.T.
1015	0·1 mg.	Intrav.	D. 19 days	G.T.
1018	0·1 mg.	Intrav.	D. 25 days	G.T.
1014	1·0 mg.	Intrap.	D. 5 days	—
1017	0·1 mg.	Intrap.	D. 28 days	G.T.

TABLE 8.

LEMUR PASSAGE.

VIRUS B. IX.

(June 27, 1904.)

LEMUR 2.

Fed June 2, 1905, with 1·0 mg. of culture derived from the original material of Virus B. IX.

Killed, July 18, 1905, 46 days after.

P.M.—General tuberculosis.

CULTURE
derived from a mesenteric gland.

LEMUR 12.

Inoculated subcutaneously on November 24, 1905, with 1·0 mg. of culture from Lemur 2.

Killed, dying, December 17, 1905, 23 days after.

P. M. — Local abscess, caseation of nearest glands and early tuberculosis of the spleen.

LEMUR 10.

Inoculated subcutaneously on November 24, 1905, with 0·01 mg. of culture from Lemur 2.

Died, December 24, 1905, 30 days after.

P. M. — Local lesion, caseation of nearest glands, a few tubercles in spleen, liver, and lungs.

RABBITS.

For rabbits inoculated with culture from original material, *see* Table 2.

RABBITS.

Number.	Dose.	Method.	Duration of Life.	Result.
535	1·0 mg.	Intrav.	D. 14 days	G.T.
536	0·1 mg.	„	D. 21 „	G.T.
537	1·0 mg.	Intrap.	D. 18 „	G.T.

TABLE 9.

DOG PASSAGE.

VIRUS B. IV.

(July 29, 1903.)

DOG 54.

A collodion capsule containing an emulsion of culture derived from the original material of Virus B. IV. was inserted into the peritoneal cavity on May 20, 1905.

The dog was killed on October 9, 1905, 142 days later.

P.M.—The capsule was found ruptured and surrounded by a tumour, and there was tuberculosis of the peritoneum, spleen and kidneys.

CULTURE
raised from the interior of the capsule.

DOG 92.

Fed on January 22, 1906, with 25 mg. of culture from Dog 54.

(The culture used was a glycerine serum culture and was 68 days old.)

Killed, March 27, 1906, 64 days after.

P.M.—Tuberculosis of one submaxillary gland: about 12 caseous nodules in the lung.

CULTURE
derived from the lung.

RABBITS.

Inoculated on July 18, 1906.

Number.	Dose.	Method.	Duration of Life.	Result.
1009	1·0 mg.	Intrav.	D. 14 days	G.T.
1010	0·1 mg.	Intrav.	D. 21 days	G.T.
1013	1·0 mg.	Intrap.	D. 20 days	G.T.
1011	0·1 mg.	Intrap.	D. 26 days	G.T.
1012	1·0 mg.	Subcut.	K. 40 days	G.T. (slight)

DOG 82.

Fed on December 15, 1906, with 32 mg. of culture from Dog 54.

(The culture used was a serum culture and was 30 days old.)

Killed, March 16, 1906, 91 days after.

P.M.—There were some doubtful tubercles in the kidneys and one doubtful nodule in the lung (not apparently tuberculous).

TABLE 10.

DOG PASSAGE.

VIRUS B. IV.

DOG 56.

A collodion capsule containing culture from Chimpanzee 8 was inserted in the peritoneal cavity on June 8, 1905.

The dog was killed on August 23, 1905, 76 days after.

P.M.—The capsule was found ruptured and surrounded by a tumour; there was peritoneal tuberculosis and miliary tuberculosis of the lungs.

CULTURE
obtained from the tumour.

DOG 74.

Inoculated subcutaneously on October 13, 1905, with 10 mg. of culture from Dog 56.

Killed, December 15, 1905, 63 days later.

P.M.—Local lesion; fairly numerous grey miliary tubercles in the lungs; a few tubercles in the kidneys.

CULTURE
derived from a bronchial gland (normal to the eye) through Guinea-pig 1808, January 26, 1906.

DOG 116.

Fed on March 29, 1906, with 10 mg. of culture from Dog 74.

Killed, July 5, 1906, 98 days later.

P.M.—Tuberculosis of pharyngeal, cervical and mesenteric glands, liver, lungs and bronchial glands.

RABBIT.

Inoculated with emulsion of lung of Dog 116 (very small dose).

Number.	Method.	Duration of Life.	Result.
971	Intrap.	K. 52 days	Very slight tub'rc'los's

TABLE II.

DOG PASSAGE.

VIRUS B. IV.

(July 29, 1903.)

DOG 18.

Fed on August 23, 1904, with emulsion made from organs of Guinea-pigs 1148 and 1150, inoculated with milk of Cow 164.

Dose : 10·15 million tubercle bacilli.

Killed, January 5, 1905. 135 days later.

P.M.—Tuberculosis of two mesenteric glands and a few doubtful tubercles in the lungs.

RABBITS.

For rabbits inoculated with culture from original material *see* Table I.

CULTURE
derived from a mesenteric gland.

DOG 132.

Fed on June 1, 1906, with 50 mg. of culture derived from Dog 18.

Killed, August 17, 1906. 77 days later.

P.M.—Caseous focus in a pharyngeal gland; caseous patches and foci in the mesenteric glands; two tubercles in the lungs and a caseous focus in each bronchial gland.

RABBITS.

Inoculated with culture from Dog 18.

Number.	Dose.	Method.	Duration of Life.	Result.
410	0·1 mg.	Intrav.	K. 97 days	A few T.'s in lungs and kidneys.
890	10· mg.	Intrap.	K. 83 days	A few T.'s on peritoneum, in lungs and kidneys.
409	1·0 mg.	Intrap.	K. 97 days	A few T.'s on peritoneum, in lungs and kidneys.
411	0·1 mg.	Intrap.	D. 68 days	A few T.'s on peritoneum and in lungs.
408	1·0 mg.	Subcut.	K. 97 days	Local lesion, 2 nodules in lung.
412	0·1 mg.	Subcut.	K. 97 days	Ulcerated local lesion, 1 nodule in lung.

TABLE 12.

DOG PASSAGE.

VIRUS B. IX.

(June 21, 1904.)

DOG 60.

Fed on July 10, 1905, with 1·0 mg. of culture derived from the original material. Killed, October 30th, 112 days after.
P.M.—Retrogressive tuberculosis of pharyngeal, mesenteric and ileo-colic glands.

CULTURE
derived from a mesenteric gland.

DOG 84.

Fed on January 2, 1906, with 50 mg. of culture originating from a number of primary colonies.

Killed, March 16, 1906, 73 days later.

P.M.—There were a few miliary tubercles in the lungs, and no disease elsewhere.

CULTURE
derived from the lung.

RABBITS.

Inoculated on July 17, 1906, with culture from Dog 84.

Number.	Dose.	Method.	Duration of Life.	Result.
1003	1·0 mg.	Intrav.	D. 17 days	G.T.
1000	0·1 mg.	„	D. 23 „	G.T.
1001	1·0 mg.	Intrap.	D. 21 „	G.T.
1002	0·1 mg.	„	D. 32 „	G.T.
999	1·0 mg.	Subcut.	K. 111 „	G.T. (slight).

RABBITS.

Inoculated with culture originating from a number of primary colonies.

Number.	Dose.	Method.	Duration of Life.	Result.
637	0·1 mg.	Intrav.	D. 28 days	G.T.
639	1·0 mg.	Intrap.	D. 28 „	G.T.
638	0·1 mg.	„	D. 25 „	G.T.
640	1·0 mg.	Subcut.	D. 161 „	G.T.
641	0·1 mg.	„	D. 78 „	G.T.

RABBITS.

Inoculated with cultures originating from single primary colonies. Dose in each case=the growth from one glycerin agar tube.

Number of Rabbit.	Number of Colony.	Method.	Duration of Life.	Result.
729	1	Subcut.	D. 90 days	G.T.
730	2	„	K. 105 „	G.T.
731	3	„	K. 90 „	G.T.
732	4	„	D. 88 „	G.T.
733	5	„	D. 75 „	G.T.
734	6	„	D. 71 „	G.T.
735	7	„	D. 93 „	G.T.
736	8	„	K. 105 „	G.T.
737	9	„	K. 105 „	G.T.

DOG 130.

Inoculated subcutaneously and intraperitoneally on May 17, 1906, with 48 mg. of culture originating from a single primary colony.

Died, August, 1906, 85 days after inoculation.

P.M.—General tuberculosis.

RABBITS.

Inoculated intraperitoneally with an emulsion from the liver.

Number.	Duration of Life.	Result.
1029	K. 55 days.	G.T.
1030	D. 14 days	S.T.

TABLE 13.

DOG PASSAGE.

VIRUS B. IX.

(June 21, 1904.)

DOG 50.

Fed on June 2, 1905, with 1·0 mg. of culture derived from the original material of Virus B IX.

Killed, September 21, 1905, 111 days later.

P.M.—Tuberculosis of one lobe of the lung, and (?) of the kidneys.

LUNG

GUINEA-PIG 1707.

Inoculated September 21, 1905.

Died October 21, 1905 (30 days).

P.M.—General tuberculosis.

CULTURE

7th generation, 20 days old.

DOG 108.

Fed on March 21, 1906, with 10 mg. of culture.

Killed, June 27, 1906, 98 days after.

P.M.—One miliary tubercle in the lungs; no sign of tuberculosis elsewhere.

DOG 110.

Inoculated subcutaneously on March 21, 1906, with 10 mg. of culture.

Killed, June 28, 1906, 99 days after inoculation.

P.M.—Small healing ulcer; two grey tubercles in the lungs.

GUINEA-PIG 1708.

Inoculated September 21, 1905.

Died October 18, 1905 (27 days).

P.M.—General tuberculosis.

CULTURE

3rd generation, 20 days old.

CALF 374.

Inoculated subcutaneously on January 2, 1906, with 50 mg. of culture.

Killed April 3, 1906, 91 days later.

P.M.—Caseous local lesion; caseation of nearest glands; a few tubercles in the portal, thoracic, coeliac and ileo-colic glands and the lungs.

CALF 394.

Inoculated subcutaneously on April 3, 1906, with an emulsion from the prescapular gland of Calf 374.

Number of tubercle bacilli.

Killed, June 6, 1906, 64 days after inoculation.

P.M.—Local lesion only.

RABBITS.

For rabbits inoculated with culture from the origi material, *see* Table 2.

RABBITS.

Number of Rabbit.	Dose.	Method.	Duration of Life.	Result.
634	0·1 mg.	Intrav.	K. 91 days	T. of lung kidneys an muscles.
633	1·0 mg.	„	K. 91 „	G.T.
636	0·1 mg.	Intrap.	D. 68 „	T. of peritoneu and lungs.
632	1·0 mg.	„	D. 81 „	G.T.
635	1·0 mg.	Subcut.	K. 91 „	Slight tuber culosis.

TABLE 14.

DOG PASSAGE.

VIRUS B. XXII.

(April 18, 1905.)

DOG 72.

Inoculated intraperitoneally on September 4, 1905, with 1·0 mg. of culture derived from the original material through Guinea-pig 1509.

Killed, February 7, 1906, 156 days later.

P.M.—There were a few nodules and fibrous tubercles in the lungs, a few yellow foci in the liver and some doubtful tubercles in the kidneys.

CULTURE
derived from a lung nodule.

RABBITS.

Inoculated with culture from the original material through Guinea-pig 1509.

Number.	Dose.	Method.	Duration of Life.	Result.
419	0·1 mg.	Intrav.	D. 22 days	G.T.
422	1·0 mg.	Intrap.	D. 21 „	G.T.
420	0·1 mg.	Intrap.	D. 28 „	G.T.
418	1·0 mg.	Subcut.	D. 79 „	G.T.
421	0·1 mg.	Subcut.	D. 113 „	G.T.

DOG 126.

Inoculated subcutaneously on May 9, 1906, with 10·0 mg. of culture from Dog 72.

Killed, August 16, 1906, 99 days after inoculation.

P.M.—Local lesion; slight tuberculosis of nearest glands: three tubercles in the lungs; bronchial glands slightly affected; liver contained numerous miliary tubercles.

RABBITS.

Inoculated on May 9, 1906 with culture from Dog 72.

Number.	Dose.	Method.	Duration of Life.	Result.
867	0·1 mg.	Intrav.	D. 26 days	G.T.
868	1·0 mg.	Intrap.	D. 19 „	G.T.
869	0·1 mg.	Intrap.	D. 8 „	Coccidiosis.
871	1·0 mg.	Subcut.	D. 109 „	G.T.
870	0·1 mg.	Subcut.	D. 55 „	G.T.

TABLE 15.

PIG PASSAGE.

VIRUS B I.

(May 6, 1902.)

PIG 8.

Fed with milk of Cow 40 for 92 days.

Killed, June 4, 1903, 247 days after the experiment began.

P.M.—Two or three caseous nodules in the small intestine, not ulcerated; enlarged and caseous mesenteric glands; slight tuberculosis of lungs, thoracic glands and liver.

Emulsion of mesenteric glands.

PIG 10.

Subcutaneous.

Dose: 10 cc. (no T.B. found).

Killed, October 8, 1903 (126 days).

P.M.—Small local lesion; adjoining glands caseous and calcareous; tuberculosis of all the organs except the spleen.

PIG 12.

Subcutaneous.

Dose: 10 cc. (no T.B. found).

Killed, October 3, 1903 (121 days).

P.M.—Caseous local lesion; neighbouring glands caseous and calcareous; general tuberculosis.

CALF 104.

Subcutaneous.

Dose: 10 cc. (no T.B. found).

Killed. September 11, 1903 (99 days).

P.M.—Calcareous tubercles at seat of inoculation; calcareous prescapular gland; tubercles in liver, spleen, lungs, thoracic and two mesenteric glands.

CALF 106.

Subcutaneous.

Dose: 10 cc. (no T.B. found).

Killed, September 12, 1903 (100 days).

P.M.—Calcareous tubercles at seat of inoculation; prescapular gland caseous and calcareous. One calcareous tubercle in a mediastinal gland.

Emulsion of inguinal and hepatic glands and spleen.

PIG 14.

Subcutaneous.

Dose: 9,000 T.B.

Killed, January 25, 1904, (114 days).

P.M.—Small ulcerated fibro-caseous lesion; enlargement and caseation of inguinal, iliac, and gastric glands; a few tubercles in liver, about 30 in each lung; spleen unaffected; bronchial glands tuberculous.

PIG 16.

Subcutaneous.

Dose: 9,000 T.B.

Killed, January 16, 1904, (105 days).

P.M.—Ulcerated local lesion; enlargement and caseation of inguinal, iliac, and gastric glands; numerous tubercles in liver, spleen, lungs, and bronchial glands; tubercles on the diaphragm, pleura and meso-testis.

CALF 124.

Subcutaneous.

Dose: 9,000 T.B.

Died, December 12, 1903 (71 days).

P.M.—General tuberculosis.

CALF 130.

Subcutaneous.

Dose: 9,000 T.B.

Killed, January 15, 1904. (104 days).

P.M.—Small calcareo-caseous tumour; adjacent glands tuberculous; slight tuberculosis of liver, spleen, lungs and thoracic glands.

Culture.

PIG 98.

Subcutaneous.

Dose: 1·0 mg.

Died, March 29, 1905, (78 days).

P.M.—Small local lesion; one inguinal gland partly caseous; a few tubercles in the liver and spleen, and numerous tubercles in the lungs which showed areas of consolidation; tuberculosis of pleura and diaphragm.

PIG 96.

Subcutaneous.

Dose: 0·1 mg.

Killed, September 7, 1905, 240 days after.

P.M. — Small caseous lesion; neighbouring glands caseous; spleen contained three miliary tubercles, the liver numerous caseous nodules and the lungs sparsely scattered hard tubercles; the thoracic and several of the abdominal glands, and two glands in the neck were tuberculous.

CALF 230.

Subcutaneous.

Dose: 50 mg.

Died, February 13, 34 days after inoculation

P.M.—General tuberculosis.

RABBITS.

No.	Dose.	Method.	Duration of Life.	Result.
159	1·0 mg.	Intrav.	D. 21 days.	G.T.
158	1·0 mg.	Intrap. partly subcut.	D. 62 „	G.T.

Culture.

RABBITS.

Number.	Dose.	Method.	Duration of Life.	Result.
558	1·0 mg.	Subcut.	D. 81 days	G.T.
559	0·1 mg.	„	D. 137 „	G.T.
560	0·01 mg.	„	D. 106 „	G.T.
561	0·001 mg.	„	D. 50 „	G.T.

DETAILED DESCRIPTION OF EXPERIMENTS.

CHIMPANZEE PASSAGE.

VIRUS B. IV.

CHIMPANZEE 8.*

Fed on October 25th, 1904, with 107 c.c. milk of Cow 172, containing approximately 10,000,000 tubercle bacilli.

Died of broncho-pneumonia, March 21st, 1905. 144 days later.

Abstract of Post-Mortem Notes.

Pharynx and Glands in Neck normal ; the small intestine showed five tuberculous ulcers, varying from 3 to 4 mm. up to 2 cm. in diameter. Nine of the mesenteric glands were slightly enlarged and contained dense fibroid nodules, caseous in the centre and appreciably gritty from calcification. In the meso-colon there were three enlarged glands, close together ; two were caseous throughout and slightly gritty, the third was partly caseous. There was an abscess (not tuberculous) in the wall of the abdomen and broncho-pneumonic patches in the lungs.

A culture was isolated from one of the colic glands and inoculated into a series of six rabbits and chimpanzee 10 (by feeding).

Two of the rabbits died prematurely ; the rest died of general tuberculosis in periods varying from 16 to 64 days.

CHIMPANZEE 10.

(SECOND CHIMPANZEE PASSAGE OF VIRUS B. IV.)

Fed with culture derived from a colic gland of Chimpanzee 8.

Dose.—0·1 milligramme (4th generation, 24 days old).

Date.—July 16th, 1905.

Weight.—July 18th, 1905. 11 lbs. 14 ozs.

Died.—November 6th, 1905. 113 days after the experiment began.

Clinical History.

Temperature.—The temperature during the first six weeks varied between 37° and 38·4°, with the exception of two rises each to 39·1° on the twentieth and fortieth days respectively. On August 27th (42 days after feeding) there was a sudden rise of temperature to 40·3° C., and a rapid fall on the next day to 36·5° C. The temperature rapidly rose again, and varied for about a fortnight between 37·9° and 39·3° ; it then gradually became more and more irregular, and in the month of October showed considerable fluctuations, on several occasions rising more than 2°, and on two falling more than 3° C. (highest temperature 39°C.). In the last few days of life no temperature was recorded.

With the rise of temperature on August 27th the Chimpanzee was a little seedy for a day or two (loss of appetite, and depression) ; he recovered, but remained somewhat anæmic. During the month of October he gradually lost flesh and strength, and became extremely anæmic.

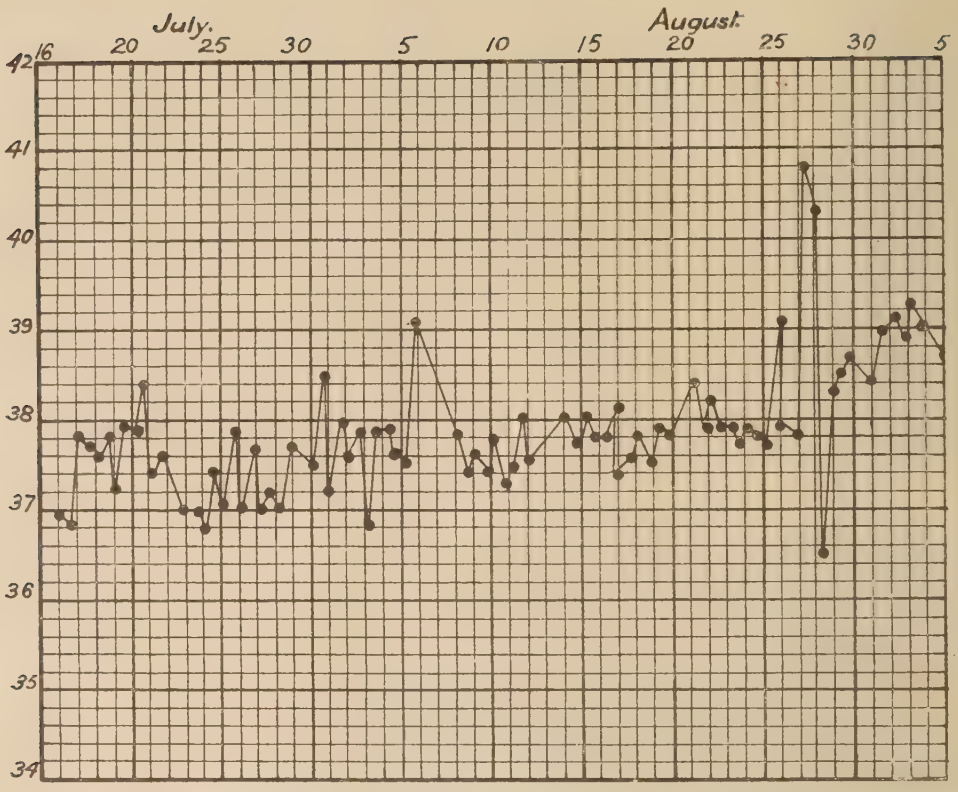
In the last week of life weakness and emaciation were extreme, and the eyelids in the morning were very œdematous.

No diarrhœa was noticed during the experiment, and there was no increase in respiration. On one occasion only was he heard to cough.

Weights.—During the first week there was a gain of 10 ozs. ; then for a fortnight the weight was stationary ; subsequently the animal steadily lost weight, at first slowly, then rapidly.

July 18th, 1905	11 lbs. 14 ozs.
July 25th, 1905	12 lbs. 8 ozs.
August 1st, 1905	12 lbs. 8 ozs.
August 8th, 1905	12 lbs. 8 ozs.
September 12th, 1905	12 lbs. 2 ozs.
October 5th, 1905	12 lbs. 0 ozs.
October 10th, 1905	11 lbs. 14 ozs.
October 24th, 1905	11 lbs. 0 ozs.
November 6th, 1905	9 lbs. 12 ozs.
Loss in weight on the original weight	2 lbs. 2 ozs.
Average loss per week from August 8th to November 6th	3·43 lbs.

* In the majority of these experiments the first animal of the series belongs to one or other of the feeding experiments and a full description of the post-mortem results will be found in Volume I. ; for convenience of reference a short abstract of the notes of these animals is given at the beginning of each series.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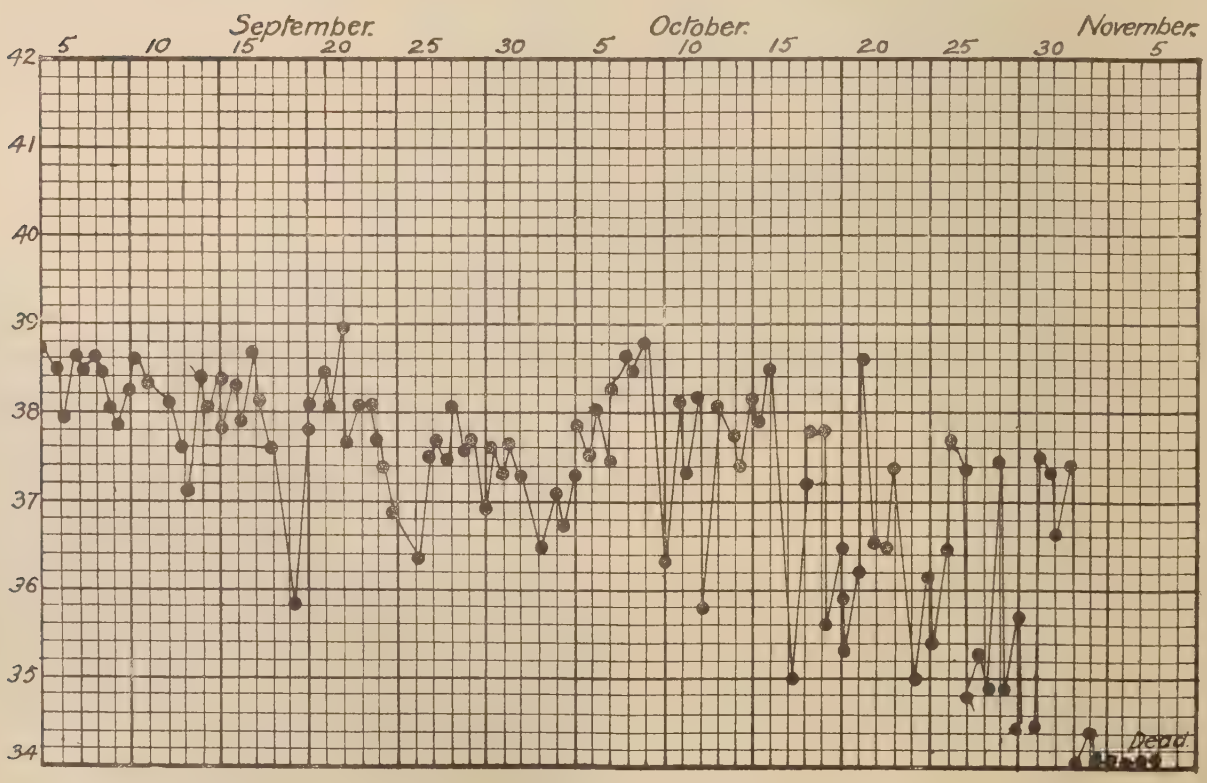
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Post-Mortem Examination.

Body.—Emaciated.

Tongue, Tonsils, Pharynx, Submental Glands, Submaxillary Glands, Parotid Glands, Retro-pharyngeal Glands.—Normal.

Cervical Glands.—Left upper cervical: one enlarged gland showed irregular caseous areas replacing most of the cortex; two smaller glands contained yellowish white softened nodules, each the size of a wheat grain.

Left lower cervical glands normal.

Right upper cervical; one showed in the cortex two softened caseous nodules, each 2 mm. in diameter; the others were normal.

Right lower cervical glands normal.

Abdomen.

Peritoneum.—There was a slight excess of fluid in the peritoneal cavity

Omentum.—The omentum along its attachment to the stomach was œdematous: it contained minute scattered pearly grey tubercles.

Stomach.—Normal.

Small Intestine.—The wall of the intestine was reduced in thickness, and in the situations of some of the larger ulcers presented saccular dilatations. The mucous membrane showed about fifty ulcers distributed along the whole length of the gut, the first situated five inches from the pylorus; twenty-five of these were large, ranging from 1 by 1 cm. to about 2·5 by 2 cm. in area (the latter almost completely encircled the gut); the rest were quite small.

The margins of the ulcers were raised, and the bases slightly thickened and fibrous, showed on the serous surface in many cases an eruption of hard raised tubercles, some pearly grey, others slightly opaque in the centre. In the larger ulcers the floors were irregular and somewhat nodular. None of the ulcers showed caseous deposit, but in some a few minute opaque foci were seen in the base and under the surrounding mucous membrane.

Around many of the larger ulcers there were numerous small ulcers; these in some cases had united with the large ulcer, causing the latter to have a sinuous outline.

Some of the Peyer's patches in the ileum were not uniformly ulcerated, but showed a number of small irregular ulcers, the mucous membrane between which was raised and thickened.

Large Intestine.—Showed one small ulcer, with slightly thickened margin and thin base; no subperitoneal tubercles.

Gastric Glands.—A small gland near the pylorus contained a soft white caseous nodule 1·5 mm. in diameter; the rest were normal.

Mesenteric Glands.—The mesenteric glands were all much enlarged; those in the upper part of the mesentery were fused together, and formed a firm nodular mass 1¾ in. by 1¼ in. by (about) 1 in.; the glands in the lower part of the mesentery were more or less moveable. On section through the large mass all the glands were seen to be caseous throughout and partially softened. They had thick fibrous capsules, which adhered together. The mesentery between this mass and the intestine was œdematous. The glands in the lower part of the mesentery were similarly affected; the caseated tissue was opaque, whitish, firm in most cases, but in some beginning to soften.

Around the margins of the mass of caseated glands were several œdematous glands, one of which contained a caseous focus.

Ileo-Colic Glands.—At the junction of the ileum and the colon there was a group of enlarged glands, some of which were normal on section, whilst others showed the substance replaced to a greater or lesser extent by opaque white caseated tissue, in one or two cases beginning to break down; one gland, the size of a large pea, was caseous throughout.

Colic Glands.—One colic gland contained a soft white caseous tubercle the size of a millet seed; another, the size of a small pea, was caseous nearly throughout. Other colic glands along the margin of the bowel appeared somewhat enlarged, but were not otherwise abnormal.

Spleen.—Was much enlarged (4½ in long by 1¾ in. broad), and was adherent to the wall of the abdomen at the level of the costal cartilages by a tough fibrous adhesion. On the surface were fairly numerous filmy grey tubercles, the largest a little over 1·5 mm. in diameter. On section the pulp contained moderately numerous spherical tubercles, each about one millimetre in diameter, with yellow softened centres and grey translucent margins. There were besides two or three soft caseous nodules with fibrous margins, the largest the size of a wheat grain.

Liver.—The substance was firm but very pale. On the surface were a few minute pearly grey foci, one of which was definitely raised above the surface. On section a few doubtful yellow points were seen.

Hepatic Lymphatic Glands.—A small gland on the duct from the gall bladder contained an opaque white pin-head sized tubercle. Two small glands on the common bile duct were normal.

Kidneys.—Were extremely pale; the capsules stripped readily. The left kidney showed on the surface about half a dozen slightly raised almost transparent tubercles, the largest about one millimetre in diameter. The right kidney showed a few similar tubercles.

Suprarenal Bodies.—Normal.

Pancreatic Glands.—On the upper border of the pancreas, near the duodenum, there was a small gland with a pin-head sized caseous tubercle. Three others further along the pancreas, each the size of a pea, were almost entirely composed of dense opaque white, or yellowish-white, caseated tissue.

Below the head of the pancreas there was a number of glands, ranging in size from a small pea to that of a swallow's egg; some of these were practically caseous throughout, others were partly caseous.

Lumbar Glands.—One on the right side, the size of a large pea, was indurated and extensively caseated; two or three other small glands contained each a caseous tubercle.

Thorax.

Pleura.—The parietal pleura was normal; the lymphatic fringes around the tendon of the diaphragm were slightly hypertrophied.

Lungs.—Were mottled with irregular purplish patches of congestion (pieces floated in water), and were closely and evenly beset with firm nodules, ranging in size from a pin's head to a small pea; the nodules were grey and translucent, the small ones showing a minute central opacity, the larger ones scattered opaque yellowish white points; some of the larger nodules were homogeneous throughout.

Bronchial Glands.—Were perhaps a little enlarged, but there were no foci of caseation.

The tissues of the posterior mediastinum were œdematous.

Heart.—The pericardial sac contained a quantity of clear serous fluid; the heart muscle and valves were normal.

Testicles.—Normal.

Iliac Glands, Inguinal Glands, Axillary Glands.—Normal.

Brain.—Normal.

Microscopical Examination.

Spleen.—Two tubercles were examined; no tubercle bacilli were seen, but there were a few organisms stained blue.

Left Superior Cervical Gland.—Two tubercle bacilli.

Pancreatic Gland.—Numerous tubercle bacilli.

Animals Inoculated.

With Tissue Emulsion.

Two guinea pigs, No. 1768 and 1769, were inoculated with an emulsion made from the lung; two, Nos. 1770 and 1771, from a mesenteric gland, and one, No. 1772, from the spleen; all died of general tuberculosis in periods varying from 15 to 40 days.

With Culture.

Seven rabbits inoculated with culture derived from a mesenteric gland died of general tuberculosi .

CHIMPANZEE 16.

Fed with culture from Chimpanzee 10.

Dose—0·01 milligramme.

Date—January 5th, 1906.

Weight—24 lbs. 10 ozs.

Died—April 6th, 1906. 91 days after the experiment began.

Clinical History.

The chimpanzee was seedy for the first three or four days, with diarrhœa and loss of appetite; the indisposition appeared after, and was probably due to the inoculation of tuberculin (2 cc.) on the day previous to the experiment.

The chimpanzee then remained in good health until about January 27, 1906, when she caught cold, probably from the attendant who was suffering from nasal catarrh at that time. During the next few days the chimpanzee lost appetite and ate nothing but fruit, and there was a constant discharge of muco-pus from the nostrils. Towards the end of this illness a cough developed and numerous râles and rhonchi were heard in the chest.

On February 5, 1906, she began to improve, and the appetite returned, but for a day or two there was still a hoarse cough.

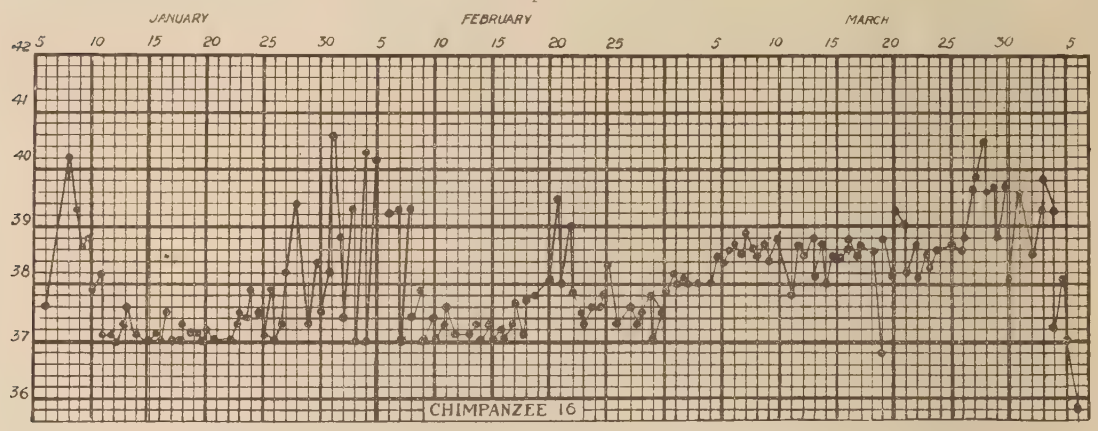
She remained well until March 12, 1906, when for a week she took scarcely any food in the morning and was much quieter than usual. She soon became obviously ill and gradually lost flesh and strength; she showed an increasing disinclination to exert herself and lay most of the day on her back.

On April 5, 1906, she was very weak and languid; respirations were not increased, but were accompanied in expiration by wheezing, probably from hypostatic congestion. She died on April 6, 1906, 91 days after she was fed.

Weights.

January 8th, 1906	-	-	24 lbs. 0 ozs.
January 25th, 1906	-	-	25 lbs. 2 ozs.
February 6th, 1906	-	-	24 lbs. 2 ozs.
February 21st, 1906	-	-	25 lbs. 8 ozs.
March 20th, 1906	-	-	24 lbs. 4 ozs.
April 6th, 1906	-	-	19 lbs. 4 ozs.
Loss in weight	-	-	4 lbs. 12 ozs.

Temperature.



POST-MORTEM EXAMINATION.

Body.—In fair condition but thinner than at beginning of experiment; moderate amount of fat in subcutaneous tissues.

Tongue, Tonsils and Pharynx.—Normal.

Submental, Submaxillary and Parotid Lymphatic Glands.—Normal.

The Right Pharyngeal Gland.—The size of a French bean, showed about three-quarters of its substance yellow, caseous and softened.

The Left Pharyngeal Gland was normal.

Cervical Glands.—On the right side one in the middle of the neck contained two yellow caseous miliary tubercles; the rest were normal. On the left side two small glands in the middle of the neck contained each a soft caseous nodule 1 to 1·5 mm. in diameter; other glands on this side were normal.

Abdomen.

There was a quantity of blood-stained serous fluid in the peritoneal cavity.

The Omentum.—Was moderately fatty and contained fairly numerous grey tubercles, somewhat irregular in outline, the largest a little over 1 mm. in diameter; most of these were translucent throughout, but a few were slightly opaque in the centre; there were besides a few larger ones with soft caseous centres.

Peritoneum.—The peritoneum covering the diaphragm and the lower ribs on the left side was covered with flattened grey tuberculous growths, forming on the tendinous part of the diaphragm an almost continuous layer and showing here and there opaque foci; elsewhere the parietal peritoneum was somewhat roughened, mainly on the left side, and showed one or two small tubercles with caseous centres.

On the meso-colon there were sparsely scattered miliary caseous tubercles.

The appendices epiploacæ were devoid of fat and along the first parts of the colon contained a varying number of hard grey tubercles, most of which were caseous in the centre.

Stomach.—Mucous membrane covered with mucus and partly digested; otherwise normal.

Small Intestine.—The duodenum below the pancreas was closely adherent to the enlarged mesenteric glands and along its mesenteric attachment was thickened and caseating; all the coats were infiltrated except the mucous which was not ulcerated.

The mucous membrane of the small intestine in the upper part was normal except for a flattened caseous nodule, about 5 mm. in diameter, with a small central ulcer.

About three feet from the pylorus there were two tuberculous ulcers close together, one nearly 1 cm. in diameter, the other smaller and more transverse; under the mucous membrane around the ulcers there were a few small caseous nodules, extending chiefly in a lateral direction. On the serous surface opposite these ulcers and for a good distance around them there was an eruption of caseating miliary tubercles.

Most of the Peyer's patches, from about the middle of the small intestine to its termination, were affected; five contained each a typical tuberculous ulcer with raised irregular caseous margins and slightly thickened fibroid base; on the serous surface opposite there were two or three hard tubercles; the ulcers varied from 4 to about 15 mm. in greatest diameter, the one in the terminal patch being larger and more irregular than the others; in the latter patch besides the ulcer there was a slightly raised yellow caseous nodule, the size of a split pea, with a central ulcer. The other affected Peyer's

patches contained from one to six yellow caseous nodules, ranging in size from 3 to 6 mm. in diameter; these resembled the ulcerated nodule in the terminal patch. Another patch contained one minute tubercle.

Large Intestine.—The mucous membrane showed numerous small hæmorrhages but no ulcers or nodules.

Gastric Glands.—Præcardiac glands normal. A gland in the lesser curvature of the stomach contained one soft yellow caseous nodule, 5 mm. in diameter and two small caseous tubercles; a smaller gland contained a miliary caseous tubercle.

Mesenteric Glands.—The mesenteric glands were much enlarged. In the upper part of the mesentery they were fused together to form a tumour, measuring 3 by 2 by 3¾ in. closely adherent at one part to the duodenum; the mass was nodular around the margins and showed on the peritoneal surface numerous slighty raised grey tubercles, the larger of which were slightly opaque in the centre; on section the glands were caseous and softened throughout and had thickened capsules. In the lower part of the mesentery there was a smaller mass, measuring 1½ by ¾ by ¾ in., the glands composing which were more or less moveable one over the other; on section they consisted of softened caseous material with a slightly greenish tinge. Between this mass and the ileo-colic glands there were two slightly enlarged glands each of which contained an irregular softened caseous nodule, the largest 4 mm. in diameter.

Ileo-colic Glands.—On one side of the junction of the ileum and cœcum there was a mass of glands measuring 1½ by 1 by ¾ in.; all these were caseous and softened throughout with the exception of a few small œdematous glands at one extremity which were not obviously tuberculous.

On the opposite side of the cœcum at the root of the appendix there was a group of three small glands; two were entirely caseous, the third contained discrete caseous tubercles.

Colic Glands.—Three colic glands, two on the ascending and one on the descending colon, were caseous and softened throughout; they ranged in size from a small to a large pea; all the other numerous small glands along the large intestine were normal.

The Cœliac and Lumbar Glands.—Were much enlarged, and formed an irregular mass situated between the pancreas and the vertebral column; the lumbar glands were all caseous throughout, whilst some of the cœliac glands were only partly caseous.

In close contact with the upper border of the head of the pancreas, there were two enlarged glands; the larger one contained several soft yellow caseous nodules, and some small tubercles; the smaller one showed an irregular caseous patch replacing about a quarter of the substance.

Spleen.—The spleen was much enlarged, measuring 5½ in. by 2½ in. by about 1 in. in greatest thickness. Projecting from the surface were numerous yellow nodules ranging in size from 1 to 5 or more millimetres in diameter; most of these were surrounded by broad, deeply congested, fibrinous fringes. On section the pulp was firm and dark red, and showed numerous soft yellow caseous nodules, up to 2 or 3 mm. in greatest diameter; these nodules were very irregular in outline, and in several places had coalesced or formed small groups; besides the yellow nodules there were numerous greyish tubercles, some just visible to the eye.

Splenic Lymphatic Glands.—One gland in the hilum of the spleen contained two or three irregular caseous tubercles; another showed a few caseous foci.

The Liver.—Was enlarged, soft and of a pale yellowish colour; on the surface there were numerous slightly raised, flattened tubercles, ranging from about 1 to 2 mm. in diameter; the smaller tubercles were transparent and glassy; the medium sized ones had minute opaque centres, whilst the larger ones were distinctly yellow in the centre. On section small caseous tubercles, the largest about 1 mm. in diameter were sparsely scattered throughout the substance; the cut surfaces showed also numerous transparent grey points.

Portal Glands.—In the portal fissure there were two glands, ranging in size from a pea to a kidney bean; one was caseous throughout; the other contained soft, caseous nodules.

The Kidneys.—Were very pale and appeared to be enlarged; each showed on the surface scattered punctiform hæmorrhages, and four or five miliary tubercles with irregular caseous centres and ill-defined grey margins; no tubercles were seen on section.

Suprarenal Bodies.—Normal.

Thorax.

Heart.—The pericardial sac contained a small quantity of blood-stained fluid; the pericardium was normal. The heart muscle under the pericardium was deeply hæmorrhagic; endocardium normal.

Pleura.—On the pleural surface of the diaphragm, on the left side, there were four flattened caseating nodules, the largest 3 mm. in diameter; the pleura on the muscular part of the diaphragm was covered with minute grey granules. Costal pleura normal.

Lungs.—The left upper lobe was slightly adherent to the chest wall; the lobes of each lung were adherent to each other by old fibrous adhesions. The ventral part of the left upper lobe was firm, dark red, and airless, and there were some red hepatised patches around the roots of the right middle and the left lower lobes; with these exceptions the lungs were crepitant. The lung tissue contained sparsely scattered hard tubercles, ranging in size from about 0·5 to 3 mm. in diameter; the tubercles showed a small amount of central caseation.

Bronchial Glands.—On the right side, one above the bronchus contained an irregular caseous tubercle, 1·5 mm. in diameter; others on this side were normal. On the left side one small gland above the bronchus showed a minute caseous tubercle; a gland below the bronchus on this side contained two miliary caseous tubercles; the rest appeared normal.

Under the pleura in the last interspace near the vertebræ, there was a soft caseating mass, the size of a broad bean.

Brain and Meninges.—Normal.

Various Lymphatic Glands.

One Right Iliac Gland was slightly enlarged, and partly caseous; other iliac glands were normal.

Inguinal Glands.—On the right side one contained two soft, yellow caseous nodules, 3 and 5 mm. in diameter; another contained a minute caseous tubercle; on the left side one contained a soft caseous nodule, 2 mm. in diameter; the rest were normal.

Axillary Glands.—Three glands on the right side contained each a miliary caseous tubercle; another contained a small soft caseous nodule. A gland on the left side was slightly enlarged and showed more than half its substance caseous and softened; other glands in this group appeared normal.

Microscopical Examination (Smear Preparations).

Nodule in Omentum.—Tubercle bacilli moderately numerous.

Inguinal Gland.—Tubercle bacilli moderately numerous.

Left Cervical Gland.—Tubercle bacilli moderately numerous.

Portal Gland (Emulsion).—Tubercle bacilli moderately numerous.

Spleen (Emulsion).—A few tubercle bacilli.

ANIMALS INOCULATED.

With Tissue Emulsion.

Two guinea-pigs inoculated subcutaneously, one from the spleen, the other from a lumbar gland, died of general tuberculosis in 32 and 31 days respectively.

Two rabbits inoculated subcutaneously, one with an emulsion from the spleen, the other with an emulsion from a lumbar gland, died of general tuberculosis in 67 days.

With Culture.

A culture isolated from the spleen of Chimpanzee 16 was used to inoculate a series of rabbits and Chimpanzee 22.

One of the rabbits died prematurely; all the others developed tuberculosis.

CHIMPANZEE 22.

Subcutaneous Inoculation of Culture (5th generation, 21 days old) derived from Chimpanzee 16.

Date—July 4th, 1906.

Dose—0·001 milligramme.

Died—September 29th, 1906.

Duration of Experiment—87 days.

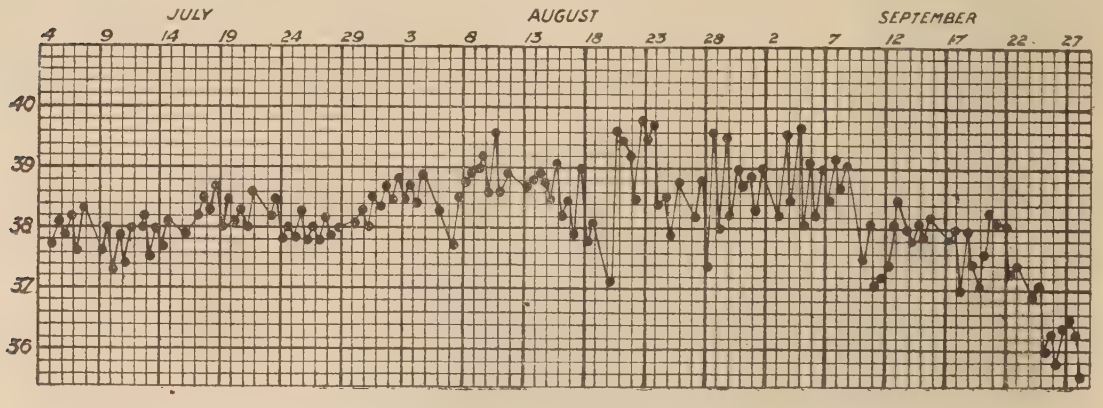
Clinical History.

The chimpanzee was inoculated in the subcutaneous tissue of the back on July 4th, 1906. One month later there was a soft slightly raised tumour about 2 in. in diameter, with a small ulcer in the skin at one margin from an opening in the base of which issued a purulent discharge: the axillary glands were not enlarged. Subsequently the ulcer in the skin became larger and the axillary glands enlarged. The first sign of ill-health appeared six weeks after inoculation and was shown by loss of appetite. From that time it slowly and progressively deteriorated in condition until its death.

Weights.

July 5th, 1906	13 lbs. 9 ozs.
September 29th, 1906	9 lbs. 8 ozs.
Loss in weight	4 lbs. 1 oz.
Average weekly loss	5·41 ozs.

Temperature.



POST-MORTEM EXAMINATION.

Condition.—Body thin; muscles of extremities wasted; very little subcutaneous and abdominal fat.

Local Lesion.—Just to the right of the middle line of the back and lying to some extent on the ala of the right scapula there was a slightly raised tumour measuring 1½ by 1½ by ½ in.; on section the tumour was found to be composed of muscular tissue closely beset with irregular caseous patches and tubercles and to contain near the skin a small compressed cavity lined internally with a thin layer of caseous substance; this cavity had leading from it to the surface a short sinus which opened at the margin of an ulcerated area ¾ in. in greatest diameter. The deeper muscles under the tumour were found to contain caseous tubercles and a soft caseous cord could be traced through an intercostal space into the thorax.

Axillary Glands.—In the right axillary region at about the middle of the margin of the pectoralis major there was a tumour measuring 1½ by 1 by ½ in. composed of several glands which had fused together; on section these glands consisted throughout of yellowish-white breaking down caseous substance; around this mass were numerous small glands, one of which was beset with minute caseous foci. Near the large mass, but lying more under the muscle, were two enlarged glands, each about 1 cm. in diameter, which showed on section yellowish white caseous patches of various sizes, undergoing softening. Near the apex of the axilla were four or five slightly enlarged glands; the largest of these was partly caseous, the others contained discrete caseous foci. The glands in the left axilla were normal with one exception, which contained a soft caseous nodule the size of a hempseed.

Thorax.

On opening the thorax the left lung collapsed and was nowhere adherent to the chest wall; the right lung was universally adherent to the parietal pleura, but could be fairly readily separated from it. The parietal pleura (costal and diaphragmatic) on the right side was thickened, greyish-yellow in colour and roughened on its internal surface.

Lungs.—Both lungs were crepitant; the lobes of the right lung were adherent.

The left lung contained moderately numerous evenly distributed hard grey nodules ranging in size from a pin's head to that of a hempseed; most of the smaller nodules were quite translucent, whilst the larger ones showed a relatively small amount of central caseation; the right lung contained similar but rather less numerous nodules.

Bronchial Glands.—On the right side the prætracheo-bronchial glands (three in number) were fused together to form a mass the size of a thrush's egg; on section they were yellow and caseous throughout and had thick fibrous capsules. On the left side one gland, the size of a sparrow's egg, showed about three-quarters of its substance replaced by soft yellow caseous substance.

The intertracheo-bronchial glands were normal.

The vertebral glands on the right side extending from the fourth to the last dorsal vertebra were enlarged, caseous and softened and embedded in dense white fibrous tissue.

A dorsal mediastinal gland at the level of the fifth vertebra was large, caseous and softened throughout.

A lymphatic gland on the diaphragm near the reflexion of caval fold of pleura contained a soft caseous tubercle.

Diaphragm.—The pleura on the left side of the diaphragm and the peritoneum on the right showed a number of filmy translucent growths varying in size up to about 3 mm.

Heart.—Normal.

Abdomen.

The omentum was devoid of fat and somewhat œdematous; it contained fairly numerous tubercles varying in size from a mere speck up to that of a millet seed.

Spleen.—The spleen was enlarged and measured 4 in. by 2¼ in. by about ½ in. in greatest thickness; on section the pulp contained numerous irregular soft yellow caseous areas ranging from about 2 to 10 mm. in diameter; the larger nodules were composed of firm reddish tissue

(firmer than the surrounding splenic pulp) containing soft yellow caseous areas; two of the smaller nodules projected from the surface and were surrounded by a fringe of congested fibrin.

Splenic Lymphatic Glands.—In the hilum of the spleen there were two enlarged glands, one the size of a broad bean, the other the size of a kidney bean; both on section showed irregular softened caseous areas extensively replacing the gland substance.

Liver.—The liver was paler than normal and showed on the surface about twenty flattened nodules, with overhanging margins, the largest 5 mm. in diameter; on section they were found to extend into the substance and to have yellow caseous centres and grey margins.

Under the capsule and throughout the substance were seen scattered tubercles up to 1 mm. in diameter; some of these were grey with opaque centres, whilst others were homogeneously caseous.

In the small omentum there was a group of four enlarged glands, the largest the size of a broad bean, which on section showed whitish caseous patches extensively replacing the gland substance.

In the hilum of the liver there was a gland, 1 cm. in diameter, similarly affected.

A gland near the neck of the gall-bladder contained a softened miliary caseous tubercle.

Kidneys.—The kidneys were extremely pale. The right showed in the cortex a pinhead-sized caseous focus. In the cortex of the left there were five or six nodules, the largest about 4 mm. in diameter; one of these appeared to be composed of three smaller nodules which had fused together and was completely caseous; the others had grey margins and irregular caseous centres.

Tongue, Pharynx, Tonsils, Larynx and Trachea.—Normal.

Small Intestines.—Three Peyer's patches in the ileum contained each a caseous focus.

Large Intestine.—Normal.

Gastric Glands.—A gland in the lesser curvature of the stomach was beset with soft caseous tubercles.

Mesenteric Glands.—In the anterior part of the mesentery a gland the size of a large pea was irregularly caseated; two smaller glands near the latter were extensively replaced by soft caseous nodules; two other glands in the middle of the mesentery contained each a miliary caseous tubercle.

Ileo-colic Glands.—Normal.

Colic Glands.—Two colic glands were affected; one contained two miliary caseous tubercles, the other contained one.

Lumbar Glands.—One lumbar gland contained a minute caseous focus; the others were normal.

Inguinal and Iliac Glands.—Normal.

Testicles and Brain.—Normal.

Microscopical Examination (Smear Preparations).

Spleen,
Bronchial Gland, } Numerous tubercle bacilli.

Intestines.—A few tubercle bacilli.

CHIMPANZEE PASSAGE.

VIRUS B. IX.

CHIMPANZEE 14.

Fed on September 11th, 1905, with 1·1 mg. of culture derived from the original material of Virus B. IX.

Died—November 13th, 1905. 63 days after.

Post-mortem Examination.

General tuberculosis; intestines and mesenteric glands severely affected.

A culture isolated from an ileo-colic gland was used to feed Chimpanzee 18 and to inoculate subcutaneously a series of rabbits.

One of the rabbits died in 25 days and showed slight tuberculosis; the others died of general tuberculosis in periods varying from 103 to 151 days.

CHIMPANZEE 18.

Fed with culture derived from Chimpanzee **14.**

Date—February 12th, 1906.

Dose—0·01 milligramme.

Weight—11 lbs. 8 ozs.

Died—June 8th, 1906. 116 days later.

Clinical History.

Towards the end of March a mass of enlarged glands was felt on the left side of the neck.

On April 19, 1906, the chimpanzee had a sharp attack of diarrhœa.

Note on April 29, 1906: "For three days has been very weak and apparently unable to walk about the cage; the weakness is manifest chiefly in the legs. The appetite is poor and has not been good for several days. The mass of glands completely fills the anterior triangle and enlarged glands are felt in the posterior triangle as well. The axillary glands are not perceptibly enlarged and no enlarged glands can be felt in the abdomen."

On May 4, 1906, a view was obtained of the left tonsil which was observed to be congested and ulcerated. The skin over the lower part of the mass of enlarged glands was slightly reddened but not adherent. The general condition of the animal was moderate and the appetite was fairly good; the chimpanzee was, however, much quieter and moved very little about the cage. During the rest of the experiment the chimpanzee gradually became more depressed and more and more disinclined to move about; the appetite was capricious but usually a fair quantity of food was taken and there was no apparent bodily wasting. On several mornings there was oedema of the eyelids and of the skin over the lower part of the glandular mass. There was no increase in the rate of respiration during the experiment.

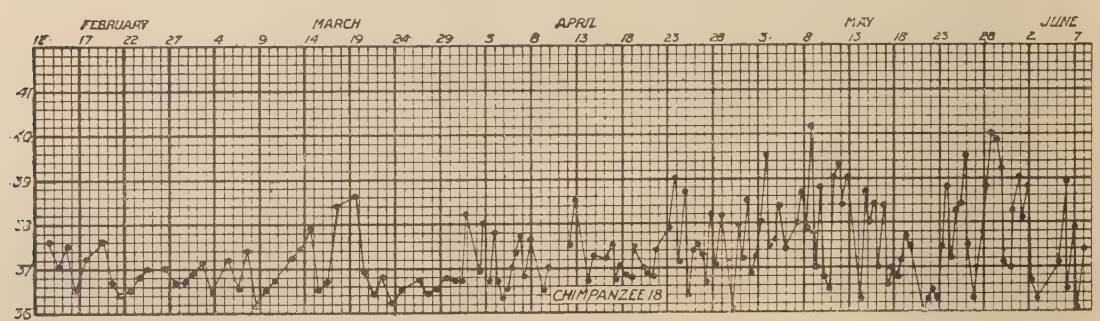
The chimpanzee died rather unexpectedly on June 8, 1906, 116 days after the experiment began.

Weights.

February 21st, 1906	11 lbs. 8 ozs.
March 20th, 1906	12 lbs. 6 ozs.
April 18th, 1906	11 lbs. 2 ozs.
May 15th, 1906	11 lbs. 0 oz.
June 5, 1906	9 lbs. 8 ozs.
June 8, 1906	10 lbs. 4 ozs.
Loss in weight	1 lb. 4 ozs.

Tuberculin Test.—April 10th, 1906. Fifty-seven days after the experiment began. Positive. Rise of temperature 2·3° C. Maximum 40·5° C.

Temperature.



Post-mortem Examination.

Condition.—Arms and legs thin; muscles poorly developed; abdomen full, not distended.

Tonsils.—In the region of the left tonsil there was a deep ulcer with caseo-necrotic walls. The right tonsil was not apparently ulcerated. the surface was irregular and the crypts were filled with whitish purulent material.

Pharynx and tongue appeared normal.

Lymphatic Glands of the Neck.—In the left anterior triangle there was a large mass of enlarged glands extending from the level of the angle of the jaw to the clavicle; in the upper part of the triangle the glands, superior cervical and retropharyngeal, had fused together and formed a soft fluctuating tumour the size of a small orange; over this swelling the sterno-mastoid muscle was stretched; on section the tumour was filled with breaking down caseous material: the glands between this tumour and the clavicle (lower cervical) were enlarged and caseous; around the fluctuating tumour were several isolated enlarged glands; some of these were caseous throughout; others showed caseous patches.

In the left posterior triangle there was a smaller mass of fused glands; on section all were caseous throughout and some were softened.

On the right side in the anterior Triangle there was a mass of glands (superior cervical) measuring $1\frac{1}{4}$ by $\frac{3}{4}$ by $\frac{1}{2}$ in.; on section the glands composing the mass were caseous throughout and beginning to soften. Below this there was a chain of slightly enlarged glands running under the sterno-mastoid; on section these were free from caseation. The retropharyngeal gland was not apparently affected.

Abdomen.

On cutting through the wall of the abdomen the omentum was found to be very intimately adherent to the parietal peritoneum, in several places so firmly that it was impossible to separate them one from the other.

The Peritoneal Cavity.—Contained a small quantity of slightly turbid fluid.

The Omentum.—Was studded with greyish-white somewhat translucent nodules, ranging in size from a mere point to that of a hempseed; some of the larger of these showed minute opaque points in the centre, the others were homogeneous throughout. Along the free margin of the omentum the nodules were aggregated together and formed irregular nodular masses composed of a firm greyish translucent tissue containing sparsely scattered caseous foci.

The Mesentery.—Was covered with similar nodules, the largest the size of a hempseed, here and there aggregated together; a few of the larger ones were definitely caseous in the centre.

There were a few similar tubercles on the serous surfaces of the stomach and small intestine.

On the large intestine there were rather numerous nodules along the line of attachment of the mesocolon; there were numerous nodules also on the mesocolon itself; near the junction of the ileum and cœcum the nodules were closely aggregated together.

All the appendices epiploacæ on the large intestine were devoid of fat and filled with greyish-white miliary tubercles, which in several had fused together to form firm nodular masses of various sizes.

The Parietal Peritoneum.—Was covered with flattened nodules similar in appearance to those on the mesentery; on the anterior abdominal wall they formed an almost continuous greyish-white layer with a yellowish mottling; in the flanks they had united together to form irregular nodular masses similar to those in the omentum; to these masses the large intestine was adherent.

The Peritoneal Surface of the diaphragm showed on the right side irregular plaques of aggregated tubercles, and on the left a uniform greyish-yellow layer of tuberculous tissue; the latter extended over the peritoneum covering the ribs.

Stomach.—Normal.

Small Intestine.—The upper part of the small intestine was normal. Twelve of the Peyer's patches in the ileum showed typical tuberculous ulceration; the ulcers varied from about 5 to 10 mm. in diameter and were mostly circular; the ulcers in the first six patches were caseous chiefly under the margins, two only showing caseous tubercles in their thickened fibroid base; in one of the patches, besides a large ulcer, there were three caseous nodules, the largest of which was ulcerated; on the peritoneal surface of one of the ulcers there were a few raised tubercles; five of the patches in the lower part of the ileum were enlarged, yellow and caseous throughout, and presented nodular elevations on the serous surface; the mucous membrane over some of the patches was almost completely ulcerated, in others only slightly.

The terminal Peyer's patch showed one small ulcer with caseous margins. The large intestine was adherent at several places to the wall of the abdomen; the mucous surface appeared normal.

Gastric Glands.—Præcardiac glands normal.

Duodenal and Pancreatic Glands.—Normal.

Mesenteric Glands.—The glands in the lower part of the mesentery were enlarged and fused together to form a lobulated yellow mass, measuring about 2½ in. by 2½ in by 1 in.; on section the substance of each gland was found to be converted into a soft caseous pulp. In the upper part of the mesentery the glands were normal in size and appearance, with one exception; this was slightly enlarged and showed in the cortex a yellow patch of softening.

In the mesentery near the ileo-cœcal junction there was a small mass of glands, more or less movable on each other; on section some of the glands were caseous and softened practically throughout, whilst the others showed lesser degrees of caseation. Quite near the junction were two isolated glands; one the size of a pea, almost entirely caseous; the other free from caseation.

Colic Glands.—In the mesentery of the ascending colon there was a yellow fluctuating mass, the size of a thrush's egg, around the margins of which were numerous small glands which were normal in appearance; on section the mass was composed of three or four caseous and softened glands which had fused together; near this group there was a pea-sized caseous and softened gland.

On the transverse colon there were two glands close together, the largest the size of a pea; both of these were caseous and softened throughout Other glands along the margin of the colon and the rectal glands appeared normal.

At the root of the meso-colon near the spine there was a group of glands; four of these were about 8 mm. in diameter and showed the substance partly or wholly caseous; two contained each a small nodule; a few others were normal.

Spleen.—The spleen was intimately adherent to the parietal peritoneum and was removed from it only with difficulty; it was much enlarged, measuring 5 by 2½ by about 1 in. in greatest thickness; on its convex surface there was a greyish adherent membrane and two raised firm yellow caseous masses. On section the pulp was closely beset with small irregular soft yellow caseo-purulent nodules, here and there aggregated together to form a larger nodule or a cluster, the individuals forming which were separated by a small amount of splenic tissue.

Splenic Lymphatic Glands.—One the size of a pea was caseous and softened practically throughout; another was partly caseous; others were a little enlarged but were not otherwise abnormal.

Liver.—The capsule of the liver on the right side was intimately adherent to the parietal peritoneum, and on removing the organ was completely stripped from the anterior surface of the right lobe. On the posterior surface of the liver there were numerous flattened slightly raised nodules, the majority not larger than 2 mm. in diameter, which resembled the peritoneal nodules; on the left lobe there were some larger nodules, irregular in outline, ranging up to 1 cm. in diameter; these were greyish-white, and caseous in the centre and penetrated varying distances into the substance of the liver. On the anterior surface the capusle that remained showed numerous small nodules, and one larger more raised and more opaque nodule with a connective tissue outgrowth from its surface; in the substance underneath this there was a soft caseous nodule 4 mm. in diameter.

The liver substance was pale and showed on section sparsely scattered translucent tubercles and a few caseous nodules, some of which were bile-stained. A gland in the hilum of the liver showed two or three tubercles. Two glands in the small omentum were enlarged and contained a few soft yellow nodules.

Kidneys.—The kidneys were very pale; the left showed three translucent nodules, the largest the size of a hemp seed; the latter was slightly opaque and yellowish in the centre.

The right kidney showed three nodules on the surface similar to those in the left and two or three small transparent tubercles; the latter were seen with difficulty on account of the pallor of the tissue surrounding them.

On section one small tubercle was seen in the depth of the left kidney and two or three in that of the right.

Suprarenal Bodies.—Normal.

Thorax.

Each pleural cavity contained a small quantity of clear serous fluid.

Lungs.—The lungs were expanded, congested and œdematous; they contained scattered nodules, ranging in size from a millet seed to that of a pea, evenly distributed throughout the parenchyma; the larger ones were firm yellow and caseous throughout, the smaller ones were grey and translucent or showed minute opaque central foci; there were no areas of consolidation.

Bronchial Glands.—On the left side one in the root of the lung contained a soft caseous tubercle; another near the latter, much enlarged, showed no foci of caseation; other bronchial glands were a little enlarged and firmer than normal, but no foci of caseation were seen.

The Ventral Mediastinal Glands on each side were enlarged, firm and contained each a yellow caseous nodule and several small tubercles.

Dorsal Mediastinal Glands.—Normal.

Pleura.—The parietal pleura on the left side showed one pin-head sized tubercle.

On the pleural surface of the diaphragm there were numerous hard flattened tubercles similar to those on the omentum ; they were most numerous on the left side, the largest being about 2 mm. in diameter. Under the pleura covering the diaphragm were three yellow projecting caseous nodules (? tuberculous glands), the largest the size of a kidney bean.

Near the apex of the heart there was a small gland in a state of early caseation.

Heart.—The pericardial sac was filled with clear serous fluid. The heart muscle and valves were normal.

Inguinal and Axillary Glands.—Normal.

The glands along the abdominal aorta were normal. Under the periosteum about the middle of the parietal suture there was a slightly projecting nodule nearly 1 cm. in diameter ; on section this contained soft caseous material ; the bone beneath it was slightly necrosed.

Brain and Meninges.—Normal.

Microscopical Examination (Smear Preparations).

Right Tonsil.—Numerous tubercle bacilli.

Liver (Nodule in Depth).—A few tubercle bacilli.

Emulsion of Lung.—Numerous tubercle bacilli.

Emulsion of Spleen.—A few tubercle bacilli.

Nodule on Parietal Bone.—Numerous tubercle bacilli.

Animals Inoculated.

A rabbit inoculated intraperitoneally with a small dose of an emulsion made from the lung and killed 76 days later, showed general tuberculosis of the usual type.

A rabbit inoculated intraperitoneally with a small dose of an emulsion made from the spleen and killed 76 days later, showed general tuberculosis, not very severe.

BABOON PASSAGE.

VIRUS B. I.

BABOON 8.

Fed on January 22nd, 1904, with milk of Cow 64 (Table 1A, Volume I.) containing 1,000,000 tubercle bacilli.

Killed.—April 28th, 1904. 97 days later.

Abstract of Post-Mortem Notes.

Ulceration and caseation of both tonsils. Great enlargement and caseation of the pharyngeal, and superficial and deep cervical glands; caseous foci in submaxillary lymphatic glands; one caseous nodule in the right submaxillary salivary gland, and several in the right half of the thyroid. Intestines and mesenteric glands normal. The liver showed several caseating nodules, and a number of miliary tubercles. The spleen contained numerous large softened caseous nodules; the kidneys a few small tubercles. The lungs showed numerous large firm caseous nodules; the bronchial glands caseous foci. There was miliary tuberculosis of the peritoneum, omentum and diaphragm.

A rabbit inoculated intraperitoneally with an emulsion made from a cervical gland was killed, when dying, 88 days after; the post-mortem examination showed general tuberculosis.

A culture was isolated from the cervical gland of Baboon 8 and after cultivation for 533 days was used to feed Baboon 64.

BABOON 64.

Fed with culture from Baboon 8.

Dose.—0·1 milligramme.

Date.—October 13th, 1905.

Died.—January 22nd, 1906. 101 days later.

Clinical History.

The baboon remained lively and well until the end of December. She then became quieter and more irritable and the appetite was not so good. During the first fortnight in January these symptoms became more pronounced and she was obviously losing flesh; the appetite was much diminished.

Towards the end of the experiment she became very emaciated and weak. During the experiment the respirations were never increased and there was no diarrhœa.

Tuberculin Test.—November 24th, 1905, 42 days after feeding. Positive. Rise of temperature 1·7° C.

Post-Mortem Examination.

Body emaciated; no subcutaneous fat.

Both Tonsils were ulcerated and entirely destroyed by caseation; the margins of the ulcers were undermined and the edges congested; the floors were covered with soft caseo-pus, dark coloured on the surface (result of haemorrhage).

Tongue.—Most of the solitary lymphatic follicles at the base of the tongue were caseous and two were ulcerated.

Pharynx.—Normal.

Right Submaxillary Lymphatic Glands.—Normal.

On the right facial vein there were two small caseous tubercles, and on the lower part of the right masseter muscle a caseous gland, the size of a wheat grain.

On the left side there were six enlarged submaxillary glands, the largest the size of a large pea; these on section showed soft caseous nodules, in some replacing almost the whole of the gland substance.

The Left Submaxillary Salivary Gland contained a caseous nodule, about 1·5 mm. in diameter.

The Salivary Gland on the right side was normal.

Retro-Pharyngeal Glands.—Behind each tonsil there was a large irregular mass, the size of a pigeon's egg, consisting of several glands fused together; on section they were all caseous throughout and softened.

Cervical Glands.—In the anterior triangle on the left side and under the insertion of the sterno-mastoid there was a group of enlarged caseous and softened glands; the largest one was the size of a pigeon's egg.

Under the sterno-mastoid in the lower part of the neck there was a chain of enlarged glands, ranging in size from a pea to that of a sparrow's egg; in the posterior triangle under the anterior border of the trapezius there was a gland the size of a sparrow's egg; all these glands were caseous and softened throughout. Two small glands under the middle of the clavicle contained small caseous nodules.

On the right side an upper cervical gland, the size of a French bean, showed caseous areas replacing almost the whole of the substance. A mid cervical gland, slightly enlarged, contained three soft caseous nodules, the largest 3 mm. in diameter. The lower cervical glands on this side were normal.

Abdomen.

The Peritoneal Cavity contained several cubic centimetres of turbid serous fluid.

The Omentum was drawn up and was adherent in several places to the floor of the abdomen and the intestines; it was congested and closely beset with caseous tubercles, the largest 3 mm. in diameter; along the free border the tubercles were closely aggregated together and formed firm masses. The omentum surrounded the spleen which it enclosed in a complete bag; this part of the omentum was also beset with caseous nodules and showed several firm masses composed of aggregated caseous nodules embedded in reddish tissue; the omentum was adherent to the spleen along the hilum and to several projecting caseous nodules.

The Parietal Peritoneum was covered with slightly raised yellowish white caseous tubercles, the largest about 2 mm. in diameter; they were most numerous towards the pelvis and on the diaphragm where they showed a tendency to coalesce.

There were fairly numerous caseous tubercles, up to 2 mm. on the mesentery, and also fairly numerous miliary caseous tubercles on the serous surface of the ileum; on the rest of the intestine they were more sparsely scattered.

Stomach.—Normal.

Small Intestines.—The mucous membrane of the duodenum showed two small ulcers, one with congested margins; in the rest of the intestine there were six typical tuberculous ulcers, majority situated in the ileum; five of these were transverse and ranged from 8 to 10 mm. in length; the margins were thickened and congested and the base contained caseous nodules which projected from the peritoneal surface. The sixth ulcer, situated about three feet from the ileo-cœcal valve, was large and irregular and had congested margins and yellow necrotic base, which was adherent to the peritoneum

over the lumbar vertebrae. The margin of the ileo-coecal valve showed an irregular ulcer with congested margins and caseous base.

Large Intestine.—Normal.

Gastric Glands.—A præcardiac gland contained a small caseous nodule.

Mesenteric Glands.—In the lower part of the mesentery there was an irregular nodular mass of enlarged glands, measuring 1½ by 1½ by nearly 1 in.; on section the glands composing the mass were caseous throughout and had thickened fibrous capsules. The glands in the upper part of the mesentery were not much affected; one was slightly enlarged and caseous throughout; one or two others contained discrete tubercles.

Ileo-Colic Glands.—On one side of the junction of the ileum and coecum there was a group of three glands; two were caseous throughout and fused together; the third was œdematous and deeply congested but free from tubercles. On the opposite side two small glands contained caseous tubercles.

Colic Glands.—Two mid-colic glands lying close together were enlarged to the size of a pea; one was partly caseous, the other was caseous throughout. Other colic glands were normal.

Pancreatic Glands.—Around the head of the pancreas there were several enlarged glands, the largest the size of a sparrow's egg; all were caseous and softened throughout. Along the superior border of the pancreas there were several normal glands.

The Spleen contained two large hydatid cysts; one measured 2½ in. by 2½ in. by 2 in.; the other 1½ in. in greatest diameter; only a small amount of splenic tissue remained; this contained a number of caseous nodules, varying much in size, the largest being about the size of a thrush's egg; the majority were flattened out on the surface of the large cyst from which they projected; on section they were soft, yellow, and caseous throughout. Two splenic lymphatic glands contained caseous tubercles.

The Liver.—Was soft and contained five or six hydatid cysts, the largest the size of a hen's egg; scattered throughout the substance were caseous tubercles, with grey margins, mostly about 1 mm. in diameter; a few were larger and measured 2 mm. in diameter.

The Portal Gland was enlarged and showed more than half the substance caseous.

Kidneys.—The right kidney showed on the surface two caseous miliary tubercles; in the depth of the cortex there was a caseous nodule 3 mm. in diameter. In the cortex of the left kidney there was a caseous nodule, 3 mm. in diameter, projecting from the surface.

Suprarenal Bodies.—Normal.

Thorax.

Heart.—Normal.

Pleura.—On the costal pleura there were a few flattened caseous nodules, each about 2 mm. in diameter.

On the pleural surface of the diaphragm, around the margins of the tendon, there was a number of caseous tubercles in several places arranged in clusters and surrounded by reddish granulation tissue; on the pleura covering the muscle of the diaphragm, chiefly on the left side, there was an eruption of grey granules.

Lungs.—In each of the three lobes of the right lung there was a hydatid cyst; they ranged in size from a pigeon's egg to a hen's egg; in the right cephalic lobe there was a smaller cyst; the lungs also contained fairly numerous firm caseous nodules evenly distributed, ranging in size from about 2 up to 8 mm. in diameter; the nodules were caseous throughout with the exception of a narrow grey or greyish red margin.

Bronchial Glands.—A superior bronchial gland on the right side contained two caseous foci. Other bronchial glands appeared normal. The ventral mediastinal glands were enlarged and closely beset with discrete caseous tubercles.

Special Lymphatic Glands.

Axillary and Inguinal Glands.—Normal.

Lumbar Glands.—Two, the size of sparrows' eggs, were caseous throughout; several others contained discrete caseous tubercles.

Iliac Gland.—Normal.

Uterus.—Normal.

Brain and Meninges.—Normal.

Microscopical Examination (Smear Preparations).

Cervical Gland, Lung. } A few tubercle bacilli.

Guinea-pig Inoculated.

A guinea-pig inoculated with an emulsion made from the lung died 8 days afterwards, too soon for tuberculosis to have developed.

BABOON PASSAGE.

VIRUS B. IV.

BABOON 22.

Fed for 25 days with tuberculous milk of Cow 172 (Table IVa, Volume I) from September 9th, 1904, until October 4th, 1904.

Died.—December 8th, 1904. 90 days after the experiment began.

Abstract of Post-Mortem Notes.

Both tonsils were enlarged but not ulcerated; the right showed in the centre a soft caseous mass; the left two caseous foci; the lymphoid follicles at the base of the tongue were enlarged, caseous, and ulcerated. The mucous membrane of the stomach showed six small tuberculous ulcers. The small intestine was severely affected; the upper half was covered with typical tuberculous ulcers with sub-peritoneal tubercles; the lower half was affected chiefly in the last 12 inches, where there were several groups of ulcers in the Peyer's patches. In the first part of the large intestine there were six ulcerated caseous nodules. Pharyngeal, superior cervical, ileo-colic, several colic, and the lumbar glands were greatly enlarged and caseous throughout. The mesenteric glands formed a very large irregularly softened caseous mass adherent in its upper part to the duodenum into which it had ulcerated. The mesentery corresponding to the upper half of the intestine was greatly thickened and tuberculous. Omentum and peritoneum studded with tubercles. Lungs crammed with caseous nodules, the largest the size of a large marble. Bronchial glands only slightly affected. The spleen contained numerous large softened caseous nodules; the liver caseous nodules up to the size of a pea and innumerable minute grey foci. The left kidney contained two caseous nodules, the right one. Diaphragm covered with tuberculous growths chiefly on the right side. One ileo-sacral, two splenic, and two left submaxillary glands contained caseous tubercles; the portal glands were beset with caseous nodules.

Animals Inoculated.

A culture was isolated from a cervical gland and, when it had been in cultivation 292 days, used to inoculate three rabbits and to feed Baboon 62.

No. of Rabbit.	Initial Weight in grammes.	Final Weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	RESULT.
481	1,330	1,020	1·0 mg.	Intraperitoneal	Died 25 days	General tuberculosis of the usual acute type.
482	1,280	1,070	0·1 mg.	Intraperitoneal	32 days (killed accidentally)	General tuberculosis, almost as severe as in Rabbit 481.
480	1,200	1,050	0·01 mg.	Intraperitoneal	Died 37 days	General tuberculosis; lungs packed with caseous coalescing tubercles. Tubercles not so numerous on the peritoneum as in the first two cases.

BABOON 62. (Adult Male).

Fed with culture from Baboon 22.

Dose.—0·1 milligramme.

Date.—September 26th, 1905.

Died.—February 9th, 1906. 136 days after feeding.

Clinical History.

During the first seven weeks his condition was unchanged. About the middle of November he was quieter and more tractable, and towards the end of November he began to have difficulty in breathing, especially after food, due, apparently, to enlargement of the retro-pharyngeal glands.

He then (from about the middle of December) began gradually to lose flesh and strength; the appetite, however, remained undiminished.

The glands in the neck became more and more enlarged and caused greater and greater difficulty in breathing; ordinary quiet respiration was accompanied by a snoring noise, which was very pronounced during sleep and when drinking.

At the end of January he was very thin and weak and moved very little about his cage.

On the day preceding his death there was a hæmorrhagic discharge from the nostrils.

During the experiment there was no increase in the rate of respiration, and no diarrhœa.

At the beginning of the experiment an attempt was made to get a record of the temperature, but was discontinued on account of the animal's uncontrollable struggles.

Post-Mortem Examination.

Tongue.—Normal.

Both Tonsils were slightly enlarged and composed of firm greyish red tissue, containing irregular patches of caseation; there was no ulceration.

Submaxillary Glands.—On the right side three, anterior to the salivary gland, were slightly enlarged and contained from one to three soft caseous nodules, the largest about 3 mm. in diameter. In a gland near the angle of the jaw there were four miliary caseous tubercles. On the left side one gland contained a pea-sized softened caseous nodule.

Submaxillary Salivary Glands.—Normal.

Retro-Pharyngeal Glands.—The pharyngeal glands were very greatly enlarged, and formed on each side a large irregular mass extending from the base of the skull to the level of the larynx. The naso-pharynx was almost completely filled in. The two masses had united in the midline in the upper part; the wall of the pharynx here showed a deep necrotic ulcer from which recent hæmorrhage appeared to have taken place. On section the glands on each side were fused together and were caseous and softened throughout.

Cervical Glands.—On the left side in the middle of the neck there was a large soft mass measuring $1\frac{3}{4}$ in. by $\frac{3}{4}$ in. by about $\frac{2}{3}$ in., which on section was found to be composed of several glands fused together; these were all completely caseous and softened. Internal to the carotid artery on the level of the larynx there was a gland the size of a pigeon's egg, which on section was a thin-walled cyst filled with caseo-pus (immediately above this gland were the enlarged pharyngeal glands); between this and the clavicle along the carotid artery there was a chain of small glands which were normal in appearance. Behind the left clavicle there were three small glands beset with soft caseous tubercles and a gland

the size of a broad bean, composed of greyish-red translucent tissue closely studded with soft caseous tubercles, up to 1·5 mm. in diameter.

In the middle of the neck on the right side external to the carotid artery there was a large soft gland measuring 1¾ in. by 1 in. by 1 in. which on section was filled with caseo-necrotic masses and caseo-pus; internal to the carotid artery there was a gland the size of a sparrow's egg, which was caseous and softened practically throughout (immediately above this were the enlarged pharyngeal glands).

Behind the outer extremity of the clavicle on the right side there were two glands, each 1 cm. in greatest diameter, filled with caseous and softened nodules which almost completely replaced the gland substance.

Abdomen.

The Peritoneal Cavity contained a small amount of blood-stained fluid.

The Omentum was very closely studded with yellow, caseous, and softened nodules ranging in size from a No. 4 shot to a barley grain; in many parts of the omentum the nodules were closely aggregated together; the lower part of the omentum showed a large irregular mass, composed of reddish fibroid tissue, closely beset with caseous softening nodules up to 2 mm. in diameter.

On the meso-colon and serous surface of large intestine there were fairly numerous caseous nodules, similar to those in the omentum. There were similar but less numerous nodules on the mesentery; these stood up prominently above the surface, and in many places occurred in small clusters.

The Parietal Peritoneum showed chiefly on the right side and on the diaphragm fairly numerous somewhat flattened caseous nodules; on the left side and in the right lumbar region there was besides an eruption of minute transparent granules.

Stomach.—The mucous membrane showed numerous punctiform hæmorrhages; otherwise normal.

Small Intestine.—In the early part of the ileum there was a small transverse healing ulcer with congested margins; in the base there were about half a dozen caseous tubercles which projected from the serous surface. In another part of the ileum there was a caseous tubercle about 1·5 mm. in diameter, which projected beyond both surfaces: the mucous membrane was not ulcerated.

Large Intestine.—Except where it was adherent to the ileo-colic glands, was normal.

Gastric Glands.—A gland in the lesser curvature of the stomach was enlarged, and contained three soft, yellowish, caseous nodules, up to 3 mm.

Duodenal Glands.—On the first part of the duodenum above the head of the pancreas there were several glands; one contained a caseous tubercle; the others were normal. Below the head of the pancreas there was a group of enlarged glands, all of which were caseous and softened throughout.

Mesenteric Glands.—The mesenteric glands were enlarged and fused together to form a smooth fluctuating tumour 2 in. by 2 in. by 1½ in.; on the surface a few caseous nodules projected from under the peritoneum; on section the tumour was a very thin-walled cyst filled with caseo-pus.

Ileo-Colic Glands.—The glands on one side of the junction of the ileum and the cœcum formed a large, irregular, caseous and softened mass which was adherent to the cœcum in two places: at one of these places the mass had ulcerated into the lumen of the gut, the mucous membrane showing three separate openings through which caseo-pus could be expressed.

On the opposite side of the junction there were two slightly enlarged glands which contained a few soft caseous nodules.

Colic Glands.—Three showed soft caseous nodules in the cortex; a few others showed one or two early tubercles.

The Spleen was enlarged and measured 3½ in. by 1½ in. by 1; on the surface there were a few flattened caseous nodules; the pulp contained very numerous thin-walled nodules filled with yellow tenacious pus (mucoid in character); they ranged in size from a shot to a large pea.

In the hilum of the spleen there was a caseous and softened gland, the size of a French bean.

Liver.—On the surface under the capsule there was a moderate number of caseous nodules, with fibrous margins, ranging from 1 to 2 mm. in diameter: in the depth no large nodules were seen, but extremely minute grey tubercles just visible to the eye and a few miliary tubercles with slightly opaque centres.

The Portal Gland was enlarged and contained in the cortex two or three soft caseous nodules.

The Left Kidney showed in the cortex three caseous nodules, 1·5 to 3 mm. in diameter.

The Right Kidney contained one soft caseous nodule 2·5 mm. in diameter.

Suprarenal Bodies.—Normal.

Thorax.

Heart.—The pericardium was universally adherent to the heart, the pericardial sac being obliterated; the heart muscle and valves were normal.

Lungs.—The left lung showed numerous thin fibrous adhesions to the costal pleura and diaphragm; the two lobes were intimately adherent.

The right lung was firmly adherent to the diaphragm and showed more recent adhesions to the ribs.

The right upper and lower lobes were crepitant, except for a patch of red hepatisation in the latter, and contained numerous caseous nodules with narrow grey margins, ranging from 1 to 3 mm. in diameter; in the upper lobe there was besides a caseous nodule 1 cm. in greatest diameter. The right middle lobe was completely consolidated and externally had a dark red colour; on section it was beset with caseous nodules, which near the root of the lobe were closely aggregated together and much softened.

The left lobes contained numerous caseous nodules similar to those in the right; there were besides four larger nodules, one measuring 1·5 cm in greatest diameter.

Pleura.—There were no tubercles on the pleura.

Bronchial Glands.—The prætracheo-bronchial glands were enlarged and contained discrete caseous nodules up to 2 mm. in diameter; one intertrachea-bronchial gland on the right side was much enlarged, œdematous and congested, and contained similar nodules.

Dorsal Mediastinal Glands.—Normal.

Ventral Mediastinal Glands.—Enlarged and very closely beset with discrete soft caseous nodules ranging up to 2 mm. in diameter.

Larynx and Trachea.—Normal.

Special Lymphatic Glands.

Cœliac Glands.—One the size of a swallow's egg, was caseous and softened throughout.

The Lumbar and Iliac Glands were enlarged and contained numerous soft yellow caseous nodules, up to 2·5 mm. in diameter (one right iliac gland was normal).

Inguinal and Axillary Glands.—Normal.

Special Organs.

Testicles.—Normal; on the tunica vaginalis were several small hard tubercles.

Brain and meninges.—Normal.

Under the periosteum of one of the frontal bones there was a slightly raised red gelatinous nodule, about 1 cm. in diameter, containing soft yellow foci; this nodule lay in a shallow carious depression in the bone.

Microscopical Examination (Smear Preparations).

Left Tonsil.—Fairly numerous T.B.

Spleen (emulsion).—A few T.B.

Lung (emulsion).—A few T.B.

Nodule under Periosteum of Frontal Bone.—Numerous T.B.

Guinea-pigs Inoculated.

No. 1899, inoculated intraperitoneally with an emulsion made from a lung nodule, died 43 days later of general tuberculosis.

No. 1900, inoculated intraperitoneally with an emulsion made from the spleen, was killed with 1·0 cc. tuberculin and found to have general tuberculosis.

RHESUS MONKEY AND BABOON PASSAGE.

VIRUS B. I.

MONKEY 76.

Fed on April 7th, 1904, with 0·01 mg. of culture obtained from a supramammary lymphatic gland of Cow 44. *see* Vol. I.

Killed when dying, November 21st, 1905. 228 days after.

Abstract of Post-mortem Notes.

Tonsils and pharynx, normal. Twelve to fifteen small caseous ulcers in small intestine. Enlargement, caseation and softening of the pharyngeal, abdominal and thoracic lymphatic glands. Liver beset with miliary tubercles and softened nodules. Spleen, adherent, greatly enlarged and extensively caseated. Each kidney contained moderately numerous tubercles. The lungs showed numerous miliary tubercles and several large consolidated areas, some far advanced in caseation; in one of the latter there was an irregular cavity. There was tuberculosis of the omentum, peritoneum, pleura and pericardium.

A culture was isolated from the spleen and used to inoculate a series of six rabbits and to feed Baboon 66.

All the rabbits died of general tuberculosis.

BABOON 66.

Fed with culture from Monkey 76.

Dose.—0·1 milligramme.

Date.—September 27th, 1905.

Died.—February 7th, 1906. 133 days after.

Clinical History.

There was no disturbance in the general health until about the middle of January. He was then noticed to be becoming quieter and to be less inclined for food; he also seemed to be losing flesh. He rather quickly became weaker and thinner and died about 17 days after he was first observed to be ill. There was no increase in the respiration, and no diarrhœa during the experiment.

Tuberculin Test.—November 24th, 1905, 58 days after the experiment began. Doubtful. The temperature rose to 39·4° C. (maximum before the inoculation of the tuberculin 38·8 C.) 5 hours after inoculation, and then fell to 38·1° C. at the 12th hour; it gradually rose again and reached 40·7° C. at the 27th hour.

Temperature.—The temperature remained practically normal (maximum 39·2° C.) until January 19th; it then gradually fell to a lower and lower level, and finally no temperature could be recorded with the clinical thermometer.

Post-Mortem.

Body.—Slightly emaciated.

Tongue and Tonsils.—Normal.

Pharynx.—The mucous membrane of the vault of the pharynx showed an oval ulcer, 1 cm. in length, with inverted edges, smooth, undermined margins and congested, uneven floor; the base was fibroid and contained yellow caseous tubercles.

Submaxillary Glands.—Normal.

Retro-Pharyngeal Glands.—On the right side there was a group of three glands, the largest the size of a swallow's egg; these were firm and practically caseous throughout.

On the left side there were four glands, the largest the size of a large pea; two of these were caseous throughout; the third was partly caseous; the fourth contained discrete caseous nodules, irregular in outline.

Cervical Glands.—On the right side in the middle of the neck there was a gland the size of a broad bean which was firm and practically caseous throughout (composed of closely aggregated tubercles); below this there was a gland the size of a French bean closely beset with firm caseous nodules; between the carotid artery and the larynx there were three small glands which were caseous throughout; two small glands under the right clavicle contained caseous tubercles.

On the left side the cervical glands were normal.

Abdomen.

The Omentum was closely studded with caseous nodules with grey margins, ranging in size from a millet seed to that of a wheat grain; they remained discrete except near the spleen, where they were aggregated together into two or three small groups.

The Parietal Peritoneum near the spleen was studded with flattened caseating tubercles up to 1 mm.; there were similar caseous tubercles on the peritoneum covering the diaphragm, and near the spleen a yellow caseous patch about 1 mm. thick; below the spleen the peritoneum was covered with small pearly grey tubercles; on the right side the tubercles were sparsely scattered.

On the mesentery and serous surface of stomach there were a few nodules similar to those in the omentum.

Stomach.—Mucous membrane normal.

Small Intestine.—About 12 inches from the pylorus there was a small circular ulcer with irregular, slightly thickened margins and thin fibroid base in which one or two yellow points were seen. In a Peyer's patch 15 inches from the ileo-coecal valve there was an oval transverse ulcer 1 cm. in greatest diameter with thick raised irregular margins and caseous base; the peritoneum over them was thickened and darkly pigmented; there were no sub-peritoneal tubercles.

Large Intestine.—Normal.

Mesenteric Glands.—In the upper part of the mesentery there was a large gland, 1 in. in greatest diameter; on section it was caseous throughout and softened in the centre. In the lower part of the mesentery there was a group of three glands, the largest $\frac{2}{3}$ in. in diameter; these were also caseous throughout, but not softened. Most of the glands in the middle of the mesentery were normal; some contained small caseous tubercles or small nodules.

The Ileo-Colic Glands.—Were normal with one exception, which contained a small caseous focus.

Colic Glands.—One contained an irregular caseous nodule about 2 mm. in diameter; the rest were normal.

The Spleen.—Was somewhat enlarged and contained about 15 spherical nodules, some of which projected from the surface, majority about 1·5 cm. in diameter; the nodules were yellow and caseous throughout and softened in the centre.

Splenic Lymphatic Glands.—In the hilum of the spleen several lymphatic glands were enlarged and contained discrete caseous tubercles embedded in firm grey tissue.

The Liver.—Was normal in general appearance and showed scattered throughout the substance moderately numerous caseous tubercles ranging in size from 0·5 to 2 mm. in diameter.

The Portal Gland.—Contained three or four caseous tubercles in the cortex.

Kidneys.—Normal.

Suprarenal Bodies.—Normal.

Thorax.

Heart.—Normal.

Pleura.—Costal pleura normal; on the tendon of the diaphragm on the left side there was a yellow granular

patch composed of closely aggregated tubercles; elsewhere the diaphragmatic pleura showed scattered caseous tubercles.

The Lungs.—Collapsed normally, and were crepitant throughout. The left lower lobe was adherent to the patch on the diaphragm, the visceral pleura at this point being opaque and thickened.

In the right upper lobe there was one opaque miliary tubercle; in the right middle lobe there were two caseating tubercles, 1 and 2 mm. in diameter respectively; in the right lower lobe there were about half-a-dozen caseous tubercles with grey margins, ranging from 1 to 2·5 mm. in diameter.

The left lobes contained each about nine miliary caseous tubercles.

Bronchial Glands.—One left prætracheo-bronchial gland contained a firm caseous nodule 5 mm. in diameter; another contained a smaller caseous nodule. A right prætracheo-bronchial gland contained a small caseous tubercle. An intertracheo-bronchial gland on the right side showed one caseous tubercle and two on the left side small caseous nodules.

The Dorsal Mediastinal Glands.—Five in number, were enlarged, firm, and caseous throughout.

The Ventral Mediastinal Glands.—Contained a few discrete tubercles.

Lumbar Glands.—Slightly enlarged, showed on section firm caseous tubercles, varying in size.

Iliac Glands.—One on the left side, the size of a pea, was caseous throughout; those on the right side were normal.

Inguinal and Axillary Glands.—Normal.

Brain and Meninges.—Normal.

Testicles.—Normal.

Microscopical Examination (Smear preparations).

Spleen	- - - -	2 tubercle bacilli seen.
Cervical Gland	- -	4 tubercle bacilli seen.

RHESUS MONKEY PASSAGE.

VIRUS B. IX.

MONKEY 110.

Fed on June 2nd, 1905, with 1·0 mg. of culture derived from the original material of Virus B. IX.

Killed, when dying, September 28th, 1905. 80 days later.

Abstract of Post-Mortem.

Tonsils and glands in neck normal. The stomach showed one small ulcer; the small intestine, 10 typical tuberculous ulcers, the largest 10 × 4 mm.; there were two submucous caseous nodules in the large intestine. The mesenteric glands were caseous throughout, and formed a large fluctuating mass. Almost all the other abdominal lymphatic glands were enlarged and caseous. The spleen contained 7 or 8 caseous nodules, the largest the size of a pea; the liver showed about two dozen caseous tubercles on the surface and there were fairly numerous caseous tubercles in each kidney. The lungs were closely beset with miliary caseous tubercles, and the bronchial glands were caseous. There were two tubercles in the right suprarenal body, two on the omentum, one in the pancreas, and one on the parietal pleura. The cortex of the brain showed about half a dozen caseous nodules.

Animals Inoculated.

A culture was isolated from an iliac gland, and when it had been in cultivation 55 days was inoculated intraperitoneally into two rabbits and subcutaneously into two rhesus monkeys, Nos. 114 and 116. Both rabbits died of general tuberculosis in 22 and 37 days respectively.

RHESUS MONKEY 114.

Subcutaneous.—Inoculation of culture from Monkey 110.

Dose.—0·01 milligramme.

Date.—November 22nd, 1905.

Died.—December 25th, 1905. 33 days after inoculation.

Clinical History.

The monkey was inoculated subcutaneously between the shoulders on November 22, 1905.

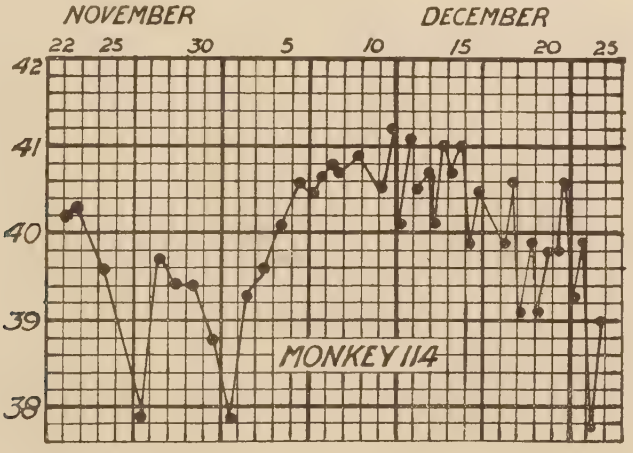
Fifteen days after inoculation there was a rounded swelling, about 1½ in. in diameter, soft and fluctuating in the centre; there was a small opening in the skin. Six days later the swelling was larger and there were three openings in the skin through which yellow pus was discharged.

On December 15 the skin showed an ulcer ½ in. in diameter covered by a scab; removal of this and pressure on the tumour caused thick brownish yellow pus to exude; there were also two small openings covered by scabs. The ulcers enlarged and coalesced and the tumour became much collapsed and appeared to have increased in area.

The axillary glands on both sides became moderately enlarged.

At the end of the third week the monkey was noticed to be unwell; he was thinner and appeared to be losing appetite. The general condition gradually became poorer, and although the appetite was moderate he was obviously losing flesh. On December 23rd he became much worse, and was found dead on the 25th.

Temperature



Post-mortem.

Body.—In poor condition.

Local Lesion.—At the site of inoculation in the middle line of the back, just below the inferior angle of the scapula, there was a slightly irregular oval ulcer, 1 in. × ¾ in.; the margins were thin, reddened, undermined, and partly caseous. The ulcer led into a cavity 2 in. in diameter filled with soft caseous substance; the walls were formed by the muscles of the back, which were infiltrated by caseation.

The Right Axillary Gland.—½ in. long, had the cortex caseous and contained discrete tubercles in the medulla. The gland on the left side was similar.

Thorax.

Parietal Pleura.—Normal.

The right pleural cavity contained an excess of fluid.

Lungs.—The right lung was dark and congested and contained a moderate number of discrete tubercles up to 2·5 mm. in diameter. The largest were caseous with translucent grey margins. On the dorsal surface of the lung there was a fibrinous deposit.

The left lung was pink in colour and contained fewer tubercles up to 2 mm.: they were grey and translucent with opaque centres.

Bronchial Glands.—The right bronchial gland was enlarged and contained soft yellow caseous tubercles up to

1·5 mm.; they were numerous and were arranged round the cortex. The left bronchial gland was small and contained three caseous tubercles. Each prætracheo-bronchial gland contained a single miliary tubercle.

Heart.—In the muscle of the right ventricle there was one pinhead-sized tubercle, and one twice the size in the left. Cavities of the heart normal.

In the dorsal mediastinum on each side of the spine there was a chain of soft caseous lymphatic glands the largest the size of a kidney bean.

Abdomen.

Peritoneum and Omentum.—Normal.

The Spleen was greatly enlarged, measuring 3 in. $1\frac{3}{4}$ in. × $\frac{3}{4}$ in.; it contained numerous tubercles, varying in size from minute grey points up to 3 mm. in diameter. The largest were irregular in outline, opaque and caseous throughout. In the splenic lymphatic gland there were a few discrete tubercles.

The Liver was large and was closely beset with tubercles the smallest minute and grey, the largest up to a pinhead in size, yellowish and caseous.

The Portal Gland was enlarged and the cortex was filled with yellowish tubercles larger than a pinhead.

Kidneys.—In each kidney there were about six scattered grey tubercles with minute opaque centres: on section there were scattered tubercles in the depth. In the right kidney in addition there was a pin head sized yellow tubercle.

Suprarenal Bodies.—In the right suprarenal there was one opaque miliary tubercle.

Lymphatic Glands.

The Mesenteric Glands were not enlarged and in each of these there was a minute opaque tubercle.

In the *Submaxillary, Inguinal, and Left Iliac Glands* there were from one to three pinhead sized caseous tubercles.

Lumbar Glands.—Normal.

Microscopic Examination (Smear Preparation).

Inguinal Gland.—Numerous tubercle bacilli.

Animals Inoculated.

A culture was recovered from an axillary gland and, when it had been in cultivation 27 days, was inoculated subcutaneously into three rabbits and used to feed two monkeys, Nos. 130 and 132.

All the rabbits died of general tuberculosis.

RHESUS MONKEY 116.

Subcutaneous.—Inoculation of culture from Monkey 110.

Dose.—0·01 milligramme.

Date.—November 22nd, 1905.

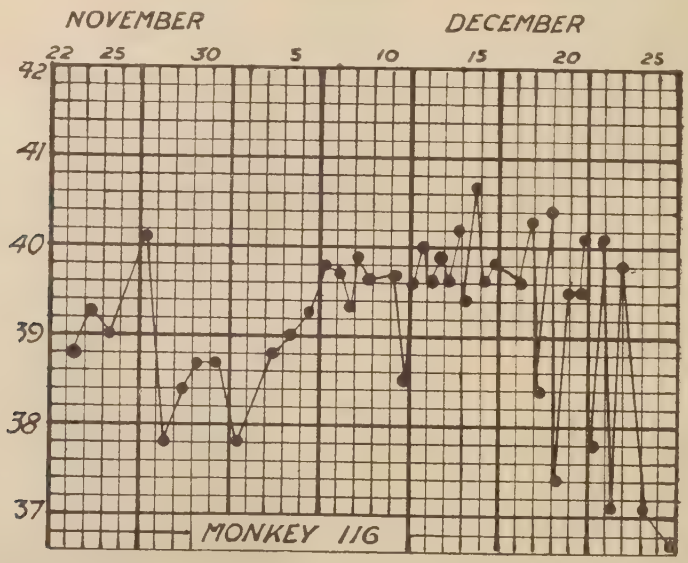
Died.—December 28th, 1905. 36 days after inoculation.

Clinical History.

The monkey was inoculated subcutaneously between the shoulders.

A fortnight after inoculation there was a slightly raised soft swelling measuring nearly 1in. in diameter; on the 23rd day the swelling measured $1\frac{3}{4}$ in. by $1\frac{1}{4}$ in. and subsequently there was a little further increase in size. A fortnight after inoculation the axillary glands were felt to be slightly enlarged; during the rest of the experiment they underwent little further enlargement. Up to December 22 there was no change in the general condition, except that latterly the animal was quieter; then illness (rough coat, depression and anorexia,) rapidly supervened and the animal soon became collapsed and died.

Temperature.



Post-mortem.

Body emaciated.

Local Lesion.—At the site of inoculation, in the mid line of the back, there was a soft fluctuating tumour ($2\frac{1}{2}$ in. by $1\frac{1}{4}$ in.) situated between the skin and muscle. On section it showed a fibro-caseous wall filled with yellow caseo-pus.

The Axillary Glands on both sides were enlarged and contained nodules filled with soft caseo-pus.

Cervical Glands.—The right inferior cervical gland was enlarged and contained pinhead yellow tubercles: the left was small and appeared normal.

Abdomen.

The Spleen, little if at all enlarged ($1\frac{1}{2}$ in. by $\frac{3}{4}$ in. by $\frac{1}{2}$ in) contained numerous tubercles varying in size, the smallest minute and grey, the largest up to 3 mm. in diameter, with soft yellow caseous centres. Those under the capsule projected slightly.

Liver.—The liver contained numerous minute grey foci just visible and a moderate number of large tubercles varying in diameter up to 3 mm. The largest of these had thin fibrous capsules and were completely softened in the centre.

The Portal Gland was a little enlarged and contained a soft yellow nodule.

The Kidneys and *Suprarenal Capsules* appeared normal.

Thorax.

Heart.—Normal.

Pleura.—The right pleural cavity contained a little fluid, and the right lung was more collapsed than the left. On the left side under the pleura at the junction of the eighth rib with the spine was a pea-sized softened yellow nodule.

Lungs.—In the lungs were sparsely scattered grey translucent tubercles up to 2 mm. in diameter, the largest of which showed minute soft yellow centres.

Bronchial Glands.—The bronchial glands were normal.

Various Lymphatic Glands.

Lumbar and Iliac Glands.—Normal.

Sternal glands were a little enlarged and contained pinhead sized caseous tubercles.

There was a pinhead sized caseous tubercle in each sub-maxillary gland, and in each of two mesenteric glands and in a left inguinal gland. Right inguinal glands normal.

Microscopical Examination.

Axillary gland showed a moderate number of T.B.

RHESUS MONKEY 130.

Fed with culture obtained from an axillary gland of Monkey 116.

Dose—0·01 milligramme.

Date—January 24th, 1906.

Killed, when dying, April 20th, 1906. 86 days after being fed.

Clinical History.

Signs of ill-health, rough coat and depression were first noticed on March 31st, 1906.

On April 16th the monkey was obviously ill and appeared to be losing flesh; there was occasional cough but no increase in respiration.

The animal rapidly became worse and lost appetite; it was killed in a dying condition on April 20th, 1906. 86 days after the experiment began.

Tuberculin Test.—April 10th, 1906. 76 days after the experiment began. Positive. Rise of temperature 1·1° C.

Temperature.

The temperature was slightly raised from February 21st to February 26th, maximum 39·7° C.; and again from March 29th until the end of the experiment, maximum 40°C.

Post-mortem Examination.

Body.—Condition, moderate.

Tongue, Tonsils and Pharynx.—Normal.

Submaxillary, Retro-Pharyngeal and Cervical Glands.—Normal.

Abdomen.

Omentum and Peritoneum.—Normal.

Stomach.—Normal.

Small Intestine.—Normal.

Large Intestine.—Normal.

Mesenteric and Colic Glands.—Normal.

One Ileo-colic Gland contained a grey miliary tubercle, with a caseous centre.

A Cœliac Gland contained a similar tubercle.

A Gland on the head of the pancreas contained two miliary caseating tubercles.

The Spleen was enlarged and showed on the surface numerous projecting yellow nodules, the largest about 8 mm. in diameter; the nodules were thin-walled and filled with caseo-pus; on section similar nodules were seen in the interior of the spleen.

Splenic Lymphatic Glands.—Normal.

The Liver was normal in general appearance; in the substance just under the capsule, there were three or four tubercles, the largest about 1 mm. in diameter with caseous centres and grey margins. On section two or three small tubercles were seen in the depth.

The Portal Gland appeared normal.

The Left Kidney showed in the cortex a grey tubercle with a slightly opaque centre. In the right kidney there was a grey miliary tubercle with no central opacity.

Suprarenal Bodies.—Normal.

Thorax.

Heart.—The pericardial sac contained a small quantity of clear serous fluid; heart muscle normal.

Lungs.—The right middle lobe and an accessory right middle lobe were much enlarged and filled the lower part of the right pleural cavity; the former was firmly adherent to the chest wall in front and laterally and less firmly to the upper and lower lobes; on section the middle lobe was composed partly of grey translucent tissue, closely beset with irregular caseous patches and tubercles, and partly of firm yellow caseated tissue, showing here and there small islands of grey translucent tissue; the accessory middle lobe was almost uniformly yellow and caseous; there was no sign of softening.

The right lower lobe was crepitant in the dorsal part and consolidated near the root; the solid portions were composed of firm translucent grey tissue in a less advanced stage of caseation than the middle lobes; the crepitant part contained a few discrete caseating nodules.

The right upper lobe contained about eight nodules, similar to those in the left lobes, and a small patch composed of grey caseating tubercles closely aggregated together.

The left lobes were dark red, collapsed and quite airless; they contained altogether about two dozen nodules, ranging in size from a pin's head to that of a pea; on section the nodules were firm yellow and caseous in the centre, grey and translucent around the margins.

Bronchial Glands.—On the left side two prætracheo-bronchial glands were affected; one contained a caseous nodule, 2 mm. in diameter; another a small caseous tubercle; one intertracheo-bronchial gland on this side contained a miliary caseous tubercle.

On the right side the glands were moderately enlarged; two prætracheo-bronchial glands were beset with caseous tubercles, here and there becoming confluent; one intertracheo-bronchial gland, the size of a broad bean, was composed of irregular yellow caseous areas, some softened, separated by a small amount of translucent grey tissue.

Ventral Mediastinal Glands.—One on the right side, the size of a French bean, was beset with discrete caseous tubercles, irregular in outline, ranging from a mere point to 1·5 mm. in diameter; another contained a miliary caseous tubercle; the rest were normal.

Vertebral Glands.—The glands in the intercostal spaces near the vertebral column were on the right side all enlarged; one in the third space showed three caseating tubercles and one in the ninth one minute tubercle; in the rest there were no opaque foci. On the left side the glands were normal.

Parietal Pleura.—The pleura, where it was adherent to the right middle lobe, was roughened and shaggy and showed two loosely attached caseous nodules, the largest 3 mm. in diameter; there were two similar nodules in the upper part of the chest and about half-a-dozen miliary caseating tubercles.

Lumbar and Iliac Glands.—Normal.

Inguinal and Axillary Glands.—Normal.

Brain and Meninges.—Normal.

RHESUS MONKEY 132.

Fed with culture obtained from an axillary gland of Monkey 116.

Date—January 24th, 1906.

Dose—0·0001 milligramme.

Died—June 7th, 1906. 134 days after the experiment began.

Clinical History.

The monkey showed no obvious signs of ill-health until about the middle of May; the coat was then noticed to be rough and he appeared to be losing flesh; he gradually became perceptibly thinner and lost appetite and was found dead on June 7, 1906, 134 days after the experiment began; the respirations were not noticeably increased and there was no diarrhœa.

Tuberculin Test.—10th April, 1906, 76 days after the experiment began. Positive. Rise of temperature, 1·2°C. Dose of tuberculin 2·0 cubic centimetres.

Temperature.—The temperature from the beginning of the experiment until 19th April, was normal, with exception of a slight rise, to 39·8° C. on 3rd April, 1906. From April 16th to May 10th there was a period of slight pyrexia (maximum, 39·0° C.); then after a slight fall the temperature rose, and from May 15th to May 25th was sustained at a fairly high level (maximum 40·8° C.); subsequently it assumed a lower level, falling finally below 37° C.

Post-mortem Examination.

Tongue, Tonsils and Pharynx.—Normal.

Submental, Submaxillary, Retropharyngeal and Cervical Glands.—Normal.

Abdomen.

The Omentum contained a few small transparent bodies which were probably early tubercles.

Stomach and Intestines.—Normal.

Gastric Glands.—A gland in the lesser curvature of the stomach near the pylorus contained a pin-head sized caseous tubercle.

Mesenteric Glands.—One mesenteric gland, the size of a large pea, was filled with yellow tenacious caseo-pus; other mesenteric glands were normal.

Ileo-colic and Colic Glands.—Normal.

Spleen.—The spleen showed on the surface half-a-dozen projecting nodules, the largest the size of a large pea; three of these were very prominent, projecting more than half their diameter from the surface; the others were completely surrounded by spleen pulp, and caused only slight projections; in the depth of the spleen three more nodules, the largest the size of a pea, were seen; all the nodules were caseous and softened throughout.

The gastro-splenic omentum was adherent to two of the projecting nodules, and showed between them about half-a-dozen millet-seed-sized caseous tubercles.

The lymphatic glands in the hilum of the spleen were perhaps slightly enlarged, but were not otherwise abnormal.

Liver.—The liver was normal in general appearance; on the surface under the capsule there were three caseous nodules, ranging from 1 to 2·5 mm. in diameter, the largest of which was softened; in the depth of the liver substance there was a nodule, 2 mm. in diameter, with a grey margin and bile-stained caseous centre.

In the Small Omentum, between the head of the pancreas and the liver, there were three enlarged glands; one of these was caseous almost throughout; the others contained soft yellow caseous nodules up to 3 mm. in diameter.

Kidneys and Suprarenal Bodies.—Normal.

Thorax.

Lungs.—The left lower lobe was enlarged, and filled the greater part of the left half of the thorax; it was practically everywhere adherent to the chest wall by adhesions which were recent and easily torn through, except at the apex, where the two layers of pleura were united more firmly; the right lung was free from adhesions.

On removal of the lungs the left lower lobe was firm, heavy and quite airless; on the dorsal surface of the apex there was a patch of thickening; externally the ventral thinner parts of the lobe had a greyish red translucent appearance, whilst the rest of the lobe showed a yellowish mottling; on section the whole of the lobe was in a condition of caseous pneumonia; the dorsal portions were almost uniformly caseous and softened, and in the midst of some of the caseous areas small cavities had formed; in the thin ventral portions a small amount of translucent greyish red tissue separated the caseous areas.

The upper lobes were adherent to the lower lobe, and were firm, red and airless except for a narrow margin along the dorsal border; these lobes contained a few caseous tubercles.

The upper and lower lobes were crepitant and congested on the dorsal surfaces; they contained sparsely scattered shotty caseous tubercles, with grey margins, ranging from about 1 to 2 mm. in diameter.

One of the small middle lobes was enlarged, firm and airless except at the extreme tip; on section it was advanced in caseation and beginning to soften. Another small middle lobe showed in the ventral part a caseous mass, 1·5 cm. in greatest diameter, with translucent margins; extending from this mass to the root of the lung

on the internal surface there was a reddish consolidated patch, which showed on section grey foci (? early caseation).

Pleura.—There were no tubercles on the pleura.

Heart and Pericardium.—Normal.

Bronchial Glands.—The glands on the right side were enlarged, congested and œdematous; one below the bronchus contained a minute caseous point; in the others there was no sign of caseation.

On the left side a prætracheo-bronchial gland, the size of a thrush's egg, was caseous and softened throughout; another slightly smaller gland in the root of the lung on the dorsal surface of the bronchus was also caseous throughout; an intertracheo-bronchial gland on this side was large, firm and congested, and contained one pin-head-sized caseous tubercle. Some glands along the trachea and a few ventral to the arch of the aorta were enlarged, congested, but were not otherwise abnormal.

Dorsal Mediastinal Glands.—Normal.

Axillary Glands.—One on the left side contained a softened yellow caseous nodule, 2 mm. in diameter; the others were normal.

Inguinal, Lumbar and Iliac Glands.—Normal.

Brain and Meninges.—Normal.

Testicles.—Normal.

Microscopical Examination (Smear Preparation).

Bronchial Gland.—Numerous tubercle bacilli.

Spleen.—Numerous tubercle bacilli.

Liver.—Numerous tubercle bacilli.

Axillary Gland.—Numerous tubercle bacilli.

RHESUS MONKEY 152.

Subcutaneous—Inoculation of an emulsion made with the spleen of Rhesus Monkey 132.

Date—June 7th, 1906.

Died—September 26th, 1906.

Duration of Experiment—111 days.

Clinical History.

The animal was inoculated in the subcutaneous tissue of the back on June 7th, 1906. After three weeks there was a small thickening in the subcutaneous tissue at the seat of inoculation and an opening in the skin over it from which issued a blood-stained purulent discharge; the axillary glands were not perceptibly enlarged. The tumour increased in size and the skin over it became ulcerated in several places. The glands in the axillae enlarged, the skin over them became adherent and finally ulcerated.

Until three weeks before its death the monkey was lively and ate well; it showed no sign of ill-health except that it increased very little in size. After the first manifestation of illness its condition became progressively and rapidly worse.

Post-mortem Examination.

Body.—In poor condition.

Local Lesion.—In the subcutaneous tissues over the lower part of the right scapula there was a small softened caseous tumour, the skin over which was pierced by a number of small openings through which caseo-pus could be made to exude; the hair was covered with masses of dried secretion. In the tissues around the tumour there was a number of discrete yellow softened nodules up to a hempseed in size.

Axillary Glands.—In the right axilla there was a group of three enlarged glands, the largest the size of a pigeon's egg, which on section were composed of creamy yellow caseo-pus and a fibrous wall varying in thickness; the skin over two of the glands was adherent and ulcerated, the floors of the ulcers being formed by the dried caseous contents of the gland.

In the left axilla there were two enlarged glands, one the size of a large pea, the other a little smaller, both of which were caseous and softened practically throughout.

Cervical Glands.—Normal.

Thorax.

Lungs.—The lungs were crepitant and collapsed normally, they contained moderately numerous nodules varying greatly in size, ranging from about the size of a pin's head up to about 1 cm. in diameter; most of the larger nodules had caseous and softened centres and relatively thick fibrous margins, but a few were composed of a firm greyish tissue containing purulent foci (resembling muco-pus); the smallest nodules were firm, grey, and had opaque yellowish centres.

Bronchial Glands.—Four (two on each side) were much enlarged and varied in size from 1 to 2 cm. in diameter; on section they were thin-walled cysts filled with soft yellow tenacious caseo-pus; the other bronchial glands were enlarged and one contained a miliary caseous tubercle.

Mediastinal Glands.—One of the ventral mediastinal glands on the right side, the size of a large pea, was caseous and softened throughout.

Vertebral Glands. On the right side of the vertebral column opposite the heads of the eighth to tenth ribs there was a prominent thin-walled tumour, the size of a thrush's egg, filled with thick caseo-pus; the right caudal lobe was closely adherent to the capsule of the tumour; other vertebral glands were normal.

Pleura.—Costal pleura, normal. The tendon of the diaphragm showed about half a dozen flattened nodules with fibrous margins and soft caseous centres, ranging in size from a millet seed to a hempseed.

Heart.—Normal.

Abdomen.

Omentum.—The omentum contained six nodules ranging in size from a pin's head to a hempseed; the larger ones were caseous and softened in the centre; the smaller ones were translucent.

Spleen.—The spleen contained two pea-sized yellow caseous and softened nodules with thin fibrous capsules and about a dozen smaller caseous nodules ranging in size from that of a pin's head to that of a hempseed. In the hilum of the spleen there were three enlarged glands, all of which were caseous and softened throughout.

Liver.—Sparsely scattered over the surface of the liver were grey translucent foci, the largest 1 mm. in diameter; these extended nearly their own diameter into the substance and did not project from the surface. In the substance there was a number of thin walled cysts, containing a greenish mucoid material, ranging in size from that of a pea downwards; there were besides a few caseous tubercles with fibrous margins.

Closely adherent to the head of the pancreas there was a mass of fused glands (probably including the portal) the size of a pigeon's egg; all the glands were entirely converted into thick yellow caseo-pus, which in each case was surrounded by a thin fibrous capsule.

Kidneys.—The left kidney showed in the depth of the cortex a minute grey tubercle. The right kidney showed a similar tubercle on the surface and three in the depth.

Suprarenal Bodies.—The right suprarenal body contained a firm yellow caseous nodule, 5 mm. in diameter. The left suprarenal body was normal.

Small Intestine.—The wall of the small intestine showed about eighteen thickened caseating patches, the largest of which extended transversely around nearly the whole circumference of the gut; the mucous membrane over them was ulcerated, the larger areas showing several separate ulcers. On the serous surface of the larger areas and in the mesentery there was a well-marked tuberculous lymphangitis.

Large Intestine.—The mucous membrane of the large intestine showed eight ulcers, with congested margins, the largest 1 cm. in greatest diameter; the bases of these ulcers were thickened (in some cases considerably) and fibroid and beset with soft caseous foci.

Mesenteric Glands.—The mesenteric glands were greatly enlarged and formed two large fluctuating tumours, one in the upper part, nodular in outline, about the size of a pigeon's egg, and one in the lower part about the size of a hen's egg; the two tumours were united together at the middle of the mesentery; the duodenum lay in close contact with the upper mass, but could be easily separated from it. On section all the glands composing the tumours were converted into thin-walled cysts filled with thick creamy caseo-pus; when the latter was washed away under a stream of water nothing was left but a multilocular cyst wall.

Ileo-colic Glands.—The glands on each side of the junction of the ileum and cœcum were enlarged and resembled on section the mesenteric glands.

Colic Glands.—Several of the colic glands (eight to ten in number) were enlarged, up to 1 cm. in diameter, caseous and softened throughout.

Ilio-sacral Glands.—Two ilio-sacral glands, the size of large peas, were yellow, caseous and softened throughout.

Lumbar Glands.—A left lumbar gland contained a millet-seed sized, soft, caseous tubercle.

Iliac Glands.—A left iliac gland, the size of a pea, was caseous and softened thoughout; the right was normal.

Inguinal Glands.—One on the left side resembled the iliac; the rest were normal.

Tongue, Tonsils, Pharynx, Submaxillary and Pharyngeal Lymphatic Glands.—Normal.

Microscopical Examination.

Lung Nodule.—Numerous tubercle bacilli.

Splenic Lymphatic Gland.—Very numerous tubercle bacilli.

MONKEY AND BABOON PASSAGE.

VIRUS B. IX.

MANGABEY MONKEY 4.

Fed on June 2nd, 1905, with 1·0 mg. of culture derived from the original material of Virus B. IX.

Killed.—August 25th, 1905. 84 days later.

Abstract of Post-mortem.

Tonsils and neck glands normal. The small intestine showed five tuberculous ulcers. The mesenteric glands were much enlarged, caseous throughout and softened. The lungs and spleen contained numerous caseous nodules, the largest the size of a pea. The liver was closely beset with caseous tubercles and there were several caseous tubercles in each kindey. One bronchial gland showed caseous areas; the rest were normal. The dorsal mediastinal and lumbar glands were extensively caseated, and there were areas of caseation in the portal, splenic and pancreatic glands. There were some tubercles on the omentum and pleural surface of the diaphragm.

A culture was isolated from a lumbar gland and used to feed Mandrill Baboon 68.

MANDRILL BABOON 68.

Fed with culture from Mangabey Monkey 4.

Dose.—0·1 milligramme.

Date.—October 13th, 1905.

Died.—November 28th, 1905. 46 days later.

Clinical History.

"Illness was first noticed on November 13th, 1905; he was much quieter and seemed to be losing appetite. These symptoms continued and he became thinner and lost strength. On November 20th he had a fit in the morning, suddenly losing power over his limbs; there was no loss of consciousness and only occasional slight twitchings of the legs; after a little while the symptoms abated and he was able to walk about and climb on to his perch. The fits recurred on every subsequent morning until death, and occasionally in the afternoon as well.

Post-mortem Examination.

Body in poor condition.

Tongue and Pharynx.—Normal.

Tonsils.—In one of the follicles of the right there was a little purulent material. (No tubercle bacilli found; numerous organisms stained blue.)

Left Tonsil.—Normal.

Right Retro-Pharyngeal Gland, about the size of a sparrow's egg, contained viscid tenacious yellow purulent material almost completely replacing the gland substance. (Tubercle bacilli found.)

Left Retro-Pharyngeal Gland.—Normal.

Submaxillary and Cervical Glands.—Normal.

Abdomen.

Omentum and Peritoneum.—Normal.

Stomach and Small Intestines.—Normal.

Large Intestines; mucous membrane congested; no ulceration.

Gastric, Mesenteric and Colic Glands.—Normal.

Spleen, Liver, Kidneys and Suprarenal Bodies.—Normal.

Thorax.

Heart and Pleura.—Normal.

Lungs and Bronchial Glands.—Normal.

The Muscles all over the body showed moderately numerous cysticerci (? taenia saginata); the tongue, the heart, and the wall of the stomach each contained one, and the diaphragm two.

The Brain contained numerous similar cysts; some were completely embedded in the cortical substance; others projected varying distances from the surface; the latter by their pressure had caused shallow cup-shaped hollows on the internal surface of the vault of the skull.

Microscopical Examination. (*Smear Preparations.*)

Retro-Pharyngeal Gland. Tubercle bacilli scanty.

Animals Inoculated.

Guinea-pig 1785, inoculated with an emulsion made from the spleen, died in 36 days of general tuberculosis.

A culture was isolated from a pharyngeal gland and, after cultivation on artificial media for 57 days, was used to inoculate three rabbits subcutaneously and to feed Monkeys 126 and 128.

The rabbits all died of general tuberculosis.

MONKEY 126.

Fed with culture obtained from the right retro-pharyngeal gland of Baboon 68.

Date—January 24th, 1906.

Dose—0·01 milligramme.

Died—May 24th, 1906. 120 days after the experiment began.

Clinical History.

The monkey remained well until about the end of April; he was then noticed to be quieter, and to have a "staring" coat.

He gradually became more depressed, and, although the appetite remained moderately good, slowly lost flesh and strength; there was no increase in the rate of respiration, and no diarrhœa.

Tuberculin Test.—April 10th, 1906, 76 days after the experiment began. Positive. Rise of temperature 1·9°C. Maximum 41·1°C. 3 hours after injection. Dose of tuberculin 2·0 cubic centimetres.

Temperature.—During the first twelve weeks the temperature was normal, varying from 39·3°C. to 38°C.; on April 17th it began to rise and for twelve days there was a period of moderately high pyrexia (maximum 41°C.); the temperature then became irregular and began gradually to fall; the daily fluctuations were often more than 2°C.; during the last few days the temperature was subnormal and on two occasions fell to 36°C.

Post-mortem Examination.

Body.—Slightly emaciated.

Tongue, Tonsils and Pharynx.—Normal.

Submaxillary, Pharnygeal and Cervical Glands.—Normal.

Abdomen.

The Peritoneal Cavity.—Contained about 15 cubic centimetres of clear fluid.

The Omentum.—Contained four yellow nodules, irregular in outline, each the size of hemp-seeds, which on section were entirely caseous and beginning to soften; there were, besides, four caseous tubercles, the size of millet seeds.

The Parietal Peritoneum was normal.

Stomach.—Normal.

Small Intestine.—One foot from the pylorus the wall of the small intestine showed a ring-like thickening, 13 mm. in width, which completely girdled the bowel and slightly constricted the lumen; the mucous surface was congested in patches and showed several irregular ulcers; on section all the coats of the intestine were found to be destroyed by caseation; the mesentery corresponding to the thickened intestine was almost completely obliterated, the intestine lying in close contact with the enlarged mesenteric glands; the lacteal vessels on the thickened intestine near the mesenteric attachment were opaque and yellow.

About a foot further along the intestine there was a raised area, 7 mm. in diameter, ulcerated in the centre; the submucous tissue beneath this area was caseous. Near the latter there was a small submucous caseous nodule, which was not ulcerated. Elsewhere the intestine was normal.

Large Intestine.—Normal.

Gastric Glands.—A gland near the cardiac end of the stomach was normal; one on the lesser curvature near the pylorus contained two soft caseous nodules, the largest 3 mm. in diameter.

Mesenteric Glands.—The mesenteric glands were much enlarged and formed a mass measuring $2\frac{1}{2} \times 2 \times 1\frac{1}{4}$ in.; this mass was composed of two thin walled, closely united, fluctuating swellings, one on each side of the mesentery; these swellings were smooth and shiny externally, showing a yellow mottling under the peritoneum, and were filled with soft creamy caseo-pus.

At the upper extremity of this mass, connected with the duodenum, were two glands, each 1 cm. in diameter, which were caseous and softened throughout.

The Ileo-Colic Glands.—Were œdematous, but were not otherwise abnormal.

Colic Glands.—Normal.

Thorax.

Lungs.—The lungs were expanded and filled the chest; they were somewhat emphysematous around the margins. The left lung was everywhere adherent to the chest wall; the adhesions were comparatively recent, except at one place posteriorly, where there was a tough adhesion not easily torn through; the right lung was free from adhesions.

The lobes of the left lung were adherent by recent adhesions. The right lobes were more firmly adherent together, and showed on the ventral and lateral surfaces a thin organised layer of lymph with a pitted or honeycombed surface.

The lung substance was closely beset with firm yellow nodules, ranging in size from rather less than 1 mm. up to 15 by 10 mm. in diameter; the great majority of the nodules, however, measured from 2 to 3 mm. in diameter; on section the nodules were homogeneously caseous throughout; several, particularly the larger ones, were surrounded by reddish patches of consolidation, otherwise the lung tissue between the nodules was crepitant.

Bronchial Glands.—The intertracheo-bronchial glands on each side were much enlarged, their substance being extensively replaced by softening caseous nodules, up to 4 mm. in diameter.

A Left Prætracheo-bronchial Gland, the size of a kidney bean, was partly caseous.

One on the right side contained one soft caseous nodule. A ventral mediastinal gland contained one pinhead-sized caseous tubercle.

Costal Pleura.—Normal.

On the Diaphragmatic Pleura there was some hypertrophy of the fringes around the margins of the tendon.

The Pericardial Sac.—Contained a small quantity of clear serous fluid.

Heart Muscle and Valves.—Normal.

Abdomen.

Spleen.—Not apparently enlarged, showed on the surface a few small flattened tubercles; under the capsule about a dozen nodules were seen ranging from 1·5 to 8 mm. in diameter; the larger ones projected considerably from the surface and were yellow, caseous, and softened throughout (one of these was adherent to the peritoneum covering the kidney); the smaller ones were also caseous but firmer, and the capsule over some of them showed filmy grey expansions. On section there were two or three caseous nodules in the depth of the spleen.

One Splenic Lymphatic Gland.—Contained two soft caseous nodules, the largest 2 mm. in diameter.

Liver.—Normal in general appearance, contained scattered yellow caseous and softened nodules with thin fibrous capsules, ranging from 2 to 5 mm. in diameter; twelve were counted on the surface, and there were a few in the depth, one larger (8 mm.) than the rest and very softened. At the bottom of the fissure between the right and left lobes there was a projecting yellow caseous nodule, 1 c.m in diameter.

The Portal Gland.—Appeared normal.

The Left Kidney.—Showed in the cortex five yellow caseous nodules, 2 to 3 mm. in diameter; four of these projected slightly from the surface. There were, besides, 3 or 4 caseous tubercles the largest the size of a pin's head.

The Right Kidney.—Contained two caseous nodules, the largest 3 mm. in diameter, and one small grey tubercle with an opaque centre.

The Right Suprarenal Body.—Showed in the cortex a softened caseous nodule 2.5 mm. in diameter.

Left Suprarenal Body.—Normal.

Lumbar Glands.—The lumbar glands on the left side, three in number, were much enlarged, caseous, and softened throughout; two were fused together and formed a mass the size of a filbert. On the right side two glands were slightly enlarged and practically caseous throughout.

Iliac Glands.—Normal.

Axillary and Inguinal Glands.—Normal.

Testicles.—Normal.

Brain.—Normal.

On the under surface of the dura mater, covering one of the cerebral hemispheres, there was a soft caseous tubercle.

Microscopical Examination (Smear Preparation).

Lung.—Numerous tubercle bacilli.

Spleen.—Numerous tubercle bacilli.

Dura Mater.—Numerous tubercle bacilli.

RHESUS MONKEY 128.

Fed with culture obtained from the right retro-pharyngeal gland of Baboon 68.

Date—January 24th, 1906.

Dose—0·0001 milligramme.

Killed—July 12th, 1906. 169 days after.

Clinial History.

The monkey remained well during the experiment. Weight at death, 4530 grammes.

Tuberculin Test.—April 10th, 1906, negative.

Post-mortem Examination.

There was no sign of tuberculosis in any of the organs or glands.

The Lungs showed under the pleura a few thin walled cysts containing the *Pneumonyssus griffithi.*

LEMUR PASSAGE.

VIRUS B. IX.

LEMUR 2.

Fed on June 2nd, 1905, with 1·0 mg. of culture derived from the original material of Virus B. IX.

Killed.—July 18th, 1905. 46 days later.

Abstract of Post-mortem.

There were sixteen ulcers in the small intestine and several ulcers in the cœcum; the glands in the neck, the mesenteric and colic glands were enlarged and caseous; the lungs contained four, the spleen twenty-four to thirty, caseous nodules; the liver showed scattered greyish white tubercles; the kidneys a few doubtful foci; the portal glands and a bronchial gland were tuberculous.

A culture was recovered from the mesenteric gland and inoculated into three rabbits and two lemurs (Nos. 10 and 12).

All the rabbits died of general tuberculosis of the usual type.

LEMUR 12.

Subcutaneous.—Inoculation of culture from Lemur 2.

Dose.—1·0 milligramme.

Date.—November 24th, 1905.

Killed, moribund. December 17th, 1905. 23 days after inoculation.

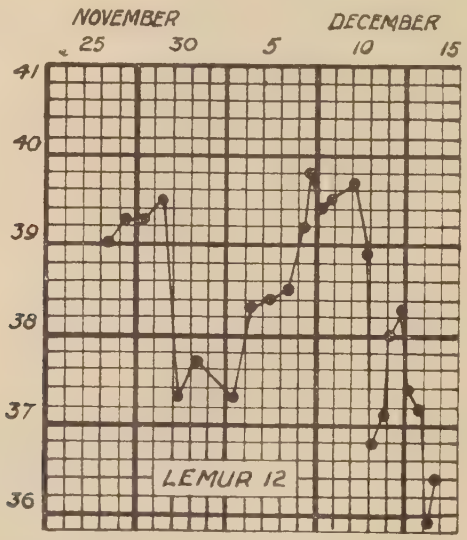
Clinical History.

The animal was inoculated subcutaneously between the shoulders on November 24th, 1905.

A fortnight after inoculation there was a hemispherical fluctuating swelling about 1¼ inches in diameter; a week later this had increased in size and the axillary glands on the left side were felt to be enlarged; on the right side enlarged glands could not be detected.

There was no obvious change in the general condition of the animal until December 16th, 22 days after inoculation, when it was found in the morning unconscious and collapsed; it remained in this state until the following morning and appeared to be dying; it was thereafter chloroformed.

Temperature.



Post-mortem Examination.

Body in fair condition; moderate amount of subcutaneous and abdominal fat.

Local Lesion.—At the seat of inoculation between the shoulders (more on the left than on the right side) there was a hemispherical cystic swelling about 1½ in. in diameter and 1 in. in depth; on section the tumour was a thin-walled cyst filled with chocolate-coloured pus; the internal surface of the cyst was yellow.

Axillary Glands.—In the left axilla there was a smooth fluctuating mass 1½ in. by ¾ in. by ½ in. composed of two or three glands fused together; on section these were converted into cysts filled with light brownish pus.

On the right side there was a gland the size of a sparrow's egg which contained a small cavity filled with pus.

Cervical Glands.—Left inferior cervical enlarged, no foci of necrosis seen. Other cervical glands normal.

Omentum and Peritoneum.—Normal.

Spleen.—(?) Enlarged, showed on the surface a grey speckled appearance; on section Malphigian bodies conspicuous, no tubercles visible.

Liver.—Pale; no tubercles seen.

Portal Gland.—Normal.

Kidneys and Suprarenal Bodies.—Normal.

Heart and Pleura.—Normal.

Lungs and Bronchial Glands.—Normal.

Tonsils, Pharynx, Intestines, and Mesenteric Glands.—Normal.

Microscopical Examination (Smear Preparation).

Spleen.—Moderately numerous tubercle bacilli.

A guinea-pig inoculated from the spleen died too early for the development of tuberculous lesions.

LEMUR 10.

Subcutaneous.—Inoculation of culture from Lemur 2.

Dose.—0·01 milligramme.

Date.—November 24th, 1905.

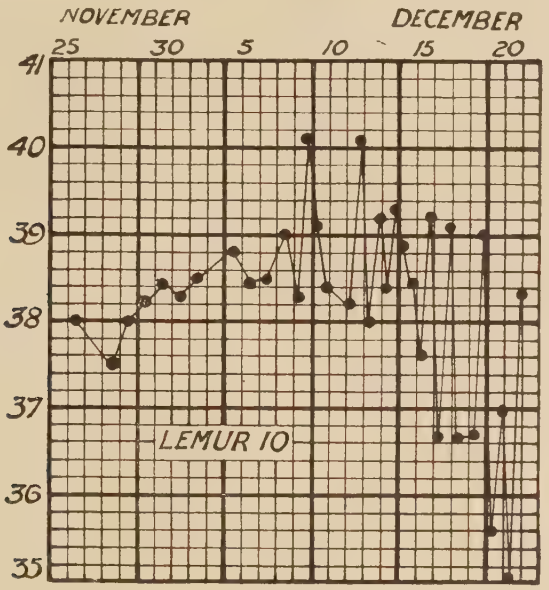
Died —December 24th, 1905. 30 days after inoculation.

Clinical History.

The animal was inoculated subcutaneously between the shoulders on November 24th, 1905. A fortnight after inoculation there was a prominent fluctuating swelling about ¾ in. in diameter; this increased in size, and on December 15th, 3 weeks after inoculation measured 1¼ in. in diameter; the skin on this date showed a small opening which discharged thin yellow pus. Subsequently the tumour increased a little in size; and the small opening became covered with dried secretion and only occasionally discharged pus. During the first three weeks there was no obvious enlargement of the axillary glands; at the end of the first month a gland in the right axilla was felt to be enlarged.

The general condition was not changed until towards the end of the experiment, when the animal suddenly became collapsed. During the last three days of life no temperature was recorded with the clinical thermometer.

Temperature.



Post-mortem Examination.

Body.—In fair condition. There was a large amount of fat in the omentum, covering completely the intestines, and also in the lumbar regions

Local Lesion.—At the seat of inoculation in the mid line of the back there was a soft tumour immediately under the skin (measuring 2 in. by 1½ in. by ½ in.). On section it was composed of soft granulation tissue with a cavity just beneath the skin containing clear serous fluid.

The Right Axillary Gland contained discrete caseous foci in the cortex; minute grey foci were seen in the left gland.

Cervical Gland on the right side was enlarged and in part softened without typical caseation. The corresponding gland on the left side was smaller and showed part of the cortex greyish white and firmer.

Abdomen.

The Spleen was a little enlarged and contained about fifteen discrete greyish miliary tubercles.

Liver.—On the surface of the liver nine grey submiliary tubercles could be seen.

Kidneys and Suprarenal Bodies appeared normal.

Thorax.

Heart.—Normal.

The Pleural Cavities were free from fluid.

Lungs.—The lungs contained two minute grey points of a doubtful nature.

Bronchial Glands.—Normal.

Lumbar, Iliac, Mesenteric, and Sternal Glands.—Normal.

DOG PASSAGES.

First Passage with Virus B. IV.

DOG 54.

A collodion capsule, containing an emulsion of culture derived from the original material of Virus B. IV. was put into the peritoneal cavity of the dog on May 30th, 1905.

The culture used was 21 days old, twenty-eighth generation, and was grown on ox serum.

The dog was killed on October 9th, 1905. 142 days after. During the last 6 to 8 weeks of life the dog gradually lost flesh, and when killed was very thin

Post-Mortem Examination.

Body.—Emaciated.

The Peritoneal Cavity.—Contained from 20 to 30 cubic centimetres of brownish turbid fluid.

The glass part of the capsule was found free in the peritoneal cavity; it was filled with whitish flocculent pus.

In the gastro-splenic omentum there was a firm mass the size of a small hen's egg; it lay between the stomach and the tail of the spleen, the latter partly encircling it; on section it was composed of fatty and fibrous tissue, showing here and there some suspicious yellow spots, but no definite caseation or necrosis.

In the centre of the mass were the remains of the collodion part of the capsule.

The Mesentery.—Showed a few small grey tubercles in the centre of some of the areas between the vessels. The surface of portions of the small intestine appeared to be covered with a thin transparent adherent layer: this layer did not cover the surface uniformly, but in several places was seen as small filmy islands.

Parietal Peritoneum.—Normal.

The Peritoneum.—Covering the diaphragm was thickened and showed dense white fibrous tissue containing faint yellow striæ.

The Liver.—Was adherent to the diaphragm and showed a patchy thickening of the capsule: on section the substance was normal.

The Portal Gland.—Seemed a little enlarged, but on section was not obviously tuberculous.

The Spleen.—Was enlarged and contained near the hilum several soft dirty yellowish nodules, up to a large pea in size: these appeared to have developed as the result of direct extension from the tumour.

Kidneys.—Each kidney showed in the cortex numerous tubercles ranging in size from 1 to 1·5 or more millimetres in diameter; many of the tubercles were elongated in a radial direction; they were grey and had minute opaque yellow centres.

The Lungs and Bronchial Glands.—Appeared normal.

The Ventral Mediastinal Glands.—Were enlarged and firmer than normal; on section there was no definite caseation, but from the cut surface a pus-like fluid could be scraped.

Mesenteric Glands and other Lymphatic Glands.—Were normal.

Microscopical Examination (Smear Preparations.)

Scraping from Tumour.—Numerous T.B., mostly in clumps.

Fluid from Glass Tube.—Numerous T.B.

Ventral Mediastinal Gland.—Numerous T.B.

Spleen Nodule.—A few T.B.

Mesenteric Gland.—No T.B.

Guinea-Pigs Inoculated.

No. 1736 was inoculated intraperitoneally with some of the contents of the glass part of the capsule and died in 54 days of general tuberculosis.

No. 1737, inoculated intraperitoneally with an emulsion made from the tumour, died of tuberculosis in 17 days.

DOG 82.

Fed with culture derived from Dog 54.

Dose.—32·5 milligrammes.

Date.—December 15th, 1905.

Killed.—March 16th, 1906. 91 days after.

Post-Mortem Examination.

Body.—In good condition.

Tongue, Pharynx and Tonsils.—Normal.

Submaxillary, Pharyngeal and Cervical Lymphatic Glands.—Normal.

Intestines, Large and Small.—Normal.

Mesenteric and Colic Glands.—Normal.

Spleen.—Normal.

Liver.—Normal.

Portal Gland.—Normal.

Kidneys.—Each kidney showed in the cortex numerous tubercles, ranging in size from about 0·5 to 1·5 mm. in diameter; the large tubercles were greyish and translucent and had in most cases minute opaque centres; the smaller tubercles were spherical, pearly grey, of greyish-white, and when cut into seemed to have narrow grey margins and soft whitish centres (these bodies were not apparently of a tuberculous nature).

The Lungs.—Were crepitant throughout; in one cephalic lobe a small firm nodule was felt; this on section had a reddish colour and was not, apparently, of a tuberculous nature; with this exception the lungs were normal.

Bronchial and Mediastinal Glands.—Normal.

All other Organs and Glands.—Were normal.

Microscopical Examination (Smear Preparations.)

Kidney Tubercle.—No T.B. (Preparation showed a large amount of fatty material.)

Lung Tubercle.—No T.B.

Guinea-pig Inoculated.

Guinea-pig 1935, inoculated with an emulsion made from the kidney, remained healthy.

DOG 92. (Age 6 to 12 months.)

Fed with culture from Dog 54.

Dose.—About 25 milligrammes.

Date.—January 22nd, 1906.

Killed.—March 27th, 1906. 64 days after.

Post-Mortem Examination.

Body.—In good condition.

Tonsils and Pharynx.—Normal.

Submaxillary Glands.—A gland at the left angle of the jaw contained a firm nodule, about 5 mm. in diameter, composed of greyish-red tissue and yellow streaks of necrosis; the gland on the right side was normal.

Pharyngeal Glands and Cervical Glands.—Normal.

Abdomen.

Omentum and Peritoneum.—Normal.

Stomach.—Normal.

Small Intestine.—Mucous membrane normal.

Large Intestine.—Normal.

Mesenteric Glands.—Normal in size; under the peritoneum covering one of the glands there was a doubtful yellow focus; on section nothing abnormal was seen in the substance of the gland (? fat in subperitoneal tissue); a piece of this gland was inoculated into a guinea-pig. Other mesenteric glands were normal.

Ileo-colic and Colic Glands.—Normal.

Liver.—Normal.

Spleen.—Normal.

Suprarenal Bodies.—Normal.

Kidneys.—The left kidney showed in the cortex on the surface a grey tubercle, about 1 mm. in diameter, with a minute opaque central point.

The left kidney contained a similar but smaller tubercle.

Thorax.

Heart and Pleura.—Normal.

The Lungs.—Contained from 10 to 12 hard caseous nodules, ranging in size from a little over 1 to 2·5 mm. in diameter; the nodules were somewhat irregular in outline and had grey translucent margins.

Bronchial and Mediastinal Glands.—Appeared normal.

All other lymphatic glands were normal in size and appearance.

Testicles.—Normal.

Microscopical Examination (Smear Preparations).

Lung Nodules (three examined).—In each a few T.B. were found.

Guinea-pigs Inoculated.

No. 1955, inoculated intraperitoneally with emulsion from mesenteric gland remained healthy.

No. 1954 inoculated intraperitoneally with emulsion from a lung nodule died in 18 days of tuberculosis.

SECOND PASSAGE WITH VIRUS B. IV.

DOG 56.

Age.—About six months.

A collodion capsule, containing an emulsion of culture derived from the mesenteric gland of Chimpanzee 8, was put into the peritoneal cavity of the dog on June 8th, 1905. The sub-culture used was 15 days old, sixth generation.

Killed when ill and emaciated, August 23rd, 1905. 76 days after the experiment.

Post-Mortem Examination.

Body.—Very emaciated.

The Peritoneal Cavity.—Was filled with yellow serous fluid containing a few small flakes of lymph.

In the Great Omentum.—There was an elastic tumour the size of a large hen's egg, firmly adherent to the floor of the abdomen along the mid-line; it was adherent also to the mesentery and to several coils of the small intestine, but from these it could with care be separated. On section the tumour was composed of soft granulation tissue showing many yellow areas of necrosis; the latter had a spongy fibrous texture, and from them turbid puriform fluid could be expressed. In the centre of the tumour the capsule lay embedded, the collodion part was brittle and partially disintegrated; the glass portion of the capsule was filled with turbid purulent fluid.

The serous surface of the mesentery was roughened and finely granular.

The Parietal Peritoneum.—In the lateral regions was profusely covered with tuberculous granulations; a thin layer of yellow lymph covered the peritoneal surface of the diaphragm.

The Spleen.—Was not enlarged; the pulp was pale and firm; on the surface were a few flakes of lymph.

Liver.—Capsule covered with a thin grey fibrinous membrane, thicker and yellower in patches; substance softer and paler than normal; no tubercles seen.

The Portal Gland.—Was much enlarged and showed in the cortex yellow patches of necrosis from which puriform fluid could be squeezed.

Kidneys.—Normal.

Lungs.—Were crepitant and closely beset with hard grey tubercles, the largest the size of a pin's head.

Bronchial and Mediastinal Glands.—Not obviously affected.

Mesenteric Glands and other Lymphatic Glands.—Were normal to the eye.

Microscopical Examination (Smear Preparations).

Tumour (necrotic patch) } T.B. extremely numerous.
Fluid from glass tube }

Portal Gland.—T.B. very numerous.

Guinea-pigs Inoculated.

No. 1647 was inoculated subcutaneously with some of the turbid fluid from the capsule.

No. 1648 was inoculated intraperitoneally with an emulsion made from the abdominal tumour.

Both guinea-pigs died, the first in 37 days, the second in 91 days, of general tuberculosis.

DOG 74. (Adult Retriever.)

Subcutaneous.—Inoculation of culture from Dog 56.

Dose.—10 milligrammes.

Date.—October 13th, 1905.

Killed.—December 10th, 1905. 63 days after inoculation.

Clinical History.

A tumour formed under the skin of the abdomen, which ulcerated. The inguinal and axillary glands enlarged; the skin over them became adherent and then ulcerated. 38 days after inoculation there was a discharging ulcer at the seat of inoculation, an ulcer in the groin and one in the left axilla; the left hind leg was drawn up, movement evidently causing pain; later the left fore-leg also became contracted.

Post-Mortem Examination.

Condition.—Good.

Local Lesion.—In the skin of the abdomen on the left side near the root of a nipple there was a small sinus discharging serous fluid; on section this was found to communicate with a space in the subcutaneous tissues, measuring 2 in. in length and 1 in. in breadth; the walls were in contact and were lined with soft pinkish granulation tissue.

In the left groin there was a linear cicatrix with a small sinus in the centre; pressure caused thin pus to exude from the latter; the sinus terminated at the left inguinal gland and was lined with granulation tissue discharging pus.

The Left Inguinal Gland.—Was indurated, but was otherwise normal to the eye.

In the Left Axillary Region.—There were two small openings in the skin in the centre of small cicatrices; on removal of the skin these were seen to communicate with a large cavity in the axilla lying under the pectoral muscle and extending dorsally as far as the scapula; the cavity was lined with soft granulation tissue and contained a small amount of pus.

The Left Axillary Gland.—Was about twice the size of the right; on section it appeared normal.

Spleen, Liver and Portal Gland.—Normal.

Kidneys.—Each kidney showed in the cortex on the surface a few opaque greyish white tubercles, the largest about 1·5 mm. in diameter; on section the latter were slightly elongated in a radial direction.

Thorax.

Lungs.—The lungs were crepitant and contained fairly numerous evenly distributed, hard grey tubercles, mostly about 1 mm. in diameter; the tubercles showed no sign of caseation.

Bronchial Glands.—Normal.

Heart and Pleura.—Normal.

Mesenteric, Iliac and other Peripheral Lymphatic Glands.—Normal.

Miscroscopical Examination.

Scraping from sinus in groin.—One tubercle bacillus.
Scraping from cavity in axilla.—No tubercle bacilli.

Guinea-pigs Inoculated.

No. 1809, inoculated intraperitoneally with a scraping from the wall of the cavity in the axilla, died in 9 days of an acute infection.

No. 1808, inoculated intraperitoneally with an emulsion from a bronchial gland, was killed 42 days later and showed general tuberculosis.

DOG 116. (Puppy.)

Fed with culture from Guinea-pig 1808, inoculated with an emulsion from the bronchial gland of Dog 74.

Date—March 29th, 1906.

Dose—10 milligrammes.

Killed—July 5th, 1906, 98 days after the experiment began.

Clinical History.

The dog gradually lost appetite, and at the end of the first month had become very emaciated; its appetite then improved, and it began slowly to put on flesh; when killed it was in moderately good health.

Tuberculin Test.—May 10th, 1906. 42 days after the experiment began. Negative. Dose of tuberculin, 5·0 cc.

Post-Mortem Examination.

Tonsils and Pharynx.—Appeared normal.

Submaxillary Glands.—Normal.

Left Retro-Pharyngeal Gland.—Was enlarged and indurated; on section it showed at one extremity a large greyish white somewhat translucent patch containing irregular opaque whitish areas of caseation, some of which were beginning to soften; the cortex of the rest of the gland showed similar but smaller patches; one of these was greyish white and not caseous.

Just posterior to this gland was a small gland (cervical), the substance of which was almost completely replaced by a large greyish white patch, in the midst of which was an irregular opaque white caseous mass beginning to soften. Other cervical glands on this side were normal.

Right Retro-Pharyngeal Glands.—Showed patches in the cortex similar to those in the left pharyngeal; they replaced about half the substance of the gland.

Right Cervical Glands.—Normal.

Parotid Glands.—Normal.

Abdomen.

Omentum and Peritoneum.—Normal.

Intestines.—Scattered throughout the whole length of the small intestine in the submucous tissue were fairly numerous yellow foci, largest less than a millet seed in size, which could just be felt between the finger and thumb (smears from two of these showed no tubercle bacilli but numerous other organisms stained blue). All the Peyer's patches were normal and there was no ulceration anywhere.

Large Intestine.—Normal.

Gastric Glands.—Normal.

Mesenteric Glands.—The mesenteric glands seemed a little enlarged. The glands in the anterior part of the mesentery were normal except for one or two small grey foci. Passing backwards, the glands became more and more severely affected. Those in the middle of the mesentery showed around the cortices sharply defined greyish-white semi-opaque areas, varying much in size, the larger ones containing opaque white friable caseous patches or streaks. The terminal mesenteric gland and the ileo-colic glands showed almost the whole of the peripheral part of the cortex composed of greyish-white caseating tissue.

Colic and Rectal Glands.—Normal.

Spleen.—Was a little soft but not apparently enlarged; on the surface there was a small group of pearly white granules; on section the Malpighian bodies were conspicuous but there were no foci of necrosis.

Liver.—Was normal in general appearance, and showed minute grey foci sparsely scattered throughout; there were besides a grey tubercle the size of a millet seed and an opaque white tubercle about 0·5 mm. in diameter (tubercle bacilli found).

Portal Gland.—Normal.

Kidneys and Suprarenal Bodies.—Normal.

Thorax.

The lungs collapsed normally and showed no patches of consolidation; they contained, evenly distributed throughout, moderately numerous firm grey nodules ranging in size from a millet seed to that of a small pea; most of the nodules were opaque and caseous in the centre; three of the larger nodules were greyish red and showed small cavities in their interior containing a small quantity of semi-purulent material.

The bronchial glands were distinctly enlarged; one on the left side showed in the cortex an opaque pinhead-sized caseous focus; an intertracheo-bronchial gland contained a minute yellow point.

Heart and Pleura.—Normal.

Prepectoral, Prescapular, and Inguinal Glands.—Normal.

Iliac and Lumbar Glands.—Normal.

Microscopical Examination (Smear Preparations).

Left Tonsil.—No tubercle bacilli.

Mesenteric Gland.—Numerous tubercle bacilli.

Liver (large grey tubercle).—No tubercle bacilli.

Liver (white tubercle).—Seven tubercle bacilli.

Intestine (two smears).—No tubercle bacilli.

Lung (two nodules).—Numerous tubercle bacilli.

Animals Inoculated.

Guinea-pigs 2137 and 2138 inoculated intraperitoneally with emulsions made from lung nodules became tuberculous.

VIRUS B. IV. PASSAGE.

DOG 18.

Fed on August 23rd, 1904, with an emulsion made from the organs of Guinea-pigs 1148 and 1150, inoculated with milk of Cow 164.

Dose—10·15 million tubercle bacilli.

Killed—January 5th, 1905, 135 days later.

Post-Mortem Examination.

Two mesenteric glands were enlarged and tuberculous, and there were a few doubtful tubercles in the lungs.

A culture was isolated from a mesenteric gland and used to inoculate a series of rabbits (*see* page 104) and to feed Dog 132.

DOG 132.

Fed with culture obtained from Dog 18.

Date—June 1st, 1906.

Dose—50 milligrammes.

Initial Weight—7470 grammes. Final weight, 8,900 grammes.

Killed—August 17th, 1906, 77 days later.

Post-Mortem Examination.

Pharynx and Tonsils.—Normal.

Pharyngeal Glands.—The left pharyngeal gland contained a single small caseous focus; the right was normal.

Submaxillary and Cervical Glands.—Normal.

Intestines.—Normal.

Mesenteric Glands.—All the mesenteric glands with the exception of two in the anterior part of the mesentery contained small irregular patches and foci of caseation; these were situated in the cortex just beneath the capsule.

Ileo-Colic and Colic Glands.—Normal.

Spleen.—Normal.

Liver.—Normal.

Portal Gland.—Normal.

Kidneys.—Normal

Lungs.—In the left lung there was a grey miliary tubercle with a yellow caseous centre and also a minute slightly opaque grey tubercle; the rest of the lung was normal.

Bronchial Glands.—One on the right side contained an opaque caseous focus. One on the left side contained an irregular caseous tubercle.

Heart and Pleura.—Normal.

Microscopical Examination (Smear Preparations).

Mesenteric Gland.—A few tubercle bacilli.

Bronchial Gland.—No tubercle bacilli.

PASSAGE WITH VIRUS B. IX.

DOG 60. (Puppy.)

Fed on July 10th, 1905, with 1 mg. of culture derived from the original material of Virus B. IX.

Killed.—October 30th, 1905, 112 days later.

Abstract of Post-mortem Examination.

Tonsils and Intestines.—Normal.

The Pharyngeal, and Ileo-colic Glands contained a few discrete opaque white slightly gritty caseous tubercles; three or four mesenteric glands contained dense irregular caseous nodules, up to about 2 mm. in diameter.

There was no sign of tuberculosis in the internal organs.

A culture was isolated from a mesenteric gland nodule and inoculated into a series of rabbits and Dog 84 (by feeding).

All the rabbits died of general tuberculosis.

DOG 84. (Puppy.)

Fed with culture derived from a mesenteric gland of Dog 60.

Dose.—50·0 milligrammes.

Date.—January 2nd, 1906.

Killed.—March 16th, 1906, 73 days later.

Post-Mortem Examination.

Tonsils and Pharynx.—Normal.

Submaxillary, Pharyngeal and Cervical Glands.—Normal.

Small Intestine.—Most of the Peyer's patches showed a number of minute yellow points in the substance, but were not otherwise abnormal.

Large Intestine.—Normal.

Mesenteric and Colic Glands.—Normal.

Spleen, Liver, Kidneys and Suprarenal Bodies.—Normal.

Portal and Cœliac Glands.—Normal.

Heart and Pleura.—Normal.

Lungs.—Normal in general appearance, contained scattered hard grey miliary tubercles, many of which showed a minute opaque point in the centre.

Bronchial Glands.—Normal.

Microscopical Examination (Smear Preparations).

Peyer's patch.—2 T.B.

Lung tubercle.—No T.B.

Guinea-pig Inoculated.

A guinea-pig (No. 1936) inoculated intraperitoneally with an emulsion made from the lung, died in 40 days of general tuberculosis.

DOG 130.

Subcutaneous (partly intraperitoneal) inoculation of culture, 7th generation, 21 days old, isolated direct from the mesenteric gland of Dog 60.

Date.—May 17th, 1906.

Dose.—48 milligrammes.

Weight.—Initial weight 4,570 grammes. Final weight 4,500 grammes.

Died.—August 10th, 1906, 85 days after inoculation.

Post-Mortem Examination.

Local Lesion.—In the skin of the abdomen in the middle line there was an oval ulcer measuring 1 in. by 1¾ ins. It was perfectly clean and was in process of healing. At the posterior extremity the edge was undermined forming a sinus lined with granulation tissue which led down to a left inguinal gland.

In the skin of the left inguinal region there were two small ulcers with granulating sinuses leading to the glands and on the right side one ulcer and sinus. The subcutaneous tissues on both sides were œdematous.

The inguinal glands on both sides were a little enlarged and the cortices were moist and yellow in colour.

Thorax.

Heart and Pleura.—Normal.

The Lungs were congested and œdematous, but floated in water; the substance had a grey translucent appearance.

Bronchial Glands.—Normal.

Ventral Mediastinal Glands.—Slightly enlarged and œdematous.

Abdomen.

The Peritoneal Cavity.—Was distended with a large quantity of clear serous fluid.

The Omentum was thickened by a network of soft grey translucent tissue.

The Mesentery and Meso-colon showed a few scattered minute translucent tubercles.

Spleen.—Appeared normal.

Liver.—The substance of the liver was infiltrated with grey translucent tissue forming a close network.

The Portal Gland was enlarged and was distinctly yellow and necrotic throughout.

The Kidneys contained scattered grey foci in the cortices: on section some of these extended radially through the cortex.

Suprarenal Bodies.—Normal.

Lymphatic Glands.

Axillary, Submaxillary and Cervical Glands.—Œdematous but otherwise normal in appearance.

Mesenteric Glands were œdematous and had a uniform yellow appearance.

Iliac Glands on both sides were œdematous and the left contained a yellow necrotic area.

Microscopical Examination (Smear Preparations).

Liver, Kidney, Spleen, Omentum, Mesenteric Gland, Right Inguinal Gland.—All contained numerous tubercle bacilli.

Lung.—Showed a moderate number of tubercle bacilli, mainly in clumps and cells.

PASSAGE WITH VIRUS B. IX.

DOG 50. (Adult.)

Fed on June 2nd, 1905, with 1 mg. of culture derived from the original material of Virus B. IX.

Killed.—September 21st, 1905, 111 days later.

Abstract of Post-Mortem Examination.

Tonsils and Intestines.—Normal.

Neck Glands and Abdominal Lymphatic Glands.—Normal.

Lungs.—The left cephalic lobe was consolidated and showed a small cavity near the apex containing a small quantity of pus; the left bronchial gland was a little enlarged.

Each kidney contained a few opaque white spherical tubercles, the size of small pins' heads (not tuberculous).

Animals Inoculated.

Two guinea-pigs were inoculated, one No. 1708 with some of the pus from the cavity, the other No. 1707 with an emulsion made from the consolidated lung; both became tuberculous and cultures were isolated from each.

A series of rabbits and a calf No. 374 were inoculated with the culture isolated from Guinea-pig 1708, and two dogs were inoculated, one subcutaneously, the other by the mouth, each with 10 mg. of the culture obtained from Guinea-pig 1707.

Abstracts of the post-mortem notes of the rabbits are given on page 177.

CALF 374.

Subcutaneous.—Inoculation of culture, third generation, 20 days old, derived from the lung of Dog 50, through Guinea-pig 1708.

Date.—January 2nd, 1906.

Dose.—50 milligrammes.

Weight.—1 cwt. 2 qrs.

Killed.—April 3rd, 1906, 91 days after inoculation.

Clinical History.

The calf was inoculated subcutaneously on the left side of the neck on 2nd January, 1906.

A fortnight after inoculation there was a prominent firm rounded swelling, 3½ inches in diameter; the prescapular gland measured about 4 inches in long diameter.

There was no further increase in size, but during the latter part of the experiment the tumour diminished a little and began to soften.

The prescapular gland also diminished a little in size and became very hard and nodular.

About seven weeks after inoculation an enlarged prepectoral gland was felt.

There was no disturbance in the general health of the calf during the experiment.

Tuberculin Test.—March 23rd, 1906. Eighty days after inoculation. Positive. Rise of temperature, 2·4°C.

Weights.

There was a steady gain in weight during the experiment:—

January 2nd, 1906	-	1 cwt. 2 qrs.	0 lbs.
January 25th, 1906	-	1 cwt. 2 qrs.	11 lbs.
February 6th, 1906	-	1 cwt. 2 qrs.	27 lbs.
February 21st, 1906	-	1 cwt. 3 qrs.	22 lbs.
March 20th, 1906	-	2¼ cwts. 1 qr.	4 lbs.
April 3rd, 1906	-	2¼ cwts. 1 qr.	25 lbs.
Gain in weight	-	3 qrs.	25 lbs.
Rate of gain per week			8·4 lbs.

Temperature.

The temperature remained practically normal throughout the experiment.

Post-Mortem Examination.

Carcass in good condition.

Local Lesion.—At the seat of inoculation on the left side of the neck there was a hemispherical tumour, measuring 3 in. by 3 in. by 1¼ in., composed of a mass of dense yellow caseated tissue, slightly gritty around the margins, surrounded by a zone of fibrous tissue about 3 mm. in greatest thickness; under the skin the mass was separated from its capsule by a layer of thick caseo-pus, but elsewhere was firmly adherent; the fibrous capsule was adherent to the skin, which was thickened, and to the muscles.

The Left Prescapular Gland measured 3¼ in. by 2 in. by 1¼ in., and was very hard and irregular in outline; attached to the convex surface there was a short fibrous cord, the thickness of a crow quill, with caseo-calcareous centre; on section the gland was composed partly of dense gritty caseous masses and partly of fibro-calcareo-caseous tissue; on the convex surface the capsule was greatly thickened, measuring at one place 1 cm. in thickness.

Right Prescapular Gland.—Normal.

Prepectoral Glands.—On the left side the spherical gland was enlarged and contained an irregular calcareo-caseous mass, the size of a kidney-bean, with a fibrous margin; other prepectoral glands on this side were normal.

Right Prepectoral Glands.—Normal.

Cervical Glands.—A gland in the middle of the neck on the left side, the side of a broad bean, showed about half its substance calcareo-caseous. Other cervical glands on this side as well as those on the right side were normal.

Abdomen.

Omentum und Peritoneum.—Normal.

Spleen.—Weight, 20 ounces, normal on the surface and on section.

Liver.—Normal.

Gall-Bladder.—Normal.

Portal Glands.—One contained a number of yellowish-white foci, not perceptibly gritty; another gland contained three or four small very gritty tubercles; in a third gland there was one gritty tubercle.

Left Kidney.—Showed on the surface a minute greyish white focus.

Right Kidney.—Normal.

Suprarenal Bodies.—Normal.

Thorax.

Heart and Pleura.—Normal.

Lungs.—The lungs were normal in general appearance. In the left caudal lobe under the pleura three grey miliary tubercles were seen; one of these had a calcareous centre; besides these there were a few minute transparent points (? tubercles); in the rest of the lung one or two grey points similar to the latter were seen. No tubercles were seen on section.

The Bronchial and Dorsal Mediastinal Glands.—Contained scattered yellow calcareous tubercles, the largest the size of a pin's head.

Infra-tracheal and Ventral Mediastinal Glands.—Normal.

Larynx and Trachea.—Normal.

Tongue, Tonsils and Pharynx.—Normal.

Intestines.—Normal.

Urinary Bladder.—Normal.

Testicles.—Normal.

Thyroid Body.—Normal.

Eyes.—Normal.

Special Lymphatic Glands.

One Coeliac Gland.—Contained a small yellow slightly gritty tubercle.

An Ileo-Colic Gland.—Showed in the cortex two irregular yellow calcareous tubercles.

The Mesenteric, Colic, Gastric, Precrural, Popliteal, Gluteal, Pudic, Iliac, Ilio-Sacral, Lumbar, Renal, Sub-Maxillary, Left Pharyngeal, Mastoid and Axillary.—Were normal.

The Right Pharyngeal Gland.—Contained one small irregular calcareous tubercle.

Microscopical Examination.

Prescapular Gland.—Moderate number of tubercle bacilli.

Pharyngeal Gland.
Portal Gland.
Coeliac Gland.
Lung.
} No tubercle bacilli.

Animals Inoculated.

Guinea-pig 1959, inoculated with an emulsion made from the spleen, died in 37 days and showed general tuberculosis.

Calf 394 was inoculated subcutaneously with 20·5 c.c. of an emulsion made from the prescapular gland.

CALF 394.

Subcutaneous inoculation of an emulsion made from the prescapular gland of Calf 374.

Dose.—83·7 million tubercle bacilli.

Date.—April 3rd, 1906.

Weight.—1 cwt. Age, 8 weeks.

Killed.—June 6th, 1906. 64 days after inoculation.

Clinical History.

The calf was inoculated subcutaneously on the left side of the neck on April 3rd, 1906.

A week later a slight indefinite subcutaneous thickening, measuring about 2 by 1, was felt; the prescapular gland was very slightly enlarged.

The thickening gradually diminished and at last became imperceptible and the gland resumed its normal size.

The calf during the whole of the experiment suffered from diarrhœa which was especially severe in the last month.

The temperature remained practically normal through out (maximum 39·6°C.)

Tuberculin Test.—May 31st, 1906. 58 days after inoculation.—Negative.

Dose of tuberculin about 1 cc.

Weights.

During the first three weeks there was a gain in weight of 2 lbs.; in the last 43 days there was a loss of 7 lbs.

April 3rd and 18th, 1906 -	1 cwt.	0 qrs.	0 lbs.
April 24th, 1906 -	1 cwt.	0 qrs.	2 lbs.
May 15th, 1906 -	0 cwt.	3 qrs.	27 lbs.
June 6th, 1906 -	0 cwt.	3 qrs.	23 lbs.
Loss in Weight -	0 cwt.	0 qrs.	5 lbs.

Post-Mortem Examination.

Condition.—Moderate.

Local Lesion.—In the subcutaneous tissues on the left side of the neck there was a very thin patch of fibrous tissue, about 1 in. in diameter, beset with discrete tubercles, the largest the size of a millet seed; the tubercles were grey, the smallest ones being almost transparent, the largest slightly opaque, though not definitely caseous, in the centre.

The Left Prescapular Gland was normal in size and nothing abnormal was seen on section.

Right Prescapular.—Normal.

Prepectoral and Cervical Glands.—Normal.

Thorax.

Parietal and Visceral Pleura.—Normal.

Lungs.—Normal except for two or three collapsed lobules.

Bronchial and Mediastinal Glands.—Normal.

Heart and Pericardium.—Normal.

Abdomen.

Omentum and Peritoneum.—Normal.

Spleen.—Weight 4½ ozs.—Normal.

Liver.—Normal.

Portal Glands.—Normal.

The Right Kidney showed on the surface of a lobule a group of about ten grey foci, the largest a little more than 1 mm. in diameter; on section these extended into the cortex in a radial manner; the larger ones were yellowish in the centre.

Left Kidney and Suprarenal Bodies.—Normal.

Tonsils, Pharynx and Intestines.—Normal.

Submaxillary, Pharyngeal, Gastric, Mesenteric and Ileo-Colic Glands.—Normal.

Testicles.—Normal.

Various Lymphatic Glands.

Cœliac, Lumbar, Renal, Iliac, Precrural, Pudic, Popliteal, Axillary and Parotid Glands.—Normal.

DOG 110. (Adult Retriever.)

Subcutaneous inoculation of culture derived from Guinea-pig 1707, inoculated with an emulsion made from the lung of Dog 50.

Date.—March 21st, 1906.

Dose.—Slightly less than 10 milligrammes.

Killed.—June 28th, 1906, 99 days after inoculation.

Clinical History.

The dog was inoculated subcutaneously in the right flank.

Fifteen days later there was a flat tumour measuring about 3 in. by 1¾ in., showing at one part a deep ulcer, 1½ in. by 1¼ in., with slightly everted margins and floor formed by soft granulation tissue; there was a discharge of pus from the ulcer.

On April 12th, 1906, the ulcer was the size of a five-shilling piece and had abrupt clean cut margins. It then began to diminish in size; on May 10th, it was shallower and about ¾ in. in diameter and at the end of the experiment had almost disappeared.

On May 10th, a doughy swelling was noticed in the right axilla, the skin over which showed two or three small ulcers.

The general health of the animal was unaffected by the inoculation.

Tuberculin Test.—May 10th, 1906. 50 days after inoculation. Dose of tuberculin 5·0 cc. The temperature rose 1° C. twelve hours after injection.

Post-Mortem Examination.

Body in good condition.

Local Lesion.—The skin of the abdomen on the right side showed a superficial ulcer, about ⅛ in. in diameter, with a pale granular floor; on the under surface of the skin around the ulcer there was a ring of white fibrous tissue from the margins of which strands of fibrous tissue radiated in all directions, giving a puckered appearance.

The Axillary and Inguinal Glands were normal in size and general appearance.

Thorax.

Lungs.—The lungs contained two grey slightly opaque tubercles the size of rape seeds; otherwise the lungs were normal.

Bronchial Glands.—Normal.

Heart and Pleura.—Normal.

Abdomen.

Spleen.—Normal.

Liver.—A grey point was seen in the liver.

Portal Gland.—Was normal.

Kidneys.—Each kidney showed in the cortex scattered grey tubercles, with opaque centres, the largest about 1·5 mm. in diameter (not apparently tuberculous).

Suprarenal Bodies.—Normal.

A Mesenteric Gland.—Contained a yellow speck.

Other Lymphatic Glands.—Normal.

Microscopical Examination (Smear Preparations).

Liver.—No tubercle bacilli.

Kidney.—No tubercle bacilli.

Mesenteric Gland.—No tubercle bacilli.

Lung.—Two tubercles.

DOG 108. (Adult Retriever.)

Fed with culture obtained from Guinea-pig 1707 (inoculated with an emulsion made from the lung of Dog 50); the culture was mixed with sterilized milk.

Date.—March 21st, 1906.

Dose.—10 milligrammes.

Killed.—June 27th, 1906, 98 days after.

Clinical History.

There was no sign of illness during the experiment.

Tuberculin Test.—May 10. 50 days after the experiment began. Positive. Rise of temperature, 1·3°C. Dose of tuberculin, 5·0cc.

Post-mortem Examination.

There was no tuberculosis of the mucous membrane of the alimentary tract or of the glands in connection with it.

Spleen.—Contained a pea-sized nodule which projected from the surface; on section the nodule consisted partly of spleen tissue and partly of soft greyish substance (? lymphoid); no tubercle bacilli were found in a smear.

Liver, Kidneys, and Suprarenal Bodies.—Were normal.

Thorax.

Lungs.—In the lungs there was one grey somewhat opaque submiliary tubercle; otherwise the lungs were quite normal.

Bronchial Glands.—Normal.

All other Organs and Glands.—Normal.

Microscopical Examination (Smear Preparations).

Lung.—No tubercle bacilli.

Spleen.—No tubercle bacilli.

DOG PASSAGE WITH VIRUS B XXII.

DOG 72.

Inoculated intraperitoneally on September 4th, 1905, with culture derived from the original material through Guinea-pig 1509.

Dose.—1·0 milligramme.

Killed.—February 7th, 1906, 156 days after inoculation.

Post-mortem Examination.

There were a few nodules and some fibrous tubercles in the lungs, a few yellow foci in the liver, and some doubtful tubercles in the kidneys.

A culture was isolated from one of the lung nodules and used to inoculate a series of rabbits and Dog 126.

All the rabbits died of general tuberculosis.

DOG 126.

Subcutaneous inoculation of culture, 2nd generation, 22 days old, derived from the lung of Dog 72.

Date.—May 9th, 1906.

Dose.—10 milligrammes.

Weight.—Initial weight 4,850 grammes. Final weight 6,000 grammes.

Killed.—August 16th, 1906, 99 days after inoculation.

Post-mortem Examination.

Local Lesion.—In the skin of the abdomen to the right of the middle line there was a small, flat, granulating ulcer.

In the skin over the inguinal glands there were three small apertures, two on the right and one on the left, which communicated with sinuses in the periglandular tissues.

The inguinal glands were not enlarged; in the cortices there were a few fine, wavy, yellowish-white streaks.

Thorax.

Heart and Pleura.—Normal.

Lungs.—In the superficial part of the lung substance there were three translucent miliary tubercles.

Bronchial Glands.—Were small, one contained in the cortex a few whitish lines, a second contained a small irregular caseous focus.

Mediastinal Glands.—Normal.

Abdomen.

Peritoneum.—Normal.

Spleen.—Normal.

Kidneys.—Normal.

Liver.—Contained numerous translucent tubercles up to about 1 mm. in diameter; occasionally a tubercle showed a minute central opacity.

Portal Glands.—Normal.

Special Lymphatic Glands.

Mesenteric, Iliac, Lumbar, Submaxillary and Axillary Glands.—Normal

Microscopical Examination (Smear Preparations).

Liver.—A few scattered tubercle bacilli seen.

Lung.—No tubercle bacilli seen.

Bronchial Gland.—No tubercle bacilli seen.

PIG PASSAGE.

PIG 8.

Fed from October 1st, 1902, to December 31st, 1902 (92 days) with tuberculous milk of Cow 40.
Killed.—June 4th, 1903. 247 days after the experiment began.

Abstract from Post-Mortem Notes.

There were two or three submucous nodules in the small intestines, one of which was caseous; there was no ulceration; the mesenteric, pancreatic and ileo-colic glands were enlarged and caseous, and there was slight tuberculosis of the lungs, thoracic glands and liver.

Animals Inoculated.

An emulsion was prepared from the mesenteric glands and a pancreatic gland and inoculated into Pigs 10 and 12 and Calves 104 and 106.

PIG 10.

Subcutaneous.—Inoculation of an emulsion made from the mesenteric glands of Pig 8.
Dose of Emulsion.—10 cubic centimetres (no tubercle bacilli were found in the emulsion).
Date.—June 4, 1903.
Weight on June 9, 1 qr. 16 lbs.
Killed.—October 8, 1903. 126 days after inoculation.

Clinical History.

The pig was inoculated subcutaneously on the inner side of the left thigh on June 4th, 1903.

A month after inoculation there was a subcutaneous thickening about ¾ inch in breadth with a cord-like prolongation into the groin.

On July 28th, 54 days after inoculation a small nodular mass was felt, around which was grouped a number of tubercles of various sizes; the skin was freely movable. Subsequently the mass increased a little in size and the tubercles in number.

The animal showed no signs of ill-health during the experiment.

Post-Mortem Examination.

Carcass.—In moderately good condition.

At the Seat of Inoculation.—On the inside of the left thigh, there was a small irregular tumour surrounded by a number (two or three dozen) of hard tubercles. The skin was freely movable over the tumour, but was adherent to a few of the tubercles. On section the tumour measured 1½ in. by 1 in., and showed fibrous tissue enclosing softened caseous gritty material and a few discrete tubercles. The subcutaneous tubercles ranged in size from a pin's head to a small pea; each had a caseous gritty centre and a pinkish translucent periphery.

Similar tubercles infiltrated the underlying muscle.

Stretching from the under surface of the local tumour to the brim of the pelvis there was a thick cord; the distal end of the cord was one-third of an inch in diameter, and was composed of a pinkish translucent tissue, showing here and there a caseous focus, deposited around a thickened lymphatic vessel: the proximal end was narrower, and showed isolated tuberculous deposits.

Inguinal Glands.—The glands (five or six in number) in the left inguinal region, were fused together to form a mass weighing a little over an ounce, and measuring 3 in. by 1 in. by 1½ in. On section they consisted of dense whitish caseous substance, slightly gritty from calcification; all the glands were easily shelled out from their capsules.

Iliac Glands.—The glands (six to eight in number) in the left iliac region were enlarged, and formed a mass 3 in. by 1 in. by ¾ in.: the larger ones contained numerous dense greyish white caseous masses, slightly gritty from calcification, the smaller ones were similarly, but not so extensively affected.

The Gland at the bifurcation of the aorta showed similar areas.

The Right Inguinal and the Right Iliac Glands.—Were not enlarged and showed no visible tubercles.

Abdomen.

In the Omentum, close to its attachment to the stomach were half-a-dozen tubercles ranging in size from a pin-head to a small pea; the small ones were grey and translucent; the large ones caseous and calcareous.

The Peritoneum covering the anterior surface of the stomach (in contact with the liver) showed a number of minute hard translucent granules. Elsewhere the peritoneum and omentum were normal.

Hepatic Glands.—Between the stomach and the anterior margin of the pancreas there was a mass of enlarged glands, from ½ in. to 1½ in. in length; this group of glands was continuous with the group in the hilum of the liver; all showed irregular caseous areas, as in the iliac glands, the smaller glands being less affected than the larger.

Gastric Glands.—Under the peritoneum on the posterior aspect of the stomach there were several enlarged glands, and a mass 2¼ in. by 1 in. tuberculous throughout.

Mesenteric Glands.—At the lower end of the mesentery, there were three enlarged glands, showing caseating areas, gritty from calcification. Other mesenteric glands and glands along the large intestine were normal.

Large and Small Intestine, Tongue, Tonsils, Pharynx, Larynx.—Normal.

Spleen.—Weighed 3 ozs. On the surface, and apparently growing in the capsule, were four small grey translucent tubercles; in the parenchyma there were between twenty and thirty tubercles, ranging in size from a pin-head to a small pea; on section they were all caseous and gritty. Some of these tubercles caused projections from the surface and one little group (an eighth of an inch in diameter) was surmounted by a vascular tuft.

Splenic Lymphatic Glands.—Near the centre of the hilum there was a gland the size of a filbert, showing gritty caseating areas.

Liver.—Weighed 1lb. 12oz; beneath the capsule, and moderately evenly distributed throughout the parenchyma, were numerous tubercles; the larger ones, the size of a small pea, had on section a lobular outline, appearing as if made up of several tubercles; they were for the most part greyish, firm and homogeneous throughout; a small proportion, however, showed yellow calcareous centres.

Hepatic Glands.—(*See* above).

Kidneys.—Normal.

Suprarenal Bodies.—Normal.

Thorax.

Pleura.—A little to the left of the middle of the tendon of the diaphragm there was a hard grey nodule a quarter of an inch in diameter.

On the pleural surface of the diaphragm, chiefly in the centre and along the margins of the tendon, there were some pale villous-like growths; at the base of some of these minute hard granules could be felt. Of those along the margin of the tendon a few had towards their free ends small tubercles, two of which were reddish, caseous and calcareous.

Similar growths were seen on the pleura covering the last ribs; these growths were very long and slender; one on each side contained at the free extremity a flattened chalky-white body, the size of a wheat grain, almost entirely detached; one such body was found free in the right pleural cavity. On section they were dense, dry and caseous.

Near the apex of the somatic pleura on the left side there was a small grey tubercle.

Lungs.—Scattered evenly throughout the lungs were numbers of hard grey tubercles; they varied in size from a mustard seed to a small pea; all were caseous and softened in the centre; nearly all were appreciably calcified.

The lung tissue between the tubercles appeared to be normal.

Mediastinal and Bronchial Glands.—The mediastinal and bronchial glands were very large. One dorsal mediastinal gland measured 2 in. by 1⅛ in., two behind this were an inch and half an inch in length respectively.

Bronchial Glands.—Each bronchial gland measured 1½ in. by 1 in. by ½ in., and showed areas similar to those described in the iliac glands.

Two glands along the œsophagus, one above the treachea, and a group at the opening of the thorax (above the manubrium sterni) were similarly affected.

Heart and Pericardium.—Normal.

Thymus and Thyroid.—Normal.

Brain and Meninges.—Normal.

Ovaries.—Were cystic; in the tissue between the cysts there were several small greyish points.

Uterus and Fallopian Tubes, Pancreas.—Normal.

Various Lymphatic Glands.

Submaxillary Glands.—The left submaxillary gland was normal; one on the right side was enlarged and tuberculous.

Left Parotid.—Showed a caseous nodule.

Right Parotid, Right and Left Prescapular, Right and Left Cervical, Right and Left Popliteal, Right and Left Lumbar, Right and Left Posterior Pharyngeal.—Normal.

Microscopical Examination (Smear Preparations).

Emulsion of Inguinal Glands.—Twenty-seven tubercle bacilli.
Liver.—No tubercle bacilli.
Lung.—No tubercle bacilli.
Pleura (free body).—No tubercle bacilli.
Spleen.—No tubercle bacilli.
Omentum.—One tubercle bacillus.
Gastric Gland.—No tubercle bacilli.
Mediastinal Gland.—No tubercle bacilli.

Animals Inoculated.

An emulsion was made from the inguinal glands, and inoculated into:—

G. P. 629.	(S.)	Died 69 days.	General tuberculosis.
G. P. 630.	(S.)	Died 67 days.	General tuberculosis.
G. P. 631.	(I.)	Died 19 days.	Tuberculosis.
G. P. 632.	(I.)	Died 29 days.	Tuberculosis.

An emulsion was made from one ovary, and inoculated into:—

G. P. 633. Killed 70 days. No tuberculosis.

PIG 12.

Subcutaneous Inoculation of an emulsion made from the mesenteric glands of Pig 8.

Dose of Emulsion.—10 cubic centimetres. (No tubercle bacilli were found in the emulsion.)

Date.—June 4th, 1903.

Weight.—June 9th, 1903, 1 qr. 10 lbs.

Killed.—October 2nd, 1903, 120 days after inoculation.

Clinical History.

The pig was inoculated subcutaneously on the inside of the left thigh on June 4th, 1903.

A month after inoculation there was a tumour measuring 1½ inches by ¾ inch by about ½ inch, the skin over which was adherent and reddened.

The tumour slowly increased in size, and on August 6th, 1903, 63 days after inoculation, measured 2½ inches by 1½ inch. The tumour then diminished a little and a small ulcer appeared in the skin.

On July 7th, 33 days after inoculation, the glands (three in number) in the groin were enlarged, but separate; they increased in size and became adherent, forming a mass about the size of a small hen's egg.

There was no disturbance of the general health of the animal during the experiment.

Post-Mortem Examination.

The carcass was in good condition; fat everywhere moderately abundant.

At the Seat of Inoculation.—On the inside of the left thigh there was a slightly elevated firm tumour 1¾ in. by 1 in. by ½ in., the skin over it was adherent, dull red, and thinned, and showed at the centre a small ulcer.

On section the tumour consisted of fibrous tissue interspersed with dense chalky white caseous areas. The caseous deposits were more numerous at the periphery of the mass than in the centre. All were gritty from calcification.

In the subcutaneous tissue around the tumour there were perhaps a dozen caseous and calcareous tubercles about the size of wheat grains.

Inguinal Glands.—The glands (five or six in number) in the left inguinal region were enlarged and glued together to form a nodular mass 3 in. by 1 in., weighing one ounce. On section the glands were almost entirely caseous; they were surrounded by thin fibrous capsules, from which they were easily shelled out; the caseous substance was white and dense and gritty from calcification.

One gland in the right inguinal region was slightly enlarged, and showed on section dense greyish-white caseous masses, slightly gritty from calcification; a second gland, not apparently enlarged, contained a small caseous focus; a third gland in this series was normal

A gland at the bifurcation of the aorta was enlarged and infiltrated throughout with caseous gritty areas.

Iliac Glands.—On the left side two were similarly, but not so extensively affected; one right iliac gland, not enlarged, showed a white gritty caseous focus.

Abdomen.

Omentum and Peritoneum.—Normal.

Spleen.—Weighed 2 ounces. Distributed evenly throughout the parenchyma was a number (thirty to forty) of grey tubercles, the size of a large shot; some formed hemispherical projections beneath the capsule; almost all had caseous and calcareous centres.

Liver.—Weighed 1 lb. 12 ozs. On the surface, and uniformly distributed throughout the parenchyma, were numerous greyish tubercles, smaller than those in the spleen; a few only of these showed central caseation. The glands in the hilum were enlarged, and showed similar areas to those described in other glands.

Kidneys.—The left kidney weighed three ounces, and showed beneath the capsule in the cortex two grey translucent tubercles, the size of mustard seeds.

The right kidney weighed three ounces; the cortex contained one tubercle as above.

Suprarenal Bodies.—Normal.

Ovaries.—The size of French beans, were cystic, and showed in the little triangular areas between contiguous cysts a number of grey specks.

Uterus, Fallopian Tubes.—Normal.

Tongue, Pharynx, Larynx, Tonsils.—Normal.

The Glands at the angle of the jaw, parotid and submaxillary, three in number on each side, were firm and enlarged (the largest about the size of a thrush's egg); on section they showed greyish-white translucent areas forming a network throughout the gland tissue; these areas were not softened nor appreciably calcareous, but showed opaque whitish streaks in their centres.

Posterior Pharyngeal Glands, Right Cervical, and Left Prescapular Glands.—Were similarly affected, but not to the same extent.

Left Cervical, Right Prescapular.—Were apparently normal.

The Glands below the trachea, at the opening of the thorax, were enlarged and contained caseating areas.

There was a mass (about the size of a tangerine orange) of enlarged glands between the stomach and the anterior border of the pancreas, which on section were extensively caseated.

The Mesenteric Glands and glands along the whole length of the large intestine, were, without exception, slightly enlarged and showed similar areas of caseation; the areas in the mesenteric glands were more softened than those in other glands.

Lumbar Glands.—Showed a few caseous foci.

Large and Small Intestine.—Normal.

Thorax.

Lungs.—Uniformly scattered throughout normal lung tissue were numerous grey translucent tubercles, of the size of a pin's head; a number were grey and homogeneous throughout, but many showed minute yellow calcareous centres.

Bronchial and Mediastinal Glands.—Were enlarged and showed caseating areas and foci gritty from calcification. The ventral mediastinal glands were similarly affected.

Diaphragm.—The abdominal surface of the diaphragam carried on the central tendon from six to eight prominent rounded hard translucent tubercles, which varied in size from a mustard seed to a wheat grain.

On the pleural surface, there were about half a dozen similar tubercles.

Costal Pleura, Heart, Pericardium.—Normal.

Microscopical Examination (Smear Preparations).

The Emulsion.—Showed tubercle bacilli.
Spleen.—Showed no tubercle bacilli.
Lung.—Showed one tubercle bacillus.
Kidney.—Showed no tubercle bacilli.
Submaxillary Gland.—Showed one tubercle bacillus.
Left Inguinal Gland.—Showed no tubercle bacilli.
Emulsion of Mediastinal Gland, (used to inoculate G.P's.)—Showed one tubercle bacillus.

Paraffin Sections.

Sections through one of the grey tubercles in the kidney showed small cell groups, the central parts of which were necrosed; no giant cells or tubercle bacilli were seen (in four sections).

Sections through one of the ovaries showed clusters of small cells in the position of the grey foci; there was nothing in the arrangement of these to indicate a tuberculous origin or nature (*i.e.*, no giant cells, degeneration or tubercle bacilli).

Animals Inoculated.

An emulsion was made from the inguinal glands, part of the spleen and two hepatic lymphatic glands; the emulsion contained 900 tubercle bacilli per c.c.

Of this Emulsion,

Calf 124, Calf 130, Pig 14, Pig 16, — Each received 10 cc. subcutaneously.
G.P. 602, G.P. 603 — 1 c.c. subcutaneously.
G.P. 605, G.P. 604, — 1 c.c. intraperitoneally.

Emulsion of Mediastinal Gland.

G.P. 606. (Subcutaneously.)

Of the guinea-pigs inoculated with the first emulsion, one died 18 days later and showed only slight tuberculosis; the rest died of general tuberculosis in periods of 60 to 71 days.

Guinea-pig 606 was killed 76 days after inoculation and showed general tuberculosis.

CALF 104.

Subcutaneous.—Inoculation of an emulsion made from the mesenteric glands of Pig 8.

Dose.—10 c.c. (No tubercle bacilli were found).

Date.—June 4th, 1903.

Weight.—June 9th, 1903. 1 cwt.

Killed.—September 11th, 1903. 99 days after inoculation.

Clinical History.

The calf was inoculated subcutaneously on the left side of the neck on June 4th, 1903.

Three weeks after inoculation a small thickening was felt in front of the inferior extremity of the prescapular gland. A week later, this had increased to form a small nodular mass about 1 inch in diameter. Later discrete hard nodules were felt around the mass and a few appeared in the tissues posterior to the gland.

The prescapular gland, three weeks after inoculation, measured 2½ inches by 1½ inch, and at the end of five weeks attained its maximum, 3½ inches by 2½ inches.

The general condition of the calf remained good throughout the experiment.

The temperature rose on the 22nd day to 40·1° C.; it then gradually fell to 39·3° C. on the 25th day, and during the rest of the experiment was practically normal.

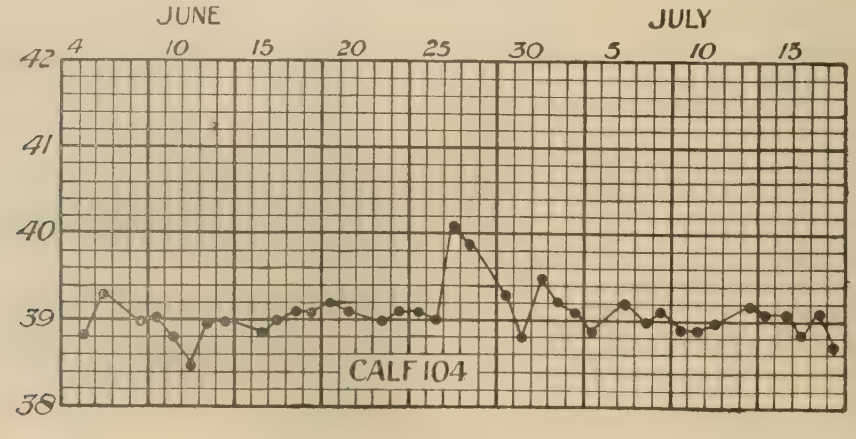
The tuberculin test on September 4th, 1905, 93 days after inoculation, was positive, the temperature rising 2·1° C.

Weights.

During the first seven weeks there was a gain in weight of 52 lbs.; then for a fortnight the weight was stationary; subsequently there was a steady increase in weight.

June 9, 1903 - -	1 cwt.	0 qr.	0 lb.
July 28, 1903 - -	1 cwt.	1 qr.	24 lbs.
August 11, 1903 -	1 cwt.	1 qr.	24 lbs.
September 8, 1903 -	1 cwt.	2 qrs.	10 lbs.
Gain in weight -		2 qrs.	10 lbs.

Temperature.



Post-Mortem Examination.

Carcass.—In moderately good condition.

Local Lesion.—On reflecting the skin from the left side of the neck, numerous yellow hard tubercles were seen in the subcutaneous tissue.

The tubercles lay for the most part anterior to the gland, but there was a small number situated postero-inferiorly. They varied in size from a small shot to a small pea. At one point anterior to the lower end of the gland they formed a nodular mass; elsewhere they were discrete. Some were closely adherent to the skin, others to the aponeurosis of the muscle. On section all had calcareous centres surrounded by pinkish translucent tissue. In the areolar tissue between the superficial and deep muscles, and 2 in. in front of the gland, was a small collection (twelve to fifteen) of similar calcareous tubercles; three others were situated nearer the gland.

The total number of tubercles was not more than 100.

Left Prescapular Gland.—Weighed three ounces, and measured 3 in. × 1¾ in. × 1¼ in. The gland tissue was firm, and the cortex was extensively infiltrated with yellow calcareous particles; near the hilum at the upper end there was an area of caseo-calcareous tissue; the calcareous particles were not so numerous at the lower end of the gland.

Lungs.—Irregularly distributed over the surface, but chiefly on the middle and posterior lobes, were numbers of small tubercles; some were seen just beneath the pleura, others projected. On section they varied in size from a small shot to a large pea and had calcareous centres, and were surrounded by an irregular zone of congestion. On the upper surface of the right lobe there were in addition two pinkish-red flattened tubercles with overhanging edges: the small one (⅛ in. in diameter) had a fringe around its margin, the large one (⅜ in. in diameter) calcareous grains at its base. On each dorsal surface of the posterior lobes there were three similar mushroom like growths, $\frac{5}{10}$ in. in diameter, surrounded by fringes.

On section of the lung, tubercles, the size of a No. 4 shot, were moderately numerous throughout the parenchyma; all were calcareous in the centre. The lung tissue apart from the tubercles was quite normal.

Around the margins of the lungs were delicate fibrinous fringes in which were felt small hard tubercles, grey in colour; delicate fringes were seen on the surfaces of the posterior and middle lobes, arranged along the lines of the interlobular septa.

Pleura.—On the anterior margins of the ribs, chiefly on the right side, were rows of firm pinkish vegetations, in which were, here and there, small hard grey nodules: similar vegetations were seen on the pleura covering the aorta, and a small number occurred at the junction of the muscle and tendon of the diaphragm, and on its central pillars; some of the vegetations on the diaphragm contained yellow caseous tubercles.

Mediastinal Glands.—The dorsal mediastinal glands, four in number, weighed four ounces: they were enlarged, and on section the gland tissue was very congested; in the long posterior gland (4 in. in length) there were numerous calcareous nodules affecting about half the cortex; the remaining three glands were calcareous throughout.

Three small glands along the thoracic trachea contained calcareous nodules.

Bronchial Glands.—Both were enlarged, the left weighing three ounces, and practically completely calcareous.

Heart.—On the left border of the left ventricle there was a prominent white firm oval nodule, the size of a filbert, homogeneous throughout, except for a small lacuna in the centre containing a greenish fluid. Microscopically the tumour was a fibroma.

Intestines.—Normal.

Mesenteric Glands.—Two glands, one at the upper and one at the lower end of the mesentery, each contained a small calcareous nodule.

Colic and Gastric Glands.—Normal.

Spleen.—Weighed nine ounces; on both surfaces there were several small red vascular tufts loosely connected to the capsule. On section there were in all about six shotty tubercles with calcareous centres.

Liver.—On the convex surface of the liver there were from fifteen to twenty small greyish-white tubercles, some just visible through the capsule, others projecting above the surface; also three yellowish white firm growths, from ⅛ to a ¼ in. in diameter, with thin overhanging margins and a slender attachment to the capsule. On the concave surface there were two hemispherical tubercles resembling small pearls.

On section, tubercles the size of a pin's head, calcareous in the centre, were sparsely distributed throughout the liver tissue.

Three Hepatic Lymphatic Glands.—Contained numerous calcareous particles in the cortex, the largest gland being the most affected.

Kidneys.—Each weighed six ounces. Both were normal on the surface and section.

Suprarenals.—Normal.

Tongue, Pharynx, and Tonsils.—Normal

Mammary Gland, Uterus.—Normal.

Various Lymphatic Glands: Left Prepectoral, Right Prepectoral, Right Prescapular, Axillary, Cervical, Submaxillary, Retro-pharyngeal, Lumbar, Iliac, Precrural, Popliteal, Gluteal, Ischial, Supramammary.—Normal.

Microscopical Examination (Smear Preparations).

Local Tumour.—No tubercle bacilli.

Left Prescapular Gland (Emulsion).—Tubercle bacilli.

Mesenteric Gland.—Tubercle bacilli.

Liver.—Tubercle bacilli.

Lung.—A few tubercle bacilli.

Spleen (Emulsion).—One tubercle bacillus.

Nodule on Diaphragm.—Two or three tubercle bacilli.

Animals Inoculated:—

With Emulsion of Left Prescapular Gland:

G. P. 562.—(S.) Died in 29 days of general tuberculosis.

G. P. 563.—(S.) Died in 38 days of general tuberculosis.

G. P. 564.—(I.) Died in 20 days of general tuberculosis.

G. P. 565.—(I.) Died in 29 days of general tuberculosis.

With Emulsion of Spleen (two tubercles):

G. P. 566.—(S.) Died in 39 days of general tuberculosis.

CALF 106.

Subcutaneous Inoculation of an emulsion made from the mesenteric glands of Pig 8.

Dose.—10 cubic centimetres. (No tubercle bacilli were found in the emulsion).

Date.—June 4th, 1903.

Weight.—June 9th, 1903. 1 cwt., 0 qr., 21 lbs.

Killed.—September 12th, 1903. 100 days after inoculation.

Clinical History.

The calf was inoculated subcutaneously on the left side of the neck on June 4, 1903.

Three weeks after inoculation a small irregular mass was felt at the lower extremity of the left prescapular gland. This increased in size a little and became very hard and nodular, and discrete nodules were felt in the subcutaneous tissues over the gland and along its posterior margin.

The prescapular gland, three weeks after inoculation, measured 2½ in. by 1½ in.; it slowly enlarged and at the end of seven weeks measured nearly 4 in. in length; it then underwent a slight diminution in size.

The general health of the calf remained good throughout the experiment.

The temperature rose sharply to 40·4°C. on the 22nd day and then gradually fell to the normal on the 25th; during the rest of the experiment the temperature was normal.

Tuberculin Tests.—September 4, 1903, 92 days after inoculation. Positive. Rise of temperature 2·3°C

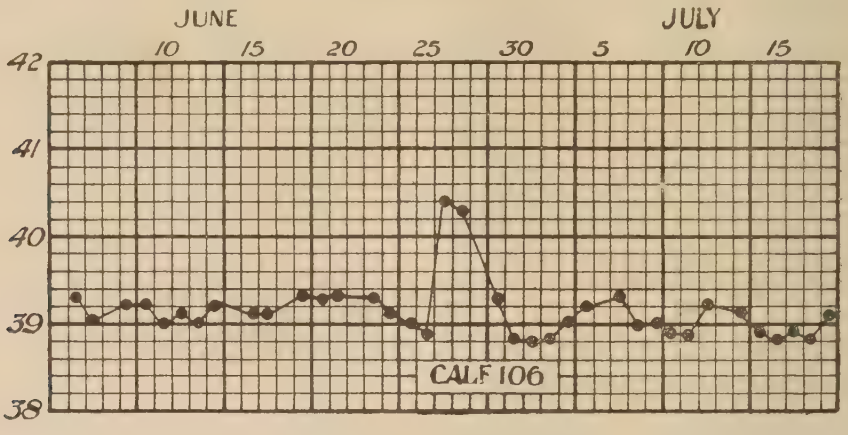
September 11, 1903, 99 days after inoculation. Positive. Rise of temperature 1·5°C.

Weights.

The weight steadily increased.

June 9, 1903.—1 cwt. 0 qr. 21 lbs.
September 8, 1903.—2 cwt. 0 qr. 4 lbs.
Gain in weight, 3 qrs. 11 lbs.
Rate of gain per week, 7·3 lbs.

Temperature.



Post-Mortem Examination.

Carcass in very good condition.

Local Lesion.—In the subcutaneous tissue immediately over that part of the sterno-mastoid muscle which covered the left prescapular gland were five yellow tubercles, varying in size from a small shot to a large pea; on section they had calcareous centres surrounded by pinkish translucent tissue.

In the loose tissue around the lower end of the left prescapular gland was a collection of similar yellow tubercles, numbering about 100. The tubercles were grouped chiefly around the inferior extremity of the gland, but they also extended half way up the posterior margin and slightly along the anterior; in the latter situations they were discrete and varied in size as above; in the former they formed nodular masses; all on section were calcareous in the centre.

Left Prescapular Gland.—Weighed 3 ounces and measured 3 in. by 1¾ in. On section the upper two-thirds of the cortex of the gland contained large caseo-calcareous masses; the lower third discrete calcareous particles.

Right Prescapular Gland.—Weighed a little over 1 ounce and measured 2½ in. by 1¼ in.; the tissue was œdematous, but there were no hæmorrhages.

At the seat of inoculation of the tuberculin on the right side there was a fairly well defined œdematous swelling. 5 in. by 3½ in. On section through the swelling the œdema was seen to infiltrate the underlying muscle, and to extend into the deep areolar tissue.

Thorax.

Lungs.—Normal on surface and on section.

Mediastinal Glands.—In the cortex of the long dorsal mediastinal gland there was a small yellow calcareous tubercle.

Other mediastinal and the bronchial glands were normal.

Pleura and diaphragm, normal.

Abdomen.

Omentum and Peritoneum.—Normal.

Spleen (9¾ ozs.).—Showed on both surfaces a few very small vascular tufts loosely attached to the capsule: on section normal.

Liver.—Normal.

Hepatic Lymphatic Glands.—Normal.

Kidneys and Suprarenal Bodies.—Normal.

Tongue, Tonsils, Pharynx
Intestines, Mammary Glands } Normal.

Other Lymphatic Glands.

Submaxillary, Posterior Pharyngeal, Cervical, Right and Left Prepectoral, Right and Left Axillary, Precrural, Popliteal, Gluteal, Ischiatic, Iliac, Lumbar, Gastric, Colic and Mesenteric Glands.—Normal.

Microscopical Examination (Smear Preparations).

Left Prescapular Gland.—Numerous T.B.
Tubercle from Local Lesion.—A few T.B.
Nodule from Mediastinal Gland.—No T.B.

Animals Inoculated.

With Emulsion from Left Prescapular Gland:—

G.P. 567. (S.) Died in 69 days of general tuberculosis.
G.P. 568. (S.) Died in 41 days of general tuberculosis.
G.P. 569. (I.) Died in 28 days of general tuberculosis.
G.P. 570. (I.) Died in 29 days of general tuberculosis.

With Emulsion from Spleen:—

G.P. 571. (S.) Died in 69 days of general tuberculosis.

PIG 14.

Subcutaneous Inoculation of an emulsion made from the inguinal glands, two hepatic lymphatic glands and part of the spleen of Pig 12.

Dose of Emulsion.—10 c.c., containing about 9,000 tubercle bacilli.

Date.—October 2nd, 1903.

Weight.—October 6th, 1903. 1 cwt., 0 qr., 25 lbs.

Killed.—January 25th, 1904. 115 days after inoculation.

Clinical History.

The pig was inoculated subcutaneously on the inside of the left thigh on October 2, 1903.

A fortnight after inoculation there was a tumour the size of a filbert, the skin over which was adherent; the inguinal glands were a little enlarged. Forty days after inoculation the tumour measured 3 in. by 1½ in. by about 1 in.; the skin was thoroughly adherent, dull red and scaly; in the groin there was a mass of enlarged glands.

On December 9th the centre of the tumour was felt to be softened and the skin in the centre was depressed, thinned, and showed some adherent scabs. Thickened

lymphatic vessels stretched between the tumour and the mass of enlarged glands in the groin. Subsequently the tumour ulcerated and diminished somewhat in size.

No signs of ill-health were observed during the experiment; the appetite remained good, and there was a gain in weight; the pig, however, was not so fat, and did not grow at the same rate, as two control pigs belonging to the same litter.

The temperature rose on the 7th day to 39·5°C., and fell to the normal on the next day; it rose again on the 18th day and was high for three days (maximum 39·9°C.); subsequently, except for two slight rises, one to 39·3°C., the temperature was normal.

Weights.

During the first four weeks there was a steady gain in weight, amounting altogether to 31 lbs.; during the remainder of the experiment the weight fluctuated between 1 cwt. 1 qr. 14 lbs. and 1 cwt. 2 qrs. 15 lbs.

October 6th, 1903 -	1 cwt.	0 qrs.	25 lbs.
November 3rd, 1903	1 cwt.	2 qrs.	0 lbs.
December 1st, 1903	1 cwt.	1 qr.	14 lbs.
December 22nd, 1903	1 cwt.	2 qrs.	15 lbs.
January 19th, 1904	1 cwt.	2 qrs.	9 lbs.
Gain in weight-		1 qr.	12 lbs.

Post-Mortem Examination.

Condition.—Moderate.

At the Seat of Inoculation.—On the inside of the left thigh, there was a ridge 3 inches long, ¾ inch broad, and about ¾ inch high. In the centre of this ridge there was a narrow shallow ulcer, with puckered margins, a little over 1 inch long; the ulcer at the anterior end discharged thick yellow pus, but elsewhere was cicatrised.

When cut into the ridge was found to be composed of fibroid tissue containing, principally in the deeper parts, scattered caseous and softened tubercles.

In the subcutaneous tissue adherent to this ridge there were a few discrete yellow tubercles, appreciably gritty.

Between the local lesion and the inguinal glands there was a slightly thickened lymphatic vessel, which showed here and there along its course caseous nodules; the lumen of the vessel was patent.

Inguinal Glands.—The superficial inguinal glands were enlarged, and fused together to form a mass 3 inches by 1½ inches by 3 inches, weighing 2 ounces. On section all the glands were caseous throughout, with the exception of one or two small glands at the margin of the mass, which contained discrete yellow foci; the caseous areas were appreciably gritty.

The superficial inguinal glands on the right side were normal in size and appearance.

Iliac Glands.—The iliac glands on the left side (five in number) were enlarged and indurated; the posterior one of the series, the size of a chestnut, was entirely caseous; the four glands anterior to this, and a quarter the size, were beset with caseous foci.

The iliac glands on the right side were slightly indurated but were otherwise normal.

Lumbar Glands.—Normal.

Abdomen.

Omentum, Peritoneum.—Normal.

Spleen.—Weighed 4 ounces, and showed no sign of tubercle either on the surface or on section.

Liver.—Showed on the surface beneath the capsule half-a-dozen minute whitish foci; in the depth of the liver substance there was a minute yellow focus with a gritty centre.

Kidneys.—The right kidney contained in the cortex, near the surface, a grey translucent point, just visible to the naked eye. Left kidney normal.

Suprarenals.—Both normal.

Gastric Glands.—On the posterior wall of the stomach, near the lesser curvature, and beneath the peritoneum, there was a group of glands, one of which contained a caseous, slightly calcareous nodule. One or two glands similarly placed, but on the anterior wall, were normal.

Portal Glands.—In the interval between the pancreas and the hilum of the liver there were four or five enlarged and indurated glands, which on section showed numerous caseous gritty foci.

Mesenteric Glands, Colic Glands.—Normal in size, showed no visible tubercles.

Small Intestine, Large Intestine.—Normal.

Thorax.

Lungs.—Contained a number (about thirty were counted in each lung) of caseous tubercles, ranging in size from a mustard seed to a large shot; the tubercles were softened in the centre, and the larger ones had distinct fibrous capsules.

Bronchial Glands.—On the left side were considerably enlarged, and closely beset with caseous tubercles.

The glands on the right side were only slightly enlarged, but contained similar caseous foci.

The glands in the dorsal mediastinum appeared normal.

Pleura (Parietal and Visceral), Heart, Pericardium.—Normal.

Larynx and Trachea.—Normal.

Right and Left Cervical (Superficial and Deep), Right and Left Prescapular Glands.—Normal.

Right and Left Prepectoral, Right and Left Precrural Glands.—Normal.

Post-Pharyngeal Glands, Submaxillary Glands.—Normal.

Testicles.—Both testicles, and the peritoneum covering them, were normal.

Over the site of the inoculation of the tuberculin (inside of right thigh) there was an area of desquamation of the superficial epithelium.

Microscopical Examination (Smear Preparations).

Left Inguinal Glands (Emulsion).—Two tubercle bacilli.

Liver.—No tubercle bacilli.

Lung.—No tubercle bacilli.

Guinea-Pigs Inoculated.

Guinea-pig 826 was inoculated subcutaneously with an emulsion from the spleen; it was killed after 97 days and found to be free from tuberculosis.

Guinea-pig 827, inoculated subcutaneously with an emulsion made from the liver, died in 95 days of general tuberculosis.

Guinea-pig 829, inoculated intraperitoneally with an emulsion from an inguinal gland, died in 34 days of general tuberculosis.

PIG 16.

Subcutaneous Inoculation of an emulsion made from the left inguinal glands, two hepatic glands and part of the spleen of Pig 12.

Dose of Emulsion.—10 c.c., containing about 9,000 tubercle bacilli.

Date.—October 2nd, 1903.

Killed.—January 16th, 1904. 105 days after inoculation.

Clinical History.

The pig was inoculated subcutaneously on the inside of the left thigh on October 2nd, 1903.

A fortnight after inoculation there was a tumour adherent to the skin, about the size of a filbert; the glands in the groin were a little enlarged.

Sixty-eight days after inoculation the tumour measured 5 in. by 2 in., and thickened lymphatic vessels were felt between it and the mass of enlarged glands in the groin; the skin over the centre of the tumour showed a few dry scabs.

The tumour then diminished a little in size and the

skin in the centre became depressed, thinned and finally ulcerated.

The general condition of the animal remained good throughout the experiment.

The temperature rose on the seventh day to 39·6° C., and during the first month there was slight irregular pyrexia (maximum 39·4°C.); subsequently, except for one or two slight rises, the temperature was normal.

Weights.

October 6th, 1903 -	- 1 cwt.	0 qrs.	7 lbs.
January 12th, 1904	- 1 cwt.	1 qrs.	24 lbs.
Gain in weight -	-	1 qr	17 lbs.

Post-Mortem Examination.

Condition.—Moderate.

Local Lesion.—Extending across the surface of the left thigh at the seat of inoculation there was a ridge about 4½ in. in length, and about ¾ in. in width at the widest part. At the base of the ridge, on its lower side, there was a narrow ulcer over an inch in length.

On dissection this ridge was found to be mainly composed of an aggregation of tubercles, mostly with yellow caseous centres. The skin was partially adherent to the mass.

Leading forwards from this lesion to the superficial inguinal glands on the same side there was a number of lymphatic vessels of the diameter of a goose quill. On section these were found to have no distinct lumen, and were almost entirely caseous.

Inguinal Glands.—The left superficial group of inguinal glands formed a single mass weighing 3½ ozs., and measuring 3½ in. by 2 in. by 1¼ in. On section the glands were necrotic and caseous throughout. The caseous tissue was firm, and was readily shelled out from the surrounding gland capsules.

On the right side the inguinal glands were of about normal size, but slightly indurated, and showed one or two points of caseation.

Iliac Glands.—On the left side the iliac glands were enlarged to three or four times the normal size, and were closely beset with caseous tubercles.

Both iliac and superficial inguinal glands were just perceptibly calcified.

Right iliac glands normal in size and appearance.

Lumbar Glands.—Normal in size and appearance.

Abdomen.

Omentum.—Normal.

Peritoneum.—The peritoneum covering the diaphragm on the right side showed two clusters of tubercles; there were also two or three tubercles on the peritoneum covering the right crus of the diaphragm (the tubercles on the tendon of the diaphragm caused subpleural as well as subperitoneal elevations); elsewhere the peritoneum, both parietal and visceral, appeared normal.

Spleen.—The spleen was of normal size, but in its substance there were about a dozen tubercles, the largest being about the size of a pea; these were caseous centrally, and had distinct fibrous capsules.

The Liver contained numerous tubercles underneath the peritoneum, also throughout the substance; these were about the size of pin-heads.

Kidneys and Suprarenal Bodies.—Normal.

Gastric Glands.—The gastric group of glands was enlarged, and almost completely destroyed by caseation.

Stomach.—Normal.

Intestines.—Several Peyer's patches showed suspicious opacity of their lymphoid tissues (a smear preparation was made from one of the patches, but no tubercle bacilli were found). Otherwise normal.

Large Intestine.—Normal.

Mesenteric Lymphatic Glands, Colic Lymphatic Glands were normal in size, and showed no visible lesions.

Tongue, Throat, Tonsils, Post-Pharyngeal Lymphatic Glands, Submaxillary Glands.—Normal.

Thorax.

Lungs.—Both lungs contained numerous tubercles (some hundreds), mostly about the size of barley grains or smaller; these had distinctly caseous centres.

The Bronchial Glands on both sides were enlarged, and closely beset with caseous tubercles.

Pleura.—Several yellow tubercles about the size of barley grains were present in the mediastinal pleura behind the heart, and also in the special fold of pleura surrounding the vena cava.

On each side of the chest wall, under the pleura covering an intercostal space, there was a single tubercle.

Pericardium, Heart Muscle and Valves.—Normal.

Thymus.—Normal.

Cervical Glands, Prescapular and Prepectoral.—Normal on both sides.

Right Popliteal.—Normal.

Testicles.—Under the peritoneum covering the left spermatic cord and epididymis there were about half a dozen firm tubercles, the largest about the size of a barley grain. The testicle itself, and the substance of the epididymis, appeared normal.

Bladder.—Normal.

Microscopical Examination (Smear Preparations).

Emulsion of Inguinal Glands.—Tubercle bacilli (not numerous).

Liver.—No tubercle bacilli.

Spleen.—Tubercle bacilli (scanty).

Intestine (Peyer's Patch) } No tubercle bacilli.
Lung. }

Animals Inoculated.
With Emulsions.

Four guinea-pigs were inoculated, two subcutaneously, and two intraperitoneally, with an emulsion made from the left inguinal glands; all died of general tuberculosis in periods varying from 24 to 56 days.

With Culture.

A culture was obtained from an inguinal gland and inoculated into two rabbits (*see* below), two pigs, and a calf.

Rabbits.

No. of Rabbit.	Initial Weight in grammes.	Final Weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
159	1,700	1,200	1·0 mg.	Intravenous	Died, 21 days	General tuberculosis.
158	1,000	1,070	1·0 mg.	Intraperitoneal	Died, 62 days	General tuberculosis; the inoculation was partly subcutaneous.

CALF 124.

Subcutaneous Inoculation of an emulsion made from the inguinal and hepatic lymphatic glands and the spleen of Pig 12.

Dose.—10 cubic centimetres, containing approximately 9,000 tubercle bacilli.

Date.—October 2, 1903.

Weight.—1 cwt.

Died.—December 12, 1903, 71 days after inoculation.

Clinical History.

The calf was inoculated subcutaneously on the left side of the neck on October 2nd, 1903.

A fortnight after inoculation there was a small local thickening. At the end of the third week there was a tumour measuring 3½ in. by 2½ in. by about 1 in.; this slowly increased in size, and at the end of the 8th week measured 5 in. by 3 in.

The left prescapular gland at the end of a fortnight was slightly enlarged, and on November 20th, seven weeks after inoculation, measured nearly 5 in. in length.

The calf was first noticed to be ill about seven weeks after inoculation; the coat was staring and there was some loss of appetite. On November 28th, 57 days after inoculation there was slight diarrhœa, and on November 30th, the respirations were visibly increased. These symptoms increased in severity; diarrhœa became more marked: the respirations were rapid and laboured, expiration ending audibly; the appetite was poor and there were progressive emaciation and weakness.

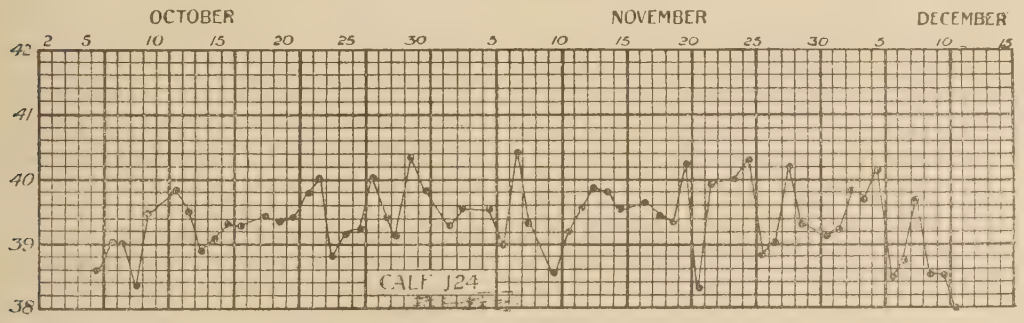
On the day before death the calf was very weak and emaciated and almost unable to stand; the head was outstretched, and respiration was very laboured.

Weights.

During the first month there was a gain in weight of 15lbs., then there was a gradual decline.

October 6th, 1903 -	1 cwt.	0 qr.	0 lb.
November 3rd, 1903 -	1 cwt.	0 qr.	15 lbs.
December 8th, 1903 -	0 cwt.	3 qrs.	22 lbs.
Loss in weight - - - -			6 lbs.

Temperature.



Post-Mortem Examination.

Carcass.—Very emaciated.

At the Seat of Inoculation.—On the left side of the neck, in the subcutaneous tissues, there was a rounded prominent tumour 4 in. by 2 in. by 1½ in. The skin over it was thoroughly adherent, but not ulcerated. On section the tumour was entirely composed of a congested, firm yellowish white tissue, dotted here and there with minute opaque yellow calcareous particles; the peripheral parts of the mass were broken up into nodules surrounded by hyperæmic fibrous tissue. At the inferior extremity of the tumour, and partly covered by it, there was a mass (2 in. by 1 in.) of caseous, slightly gritty tubercles, surrounded by firm tissue. A thickened tortuous caseous lymphatic vessel could be traced from the tumour to the left prescapular gland.

The tissues around the local tumour and the neighbouring glands were œdematous and congested.

Left Prescapular Gland.—Weighed 5 ozs., and measured 4 in. by 2 in. by 1⅛ in. On section it was caseous throughout and slightly gritty.

Prepectoral Glands.—A left prepectoral gland, the size of a small walnut, was deeply congested, and showed in the cortex a caseous mass affecting about one-third of the gland substance; in the less affected portion minute grey foci were seen. A second and deeper prepectoral gland was a little enlarged and showed one greyish focus. A third was not obviously affected.

Cervical Glands.—Two glands on the left side of the trachea, about midway, the size of broad beans, were caseous throughout. A corresponding gland on the opposite side was normal.

A gland a little anterior to the former, and slightly smaller, was slightly affected.

A gland about the level of the larynx, twice the size of a broad bean, was also tuberculous throughout.

Two small glands on the same side of the trachea (left), near the anterior opening of the thorax, were tuberculous; one showed a mass which occupied more than half the cortex, the other minute greyish-yellow foci.

Abdomen.

Omentum.—Free from fat. Showed on the inferior surface about a dozen tubercles, 1 to 2 mm. in diameter, a few of which were deeply congested; the surface in contact with the stomach and intestines was normal.

The peritoneal cavity contained about 6 ozs. of blood-stained fluid.

Spleen.—Weighed 4½ ozs. On the convex surface there was a firm flattened nodulated growth 7 mm. in diameter. On section the pulp was filled with nodules from 1 to 7 or 8 mm. in diameter. All the nodules were caseous in the centre, greyish around the margins: several contained calcareous grains.

Liver.—Weighed 4½ lbs. On both surfaces there was a considerable number of pearly nodules, ranging in size from 1 cm. downwards. On the convex surface the larger projecting nodules were flattened and had slightly overhanging margins; on the concave surface they were rounded. On section through the liver tubercles were moderately numerous in every part; these rarely exceeded 5 mm. in diameter. All the growths exhibited central caseation; a few only contained calcareous grains.

Hepatic Lymphatic Glands (four in number).—Two large, with nodular surfaces, and two small, weighed together 2½ ozs., and consisted throughout of masses and nodules of caseous, slightly gritty tuberculous tissue.

Kidneys.—The left weighed 9 ozs. and showed in the cortex near the surface twelve to fifteen slightly caseous tubercles ranging in size from a mustard seed to a good-sized shot. The tubercles appeared on the surface, but did not project. In the deeper parts of the cortex similar tubercles were seen, but these were few in number. In addition to the above there were three wedge-shaped areas occupying the whole thickness of the cortex, and projecting slightly above the surface, the largest being 1 cm. long by 0·5 cm. broad; these appeared to be formed of a number of small tubercles which had coalesced.

The right kidney (10 ozs.) showed similar, but less numerous tubercles, and two small wedge-shaped areas; the contiguous halves of two lobules in this kidney were grey in colour and anæmic.

Suprarenal Bodies.—The right contained in the medulla a grey tubercle the size of a pin's head; tubercle bacilli were found in a smear prepared from this. The left was normal.

Small Intestine.—Congested in parts (? hypostatic); no tubercles found.

Large Intestine.—No tubercles found.

Gastric Glands.—One gastric gland contained a caseating nodule 5 mm. in diameter. Two others showed minute foci.

Mesenteric Glands.—All the mesenteric glands, with about four exceptions, these being chiefly towards the posterior end, contained caseating nodules ranging in size from a small seed to a small pea.

Thorax.

Lungs.—With the thoracic glands attached, weighed 9 lbs. 4 ozs. The anterior lobes and anterior portions of the posterior lobes were completely consolidated, deeply congested and oedematous. The portions of the posterior lobes not entirely solid showed patches of consolidation alternating with oedematous and congested crepitant lung tissue.

The tissue of the lung was packed with tubercles and nodules ranging in size from 1 mm. or less up to 1 cm. in diameter. Those on the surface of the anterior lobes and the ventral surfaces of the posterior lobes formed greyish white projections, the majority rounded, but a few flattened and mushroomed. On section the nodules were grey and translucent, and contained centrally a caseous focus, in many cases gritty; the nodules had an irregular outline, and in several places had become confluent.

The fringes around the margins of the lungs were not hypertrophied, but showed at wide intervals a few small hard tubercles.

On the costal pleura, along the margins of the ribs, there were on each side from fifteen to twenty caseous tubercles 1 to 3 mm. in diameter; they occurred singly and in clusters of three and four.

Mediastinal Glands.—In the dorsal mediastinum there was a chain of large glands, seven in number, weighing together 9 ozs., the long posterior gland alone weighed ·6 ozs. and measured 5½ in. by 1¾ in. by 1½ in. On section the glands were tuberculous throughout and slightly gritty; the two glands in the anterior part of the mediastinum showed more advanced caseation than those in the posterior.

Bronchial Glands.—The left bronchial gland weighed 1½ ozs., the right about 1 oz.; both were caseous throughout.

A small gland in front of the right bronchial, the size of a French bean, was congested and showed a few grey foci.

Two peribronchial glands on the right side, the size of walnuts, were entirely caseous.

A small gland on the oesophagus contained a pea-sized caseous nodule; a second one showed minute grey foci.

The ventral mediastinal glands were slightly enlarged but not apparently tuberculous.

A gland in the caval fold of the pleura contained a caseous nodule the size of a large pea; a second gland in the same fold, but nearer the centre of the tendon, contained a nodule half the above size.

Diaphragm.—On the pleural surface of the diaphragm, on the left side, there was a group of four small caseous tubercles. Around the margins of the tendon there were a few delicate, very vascular outgrowths.

On the abdominal surface, dorsally on the right side, there were from fifteen to twenty flattened subperitoneal tubercles, about the size of wheat grains.

Pericardium.—The external surface presented some intensely red velvety granulations, and a number of small greyish-yellow tubercles.

The internal surface was normal.

The pericardial sac contained a small amount of yellow fluid.

Heart Muscle and Valves.—Normal.

Pancreas, Thymus, Thyroid, Tongue, Pharynx, Larynx, Tonsils.—Normal.

Various Lymphatic Glands.—Along the whole length of the abdominal aorta there was a chain of glands, eight to ten in number, ranging in size from a broad bean up to 2 in. in length. On section they all contained confluent caseous masses; in the case of the smaller glands replacing the whole of the gland tissue.

Iliac Glands.—A left iliac gland showed in the cortex two caseous nodules, 4 mm. in diameter; two small glands near this were not obviously affected.

A right iliac gland contained a caseous nodule 5 mm. in diameter.

A gland at the bifurcation of the aorta contained a similar nodule.

Right Submaxillary.—Contained a caseous tubercle the size of a No. 4 shot.

Right and Left Axillary, Right Prescapular, Right and Left Prepectoral, Right and Left Precrural, Right and Left Popliteal, Right and Left Gluteal, Right and Left Ischiatic, Right and Left Parotid, Right and Left Posterior Pharyngeal, Left Submaxillary, Vertebral.—Normal in size; no tubercle.

Microscopical Examination (Smear Preparations).

Prescapular Gland (Emulsion), *Right Suprarenal Body* } Tubercle bacilli.

Animals Inoculated.

With Emulsion of Prescapular Gland:—

G.P. 749. (S.), G.P. 750. (S.) } died in 37 days of general tuberculosis

G.P. 751. (I.) died in 21 days, G.P. 752. (I.) died in 20 days } of general tuberculosis.

CALF 130.

Subcutaneous.— Inoculation of an emulsion made from the inguinal and hepatic lymphatic glands and the spleen of Pig 12.

Dose.—10 cubic centimetres, containing approximately 9,000 tubercle bacilli.

Date.—October 2, 1903.

Weight.—October 6, 1903. 3 qrs. 8 lbs.

Killed.—January 15, 1904. 105 days after inoculation.

Clinical History.

The calf was inoculated subcutaneously on the left side of the neck on October 2nd, 1902.

A fortnight later there was a small patch of thickening at the seat of inoculation and the left prescapular gland was about twice the normal in size.

Three weeks after inoculation there was a flat swelling 3½ in. by 2 in., adherent to the skin ; the prescapular gland measured 2½ in. in length.

Seven weeks after inoculation the tumour measured 4 in. by 2½ in. and the prescapular gland about 3 in. in length. Subsequently there was little change in the local conditions. The calf remained in good health during the experiment and gained in weight.

Tuberculin Tests.—January 6th, 1904, 96 days after inoculation. Positive. Rise of temperature 1·1° C.

January 13th, 1904, 103 days after inoculation. Positive. Rise of temperature 1·0° C.

Temperature.

Twelve days after inoculation the temperature rose to 39·8° C., the highest point reached during the experiment. Subsequently it remained very constant, varying only between 39·1 and 39·5° C.

Weights.

The calf was weighed every week and showed an almost steady uniform increase in weight.

October 6th, 1903 -	- 0 cwt. 3 qrs. 8 lbs.
January 12th, 1904 -	- 1 cwt. 1 qr. 26 lbs.
Gain in weight - -	- 2 qrs. 18 lbs.
Rate of gain per week	- 5·25 lbs.

Post-Mortem Examination.

Carcass.—Condition fair.

At the Seat of Inoculation.—On the left side of the neck there was a firm nodular thickening, 3 in. by 2½ in. ; when removed and cut into the tumour was seen to be from ½ to ¾ inch thick, and to consist of closely packed nodules 2 to 4 mm. in diameter, surrounded by fibroid tissue. The nodules were yellow in colour, and appreciably calcified ; they were for the most part separate, and some had coalesced to form large masses. The skin was adherent, but not ulcerated or infiltrated.

The growth was fixed to the aponeurosis of the superficial muscles. The muscles showed three small groups of nodules continuous with the main mass ; one proceeded from the anterior extremity, one from the posterior, and one from the under surface ; with the exception of these the tumour was wholly subcutaneous.

In the subcutaneous tissues over the inferior extremity of the prescapular gland there were two or three isolated calcareous tubercles ; similar tubercles occurred at the extremity of the gland, embedded in the muscle and fat ; calcareous tubercles, some subcutaneous, others intramuscular, were seen between the tumour and the mid line of the neck.

Left Prescapular Gland.—Weighed about an ounce, and measured 2½ in. by 1½ in. by ¾ in. On section it showed in the midst of the gland substance an oval mass (1 in. by ¾ in) which consisted of firm caseous tissue, slightly gritty in the central part, highly calcareous around the periphery ; in the cortex there were scattered calcareous nodules showing little caseous tissue. The lower part of the gland showed two similar nodules (the size of a pea) and minute scattered calcareous grains ; with these exceptions, this part of the gland had a normal appearance.

Prepectoral Glands (on left side).—The most superficial gland, the size of a walnut, showed beneath the capsule numerous elevated yellow nodules. On section it consisted throughout of firm caseating tissue, perceptibly gritty. A second gland, more deeply situated, and about half the size, contained a few yellow gritty foci—otherwise normal.

Cervical Glands.—A lower cervical gland on the left side, an inch in front of the manubrium sterni, showed caseous foci. A middle cervical gland on the same side showed similar foci.

Upper Cervical Glands.—Normal.

Right Middle and Lower Cervical Glands.—Normal.

Right and Left Axillary Glands.—Normal.

Right Prescapular Gland was enlarged, measuring 3 in. by 1¼ in. by ½ in. ; on section soft and œdematous, with a patch of congestion (tuberculin test 41 hours previously) ; The subcutaneous tissues and muscles at seat of inoculation of tuberculin were slightly œdematous.

Right Prepectoral Glands.—Normal.

Abdomen.

Omentum.—Moderately fatty ; free from tubercles.

Peritoneum.—Everywhere normal.

Spleen.—Weighed 6 ozs. Normal on the surface ; contained in the pulp about a dozen yellow tubercles from 2 to 3 mm. in diameter ; the larger nodules only showed caseous and gritty foci centrally.

Liver.—Weighed 3 lbs. 6 ozs. On the anterior surface, beneath the capsule, but embedded in the parenchyma, there were two yellow nodules, one 2 mm., the other 4 mm. in diameter. The former was not caseous, the latter projected slightly, had a flattened surface, and was caseous in the centre.

On section the liver appeared normal throughout.

Hepatic Lymphatic Glands (three in number).—Two normal ; one contained a few calcareous grains.

Gall Bladder.—Normal.

Kidneys.—Each weighed 6 ozs. Both normal.

Suprarenal Bodies.—Normal.

Pancreas.—Normal.

Two glands between the stomach and the left pillar of the diaphragm were normal.

Gastric Glands, Mesenteric, Colic.—Normal.

Small Intestine, Large Intestine.—Normal throughout.

Tongue, Pharynx, Tonsils, Œsophagus, Posterior Pharyngeal Glands, Submaxillary, Parotid, Mastoid Glands.—On both sides normal.

Thorax.

Pericardium.—Contained a small amount of clear fluid.

Heart Muscle and Valves.—Normal.

Lungs.—Right cephalic lobe, normal.

Left cephalic lobe showed on the superior surface two soft vascular fibrous tufts, and in the substance beneath the pleura one grey translucent tubercle 2 mm. in diameter, and two or three minute grey points.

Left caudal lobe carried on the mediastinal surface a mushroomed nodule 4 mm. in diameter ; on this surface there were seen beneath the pleura two minute tubercles similar to those in the cephalic lobe.

On the superior surface of the right caudal lobe there was a small mushroomed growth; at the posterior margin of the lobe there was a firm nodule, 1 cm. in diameter, which occupied part of a lobule, and showed on section yellow foci; loosely attached to the free edge of this lobule there was a pinkish, fleshy fringe. Beneath the pleura in other parts of the lobe there were three caseous and gritty tubercles, 1 to 3 mm. in diameter; on section a similar tubercle was found in the substance.

The lung tissue, apart from the above-mentioned bodies, had a perfectly normal appearance.

Dorsal Mediastinal Glands.—The long caudal gland, not obviously enlarged, showed in the cortex here and there groups of yellow gritty foci. The two glands anterior to this showed scattered calcareous grains.

Left Bronchial Gland.—Not enlarged; contained a few groups of yellow gritty foci.

Right Bronchial Glands (two in number) were similarly affected.

Peribronchial Glands (on each side) also showed calcareous foci.

Ventral Mediastinal Glands.—Normal.

The glands just within the anterior opening of the thorax were normal.

Parietal pleura and parietal surface of diaphragm normal.

Larynx, Trachea, Mammary Gland, Supramammary Lymphatic Glands, Iliac Glands, Sacro-Iliac Glands, Lumbar Glands, Precrural Glands, Popliteal Glands, Gluteal Glands, Ischiatic Glands, Ovaries, Uterus, Fallopian Tubes, Thymus, Thyroid.—Normal.

Microscopical Examination.

Smears from:—

Prescapular Gland.—Tubercle bacilli.

Liver.—A few tubercle bacilli.

Spleen.—No tubercle bacilli.

Spleen.—No tubercle bacilli.

Animals Inoculated:—

An emulsion was prepared from the prescapular gland, and equal quantities inoculated into Guinea pigs 796 and 797 subcutaneously and 798 and 799 intraperitoneally; all died tuberculous in periods varying from 22 to 44 days.

PIG 98.

Subcutaneous.—Inoculation of culture from Pig 16.

Dose.—1·0 milligramme.

Date.—January 10, 1905.

Weight.—January 17, 1905. 1 qr. 16 lbs.

Died.—March 29, 1905, 78 days after inoculation.

Clinical History.

The pig was inoculated subcutaneously in the wall of the abdomen on January 10, 1905.

Three weeks after inoculation there was a flat tumour 1 in. in greatest diameter; the right inguinal glands were moderately enlarged. Subsequently the local conditions showed very little alteration.

The animal was first noticed to be ill on March 1, 50 days after inoculation; the back was arched, the skin tight and wrinkled and the appetite poor. These signs continued and the animal got gradually worse; towards the end the respirations were visibly increased.

The temperature rose on the 9th day to 39·5° C.; with this exception the temperature was practically normal throughout.

Weights.

During the first three weeks there was a loss in weight of 2 lbs. then the weight steadily increased.

January 17, 1905	-	-	- 1 qr. 16 lbs.
February 7, 1905	-	-	- 1 qr. 14 lbs.
February 21, 1905	-	-	- 1 qr. 20 lbs.
March 7, 1905	-	-	- 1 qr. 24 lbs.
March 14, 1905	-	-	- 2 qrs. 0 lb.
March 28, 1905	-	-	- 2 qrs. 7 lbs.

Gain in weight, 19 lbs.

Post-Mortem Examination.

Carcass in very poor condition.

The Local Lesion was situated in the muscles on the right side of the abdomen a little anterior to the umbilicus; it was small and flat, measuring 1 in. by ¾ in. by ¼ in. and consisted of tough, waxy white caseous tissue interspersed with fine opaque white streaks of beginning calcification; the skin was slightly adherent at one point and was not infiltrated.

Superficial Inguinal Glands.—On the right side the one nearest the tumour was slightly enlarged and showed irregular caseous patches replacing about half the substance; other glands in this group appeared normal.

The glands on the left side were congested but otherwise normal.

Right Precrural Gland.—Normal.

Left Precrural contained a yellow caseous tubercle.

Iliac and Lumbar Glands were slightly congested; they showed no naked-eye evidence of tuberculosis.

Abdomen.

Omentum and Peritoneum.—Normal.

The Spleen was normal in size; it contained in the pulp from twenty to twenty-four greyish white nodules ranging in size from a pin's head to a pea; the larger nodules had opaque yellow centres.

The Liver, slightly adherent to diaphragm, showed in the substance sparsely scattered grey tubercles the largest the size of a millet seed; the latter were slightly opaque in the centre.

Kidneys and Suprarenal Bodies.—Normal.

Thorax.

Heart.—Normal.

Lungs.—The lungs were deeply congested; the greater part of the middle lobes, portions of the cephalic lobes, and several lobules along the sternal borders of the caudal lobe were dark red, firm, and quite airless; they showed a few small tubercles and on close inspection grey spots were seen. In the crepitant portions of the lung there were moderately numerous yellow tubercles, the size of pins' heads; in the thin margin of the left caudal lobe there were several nodules, the largest the size of a pea, yellow and gritty in the centre. On the surface of the caudal lobes there were several small projecting nodules and numerous long strands of connective tissue, some of which grew from the surface of the nodules; the fringes around the margins of the caudal lobes were hypertrophied, congested, and contained, chiefly on the left side, firm caseating nodules, the largest the size of a pea.

The Bronchial and Mediastinal Glands were deeply congested, but were not obviously tuberculous.

Pleura.—In several places along the margins of the ribs the lymphatic fringes were slightly hypertrophied and contained little grey granules; here and there a slightly larger nodule was seen similar to those on the diaphragm.

Diaphragm.—The pleural surface showed numerous hard grey tubercles, the majority the size of millet seeds; these were situated at the points of attachment of congested connective tissue processes; the fringes around the margins of the tendon were considerably hypertrophied, and showed here and there caseous nodules, the largest the size of a split pea; some of these nodules were hæmorrhagic and were attached to the free extremity of a connective tissue strand.

On the Peritoneal Surface of the diaphragm there were numerous hard grey nodules, the largest the size of a wheat grain.

Tongue, Tonsils, and Pharynx.—Normal.

Submaxillary, Pharyngeal, and Cervical Glands.—Normal.

Prescapular, Prepectoral, and Popliteal Glands.—Normal.

Large and Small Intestines.—Normal.

Mesenteric Glands.—A gland in the anterior part of the mesentery contained a cheesy, gritty nodule, the size of a pea; in the terminal mesenteric gland there was a similar but much smaller nodule.

Microscopical Examination.

Left Precrural Gland.—Three tubercle bacilli seen.

Mesenteric Gland.—No tubercle bacilli.

Bronchial Gland.—No tubercle bacilli.

PIG 96.

Subcutaneous Inoculation of culture from Pig 16.

Dose.—0·1 milligramme.

Date.—January 10, 1905.

Weight.—January 17, 1905. 1 qr. 23 lbs.

Killed.—September 7, 1905. 240 days after inoculation.

Clinical History.

The pig was inoculated subcutaneously in the wall of the abdomen on January 10, 1905.

Three weeks after inoculation a small nodule was felt under the skin of the abdomen and the superficial inguinal glands were slightly enlarged. The tumour and glands increased a little in size and then diminished.

The animal was in fair condition when killed, and, beyond the fact that it was somewhat stunted in its growth, had not shown any signs of illness during the experiment.

The temperature was slightly raised on the 23rd and 24th days (maximum 39·6° C.), and again on the 67th day to 39·2°C.; otherwise the temperature was normal throughout.

Weights.

In the first ten weeks there was a gain in weight of 26 lbs.; in the 12th week there was a loss of 10 lbs., and for three weeks subsequently the weight showed slight fluctuations; during the rest of the experiment there was a slight, almost steady increase in weight.

January 17th, 1905 -	- 1 qr. 23 lbs.
September 7th, 1905 -	- 3 qrs. 19 lbs.
Gain in weight - -	- 1 qr. 24 lbs.
Rate of gain per week -	- 1·56 lbs.

Post-mortem Examination.

Local Lesion.—In the subcutaneous tissues to the right of the umbilicus, there was a softened, caseous, slightly gritty nodule, the size of a kidney bean; the skin was not adherent.

Superficial Inguinal Glands.—On the right side there were two moderately enlarged glands, which were caseous and softened practically throughout; the capsules of the glands were thickened; gritty particles could be detected in the caseo-pus when rubbed between the finger and thumb; in this group were also a pea-sized caseous gland, and two normal glands. On the left side one gland contained a nodule about 1 cm. in diameter with fibrous wall and caseo-purulent contents; a second gland showed on section a number of discrete yellow caseous nodules (largest size of wheat grain) embedded in fibroid glandular tissue; other glands on this side were normal.

Precrural Glands.—Normal.

Iliac Glands.—Normal.

Abdomen.

Omentum and Peritoneum.—Normal.

The Spleen contained three millet seed sized tubercles, with soft yellow centres, in one case gritty, and translucent fibrous margins.

Liver.—Embedded in the substance of the liver just under the capsule a moderate number of nodules was seen (about four dozen counted on the anterior surface, and three dozen on the posterior); the nodules ranged in size from about 1·5 up to 7 and 8 mm. in diameter; all, with the exception of one or two of the smallest, which were grey and translucent throughout, had fibrous capsules and soft viscid caseous contents; in the latter fine gritty particles could be detected; on section similar nodules were seen in the depth of the liver.

Portal Glands.—In the small omentum there was a group of four enlarged glands, the capsules of which were adherent; on section the gland tissue was completely replaced by soft, cheesy, slightly gritty, caseous material; the capsules of the glands were much thickened.

Cœliac Glands were moderately enlarged and contained discrete yellow caseous nodules the size of wheat grains; two others contained similar nodules, but these in one or two places had run together to form soft caseous collections.

Lumbar Glands.—A gland on the left side of a lumbar vertebra showed about half its substance caseous and softened.

Kidneys and Suprarenal Bodies.—Normal.

Thorax.

Heart and Pleura.—Normal.

The Lungs contained sparsely scattered hard tubercles, the largest the size of a wheat grain; the tubercles were translucent and fibrous, and most had opaque yellow centres, which in several were quite calcareous; the tubercles were most numerous in the right cephalic and middle lobes, and many of these were translucent throughout; the lungs were crepitant and there were no areas of consolidation; in the fringes around the margins of the right caudal lobe were several tubercles.

The Bronchial Glands on each side were moderately enlarged; on section their capsules were thickened and their substance almost completely replaced by soft, cheesy, slightly gritty, caseous material.

The Dorsal Mediastinal Glands were normal.

Ventral Mediastinal Glands.—One, the size of a Barcelona nut, was caseous throughout; another similar in size showed discrete yellow caseous nodules, each about 2 mm. in diameter; the gland tissue between the nodules showed an increase in interstitial connective tissue.

A small gland just within the entrance to the thorax contained two yellow millet seed sized tubercles.

Diaphragm.—On the left side of the diaphragm projecting under the peritoneum were three or four hard translucent nodules, the largest the size of a small kidney bean; the latter had a soft, yellow centre.

On the Serous Surface of the stomach near the œsophagus there were about half-a dozen nodules, the largest the size of a wheat grain; one had a soft caseous centre the others were translucent throughout.

A gland near the angle of the jaw on the left side contained a pea-sized firm caseous nodule.

The right Pharyngeal Gland contained a small collection of viscid caseo-pus.

Other Glands in the Neck were normal.

Intestines.—Normal.

Mesenteric Glands.—Two glands in the anterior part of the mesentery contained each a pea-sized softened caseous nodule; a gland towards the root of the mesentery showed two pinhead-sized caseous foci.

Colic Glands.—Normal.

Prescapular and Prepectoral Glands.—Normal.

Testicles.—Normal.

Microscopical Examination (Smear Preparations).

Portal Gland (Emulsion for G.P.).—No tubercle bacilli.

Bronchial Gland (Emulsion for G.P.).—No tubercle bacilli.

Animals Inoculated,
With Tissue Emulsions.

Guinea-pig 1679, inoculated intraperitoneally with an emulsion of a portal gland, died of general tuberculosis in 38 days.

Guinea-pig 1680, inoculated intraperitoneally with an emulsion from a bronchial gland, died of general tuberculosis in 33 days.

With Culture.

Rabbits.

Number of Rabbit.	Initial Weight in grammes.	Final Weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
558	1,620	1,090	1·0 mg.	Subcutaneous	Died, 81 days	General tuberculosis.
559	1,510	1,140	0·1 mg.	Subcutaneous	Died, 137 days	General tuberculosis.
560	1,310	1,000	0·01 mg.	Subcutaneous	Died, 106 days	General tuberculosis.
561	1,220	1,100	0·001 mg.	Subcutaneous partly intra-peritoneal.	Died, 50 days	General tuberculosis.

CALF 230.

Subcutaneous Inoculation of culture from Pig 16.

Dose.—50 milligrammes.

Date.—January 10, 1905.

Weight.—January 17, 1905. 3 qrs. 3 lbs.

Died.—February 13, 1905. 34 days after inoculation.

Clinical History.

The calf was inoculated subcutaneously on the left side of the neck on January 10, 1905.

Two days later a small swelling was felt; this rapidly increased in size, and on January 30, 20 days after inoculation, attained its maximum, measuring 6 in. in length, 3 in. in breadth, and about ¾ in. in greatest thickness; on this date the left prescapular gland measured 3¼ in. in long diameter and a prepectoral gland was the size of a walnut. The skin over the upper part of the tumour then broke down, discharging serous fluid.

On February 6 the respirations were noticed to be increased and the animal looked ill and thinner.

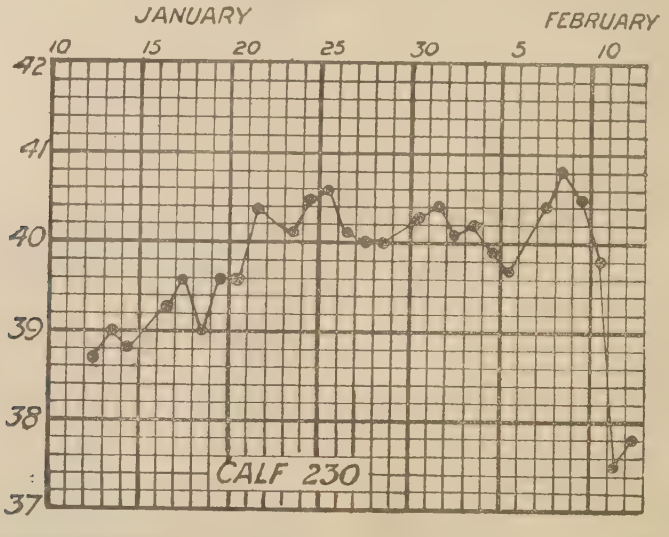
The symptoms rapidly increased in severity; the respirations became quicker, and finally laboured, and the animal visibly lost flesh. Death took place on February 13, 1905, 34 days after inoculation.

Weights.

There was a steady loss in weight.

January 17, 1905	- -	3 qrs. 3 lbs.
February 7, 1905	- -	3 qrs. 0 lb.
February 13, 1905	- -	2 qrs. 18 lbs.
Loss in Weight	- -	13 lbs.
Rate of Loss per Week	-	3·36 lbs.

Temperature



Post-Mortem Examination.

Carcass.—Emaciated.

Seat of Inoculation.—At the seat of inoculation, on the left side of the neck, there was a flat elongated tumour 6 in. by 2¾ in. by 1 in. On section it consisted of dense yellow caseo-necrotic tissue, adherent to the skin, and slightly infiltrating the muscles; in the upper part of the tumour there was an empty cavity 2 in. by 1½ in., whose walls were formed by breaking down caseated tissue; the skin over the cavity was infiltrated up to the surface, and in the central part ulcerated and necrosed.

The tissues around the tumour were slightly œdematous.

Left Prescapular Gland.—Weighed about 2 ozs., and measured 3 in. by 1½ in. by 1 in. On section it was composed throughout of dense pinkish yellow caseated tissue.

Left Prepectoral Glands.—The rounded prepectoral gland was three quarters of an inch in diameter, and on section resembled the prescapular. The kidney-shaped prepectoral and a small gland near it were œdematous, but were otherwise unaltered to the naked eye.

Cervical Glands.—A mid cervical gland on the left side showed the cortex partly caseous. All other cervical glands contained discrete caseous tubercles.

Abdomen.

Omentum.—Was quite devoid of fat. On the under surface there were a few pinhead-sized tubercles.

Spleen.—8 ozs. Normal on the surface; substance packed with firm yellow tubercles, ranging in size from 1·5 millimetres to about 3 millimetres in diameter.

Liver.—Weighed 3 lbs., and was not apparently enlarged. The substance was evenly peppered with grey tubercles, ranging in size from a mere point to that of a pin's head; the larger tubercles were slightly opaque in the centre.

Gall Bladder.—There were two or three yellow tubercles under the mucous membrane.

Portal Glands.—Were moderately enlarged. They showed in their cortices firm grey tissue, closely studded with irregular caseous foci.

Pancreas.—In the substance there was a minute yellow point.

Kidneys.—Right kidney showed in the cortex moderately numerous opaque grey miliary tubercles, some of which on section were seen to be slightly elongated in a radial direction. Left kidney similarly affected. No tubercles were seen in the medullary zones.

Suprarenal Bodies.—In the cortex of each suprarenal there was a greyish white focus.

Coeliac Glands.—Were enlarged, and resembled on section the portal.

Thorax.

Heart.—Muscle pale and flabby; cavities filled with ante-mortem blood clot; on the endocardium of the right ventricle there was a mustard seed-sized pearly tubercle.

Parietal Pleura and Diaphragm.—Normal.

Lungs.—Were moderately closely beset with opaque miliary tubercles; the right, middle and caudal lobes were solid, dark red in colour, and sank in water (animal found dead on right side); the right cephalic and the left lobes showed patches of consolidation, mainly small, which surrounded small groups of tubercles.

Mediastinal Glands.—The dorsal mediastinal glands showed a moderate degree of enlargement. On section they showed in the cortex firm grey tissue in a state of early caseation (they resembled the portal glands); the medullæ were congested.

Bronchial Glands, Peribronchial Glands.—Were enlarged, and on section resembled the mediastinal.

The gland below the trachea was enlarged, congested and œdematous; no foci of caseation seen.

The ventral mediastinal glands each showed one or two caseous tubercles.

Alimentary Tract.

Tonsils.—Contained a few caseous tubercles. On the superior surface of the soft palate there were two or three small ulcerated caseous nodules.

Small Intestine.—Under the mucous membrane of the small intestine there were sparsely scattered caseous tubercles; these were most numerous in the ileum.

Large Intestine.—Normal.

Gastric Glands.—Were enlarged, and composed practically throughout of grey translucent tissue, studded with minute opaque foci.

Mesenteric Glands.—The glands in the anterior three-fourths of the mesentery showed here and there opaque miliary tubercles; those in the posterior quarter were more severely affected.

Various Lymphatic Glands.

Colic Glands.—Several contained miliary tubercles.

Submaxillary Glands, Pharyngeal Glands were closely beset with caseous tubercles.

Gluteal Glands, Popliteal Glands, Pudic Glands, Precrural Glands, Iliac Glands, Sacro-Iliac Glands, Right Prescapular Gland, Right Axillary Gland.—Contained a moderate number of caseous tubercles.

Right Prepectoral Glands.—One right prepectoral gland showed a caseous focus.

Lumbar Glands.—Showed numerous caseous foci.

Ischiatic Glands.—Appeared normal.

Testicles, Eyes.—Normal.

Microscopical Examination.

Tonsils.—Tubercle bacilli moderateiy numerous.

Soft Palate, *Pancreas*, *Gall Bladder*—Tubercle bacilli numerous.

Heart.—One tubercle bacillus.

Right Suprarenal Body, *Left Suprarenal Body*—A few tubercle bacilli in each.

Tables showing the Virulence for

RABBITS

of Cultures derived from the different Animals in the Report on Modification Experiments.

Strains of Virus B. I.

Rabbits Inoculated with Culture derived from Monkey 76.

The culture used was the 5th generation, 20 days old, and had been in cultivation 91 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
189	3,560	1,920	0·1 mg.	Intravenous	24 days (died)	Lungs packed with grey miliary tubercles with yellow centres; numerous tubercles in the kidneys and numerous minute grey foci in the liver and spleen; latter organ much enlarged; all the lymphatic glands contained caseous foci.
192	2,930	1,870	1·0 mg.	Intravenous	16 days (died)	General tuberculosis as in Rabbit 189, but tubercles more minute.
190	3,050	1,970	1·0 mg.	Intraperitoneal	23 days (died)	Omentum retracted, slightly thickened and in a state of early caseation. Parietal and visceral peritoneum uniformly and closely covered with small opaque tubercles. Liver and spleen closely beset with minute tubercles; kidneys showed fairly numerous tubercles; lungs packed with miliary caseating tubercles; all the lymphatic glands were tuberculous, the sternal glands being caseous throughout
191	2 830	1,600	1·0 mg.	Intraperitoneal	32 days (died)	General tuberculosis as in Rabbit 190, but tubercles a little larger.
193	3,580	2,400	1·0 mg.	Subcutaneous	47 days (died)	Caseous tumour between scapulæ; skin over it ulcerated; neighbouring glands caseous; lungs crowded with caseating nodules; cephalic lobes solid; numerous tubercles in liver and kidneys; spleen greatly swollen, packed with caseous nodules, up to 5 mm. in diameter; most of the lymphatic glands were affected.
194	2,960	2,360	1·0 mg.	Subcutaneous	63 days (died)	General tuberculosis; the local lesion was situated between the scapulæ and was large, firm and caseous.

Rabbits Inoculated with Culture derived from Monkey 80.

The subculture used was the 6th generation, 20 days old, and had been in cultivation 177 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
216	920		0·1 mg.	Intravenous	8 days (died)	Cause of death not ascertained.
217	1,370	970	0·1 mg.	Intraperitoneal	37 days (died)	General tuberculosis; lungs packed with confluent caseating nodules.
218	1,220	920	1·0 mg.	Intraperitoneal	47 days (died)	General tuberculosis; tubercles were fairly numerous on the mesocolon, but sparsely scattered; on the parietal peritoneum; there was a bean-sized caseous nodule in the abdominal wall (the inoculation was partly subcutaneous).
219	1,100	980	1·0 mg.	Subcutaneous	42 days (died)	Caseous abscess between the scapulæ; general tuberculosis, lungs severely affected.
220	1,000	700	1·0 mg.	Subcutaneous.	49 days (died)	Firm caseous tumour breaking down in the centre, on left scapula; neighbouring glands caseous; lungs enlarged and composed almost entirely of caseating nodules; numerous tubercles in spleen and kidneys; liver packed with firm grey nodules; there were a few minute tubercles in the omentum and on the mesentery.

RABBITS INOCULATED WITH CULTURE DERIVED FROM MONKEY 82.

The subculture used was the 5th generation, 20 days old, and had been in cultivation 169 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose	Method of Inoculation.	Duration of Life.	Result.
221	1,050	850	0·1 mg.	Intravenous	26 days (died)	Lungs packed with miliary tubercles; numerous tubercles in kidneys and numerous very minute grey foci in liver and spleen.
222	1,320	950	0·1 mg.	Intraperitoneal	46 days (died)	Omentum thickened and caseating; sparsely scattered caseous tubercles on parietal peritoneum; very numerous tubercles on mesentery and meso-colon; a few minute tubercles in liver and spleen and a few miliary caseating tubercles in each kidney; lungs almost completely consolidated, being filled with confluent caseating nodules.
223	1,150	1,170	1·0 mg.	Intraperitoneal	91 days (died)	Omentum thickened and composed of lobulated masses of firm caseating tissue, the margins of which were darkly pigmented. Yellow caseous tubercles with pigmented margins were sparsely scattered over the surface of the parietal and visceral peritoneum. The spleen and liver showed a few flattened tubercles on the surfaces: none were seen in the depth. The kidneys were closely beset with caseating nodules. The lung tissue was extensively replaced by irregular caseating masses, in some cases softened. Sternal glands caseous throughout; the bronchial showed caseous foci. The diaphragm was thickened and caseating and there were some flattened growths on the parietal pleura.
224	1,200	870	10·0 mg.	Intraperitoneal	21 days (died)	General tuberculosis; tubercles very numerous on the peritoneum and in all the organs.
225	1,250	1,910	1·0 mg.	Subcutaneous	114 days (died)	Caseous ulcer between the shoulders; neighbouring glands large and caseous; moderate number of tubercles in omentum; lungs severely affected, consisting almost throughout of caseating tuberculous masses; a few miliary tubercles in each kidney; no tubercles in liver or spleen. The abdominal and thoracic lymphatic glands appeared normal.

Rabbits Inoculated with Culture derived from Baboon 64.

The subculture used was the 4th generation, 22 days old and had been in cultivation 99 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
857	1,760	1,200	0·1 mg.	Intravenous	21 days (died)	Lungs closely beset with grey miliary tubercles caseous in the centre; thoracic glands enlarged and oedematous, showed patches of caseations in the cortices; spleen greatly enlarged and swollen, pulp speckled with grey foci; liver closely beset with grey foci; numerous small grey tubercles in each kidney; most of the lymphatic glands contained caseous foci and there were minute grey points in the heart muscle.
858	1,630	1,070	0·1 mg.	Intraperitoneal	15 days (died)	Omentum thickened and caseating: parietal and visceral peritoneum covered with minute grey tubercles; lungs contained numerous grey tubercles with slightly opaque centres; spleen slightly enlarged; liver contained psorosperm nodules and minute grey points; a few minute tubercles just visible in the kidneys.
859	1,060	880	0·1 mg.	Intraperitoneal	16 days (died)	General tuberculosis resembling that in Rabbit 858; lungs were packed with grey tubercles, with opaque centres, which caused almost complete consolidation of all the lobes.
860	1,000	850	1·0 mg.	Subcutaneous	91 days) (killed, dying)	General tuberculosis.

Rabbits Inoculated with Culture derived from Pig 16.

The culture used was the 10th generation 20 days old, and had been in cultivation 359 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
159	1,700	1,200	1·0 mg.	Intravenous	21 days (died)	Lungs closely beset with miliary tubercles just beginning to caseate; bronchial glands showed caseous nodules; the liver and a greatly swollen spleen contained innumerable minute grey foci; there were moderately numerous early tubercles in each kidney.
158	1,000	1,070	1·0 mg.	Intraperitoneal	62 days (died)	General tuberculosis; there was a large caseous nodule in the muscle wall: inoculation probably partly intra-muscular.

Rabbits Inoculated with Culture derived from Pig 96.

The culture used was the 4th generation, 22 days old, and had been in cultivation 88 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in gam mes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
558	1,620	1,090	1·0 mg.	Subcutaneous	81 days (died)	Caseous ulcer ; widespread generalised tuberculosis ; lungs filled with caseating nodules.
559	1,510	1,140	0·1 mg.	Subcutaneous	137 days (died)	Caseous ulcer ; inguinal and axillary glands enlarged and caseous ; lungs enlarged and extensively tuberculous ; kidneys contained numerous nodules up to 12 mm. ; spleen normal ; there were two doubtful tubercles in the liver : many lymphatic glands were tuberculous ; the left lobe of the thyroid was almost entirely caseous, and there was a caseous nodule in that of the right.
560	1,310	1,000	0·01 mg.	Subcutaneous	106 days (died)	Caseous ulcer ; general tuberculosis ; the lungs were crepitant and contained only a moderate number of discrete nodules ; there was one tubercle in the spleen and a doubtful tubercle in the liver.
561	1,220	1,100	0·001 mg.	Subcutaneous	50 days (died)	Widespread generalised tuberculosis ; the omentum was thickened and caseating, and there were scattered tubercles on the peritoneum ; the inoculation was therefore partly intraperitoneal.

Rabbits Inoculated with Culture derived from the Spleen of Baboon 66.

The culture used was the 4th generation, 20 days old ; the total age of the culture was 149 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
984	1,580	1,180	1·0 mg.	Intravenous	Died, 14 days	Miliary tuberculosis of lungs ; numerous minute tubercles in spleen (very large), liver and kidneys.
985	1,410	1,090	0·1 mg.	Intravenous.	Died, 26 days	General tuberculosis ; tubercles very numerous in all the organs.
987	1,210	870	1·0 mg.	Intraperitoneal	Died, 12 days	General tuberculosis ; omentum thickened and caseating ; parietal and visceral peritoneum closely covered with minute tubercles ; numerous minute grey foci in liver and spleen ; a few in each kidney ; moderate number of minute tubercles in the lungs ; tuberculosis of abdominal and thoracic lymphatic glands.
986	1,030	—	0·1 mg.	Intraperitoneal	Died, 1 day	Severe coccidiosis of liver.

Strain of Virus B. II.

Rabbits Inoculated with Culture derived from Guinea-pig 1,735, the last of a Series of 24 Guinea-pigs, the first of which was Inoculated with the Original Material of Virus B. II.

The Culture used was the 9th generation, 12 days old ; the total age of the Culture was 300 days.

Number of Rabbit	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
1,027	1,870	1420	1·0 mg.	Intravenous	Died, 15 days	General tuberculosis.
1,024	1,020	950	0·1 mg.	Intravenous	Died, 18 days	General tuberculosis.
1,025	1,020	920	0·1 mg.	Intravenous	Died, 18 days	General tuberculosis.
1,026	1,150	820	1·0 mg.	Intraperitoneal	Died, 12 days	General tuberculosis ; tubercles everywhere numerous and very minute.
1,028	960	970	0·1 mg.	Intraperitoneal	Died, 29 days	General tuberculosis: numerous tubercles on peritoneum and mesentery ; liver and spleen packed with grey foci ; caseous tubercles in many of the lymphatic glands.

Strains of Virus B IV.

Rabbits Inoculated with Culture derived from Chimpanzee 4,

The culture inoculated was the third generation, 19 days old ; the total age of the culture was 46 days.

Number of Rabbit.	Initial Weight in Grammes.	Final Weight in Grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
166	2,150	1,460	0·1 mg.	Intravenous	28 days (died)	General tuberculosis ; lungs closely packed with miliary tubercles with caseous centres ; liver and spleen closely beset with minute tubercles; each kidney contained fairly numerous miliary tubercles ; many of the lymphatic glands showed minute caseous foci.
167	2,320	1,750	0·1 mg.	Intraperitoneal	39 days (died)	General tuberculosis; the peritoneum was covered with tubercles, and there were numerous tubercles in the lungs, spleen, and liver ; the kidneys contained only a few.
168	2,400	1,950	1·0 mg.	Intraperitoneal	61 days (died)	General tuberculosis ; there was a small caseous nodule in the subcutaneous tissues, and a larger one in the muscles of the abdominal wall.
169	2,660	1,890	1·0 mg.	Intraperitoneal	24 days (died)	General tuberculosis ; tubercles were very numerous on the peritoneum and in the lungs ; the spleen and liver were peppered with minute grey foci, and there were fairly numerous tubercles in each kidney: the sternal glands were caseous throughout ; the bronchial glands contained caseous tubercles.
170	2,250	1,550	1·0 mg.	Subcutaneous	74 days (died)	General tuberculosis ; lungs severely affected.
171	2,000	1,640	1·0 mg.	Subcutaneous	94 days (died)	Local caseous ulcer ; adjacent gland caseous ; lungs enlarged and composed practically throughout of caseating nodules ; there were very numerous nodules in the kidneys and none in the liver or spleen ; several lymphatic glands contained caseous tubercles.

RABBITS INOCULATED WITH CULTURE DERIVED FROM CHIMPANZEE .

The culture inoculated was the 3rd generation, 21 days old ; the total age of the culture was 49 days.

Number of Rabbit.	Initial Weight in Grammes.	Final Weight in Grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
240	1,360	1,040	0·1 mg.	Intravenous	18 days (died)	Lungs, œdematous and congested, packed with miliary tubercles ; spleen (enlarged) and liver speckled with minute grey foci, numerous miliary caseating tubercles in each kidney. There were a few tubercles on the endocardium of the right ventricle. The thoracic glands showed caseous areas in the cortices ; the iliac and lumber glands caseous foci ; numerous tubercle bacilli were found in the marrow.
242	840	720	0·1 mg.	Intraperitoneal	31 days (died)	General tuberculosis ; all the organs were affected, the lungs and kidneys severely.
239	1,620	1,140	1·0 mg.	Intraperitoneal	20 days (died)	General tuberculosis ; there were very numerous tubercles on the peritoneum (parietal and visceral) and in all the organs.
244	680	570	1·0 mg.	Intraperitoneal	19 days (died)	General tuberculosis ; similar to that in Rabbit 239.
241	1,260	1,070	1·0 mg.	Subcutaneous	77 days (died)	General tuberculosis ; the local lesion was a large baggy cyst, situated over the left scapula, containing blood-stained serous fluid and large caseo-necrotic masses ; the lungs were enlarged and extensively tuberculous.
243	730	910	1·0 mg.	Subcutaneous	62 days (died)	Local caseous ulcer ; inguinal and axillary glands much enlarged and extensively caseous ; tuberculosis of all the organs and most of the lymphatic glands ; the lungs were almost completely consolidated.

RABBITS INOCULATED WITH CULTURE DERIVED FROM CHIMPANZEE 8.

The culture inoculated was the 6th generation, 20 days old ; the total age of the culture was 84 days.

Number of Rabbit.	Initial Weight in Grammes.	Final Weight in Grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
320	1,210	1,000	0·1 mg.	Intravenous	16 days (died)	General tuberculosis.
321	1,190	1,100	1·0 mg.	Intraperitoneal	14 days (died)	General tuberculosis (uncomplicated); there were very numerous minute tubercles on the peritoneum and in all the organs. Tubercle bacilli were found in the bone marrow.
323	970	850	0·1 mg.	Intraperitoneal	5 days (died)	No sign of disease.
324	560	470	0·1 mg.	Subcutaneous	64 days (died)	Caseous ulcer ; adjacent glands caseous ; the lungs contained numerous caseating tubercles and irregular caseous patches ; there were sparsely scattered tubercles in the kidneys ; no tubercles were seen in the spleen or liver.
322	1,030	860	1·0 mg.	Subcutaneous	53 days (died)	General tuberculosis.
325	850	670	1·0 mg.	Subcutaneous	8 days (died)	Caseous patch in subcutaneous tissues of abdominal wall ; inguinal slightly enlarged and congested ; no sign of disease elsewhere. Microscopical. — Tubercle bacilli numerous in an inguinal gland ; none found in the spleen.

RABBITS INOCULATED WITH CULTURE DERIVED FROM CHIMPANZEE 10.

The culture inoculated was the 3rd generation, 21 days old ; the total age of the culture was 60 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
658	1,500	1,240	0·1 mg.	Intravenous	20 days (died)	Lungs packed with miliary tubercles ; liver and spleen peppered with minute grey points ; kidneys contained numerous submiliary tubercles ; there were numerous tubercles on the surface of the left ventricle and endocardium of the right.
660	1,780	1,370	1·0 mg.	Intraperitoneal	18 days (died)	General tuberculosis ; the tubercles were very numerous on the peritoneum and in all the organs.
662	1,520	1,290	0·1 mg.	Intraperitoneal	32 days (died)	General tuberculosis ; the lungs were closely beset with caseating miliary tubercles which caused almost complete consolidation of the anterior lobes ; the tubercles on the peritoneum and on the liver and spleen were not so numerous as in Rabbit 660.
664	1,570	1,240	0·01 mg.	Intraperitoneal	41 days (died)	General tuberculosis ; the lungs were extensively consolidated and composed practically throughout of greyish red translucent tissue containing irregular yellow caseous foci ; there were fairly numerous miliary tubercles in all the other organs and most of the lymphatic glands were tuberculous.
659	1,470	1,090	1·0 mg.	Subcutaneous	96 days (died)	Local ulcer ; tuberculosis of all the lymphatic glands and all the organs, except the spleen ; the lungs were severely affected and were almost quite solid. Each lobe of the thyroid contained an opaque tubercle.
661	1,430	1,430	0·1 mg.	Subcutaneous	85 days (died)	Large local ulcer ; nearest glands caseous ; lungs and kidneys closely beset with caseous nodules ; the liver appeared normal ; the spleen contained one tubercle ; most of the lymphatic glands were tuberculous ; the submaxilliary salivary gland contained a yellow tubercle and there was one tubercle in each lobe of the thyroid.
663	1,590	1,300	0·01 mg.	Subcutaneous	100 days (died)	General tuberculosis similar to that in Rabbit 659.

RABBITS INOCULATED WITH CULTURE DERIVED FROM CHIMPANZEE 16.

The culture inoculated was the 5th generation, 21 days old; the total age of the culture was 89 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
948	1,120	940	1·0 mg.	Intravenous	14 days (died)	General tuberculosis of the usual type for this mode of inoculation.
949	1,120	850	0·1 mg.	Intravenous	16 days (died)	
950	1,200		1·0 mg.	Intraperitoneal	9 days (died)	Slight tuberculosis of omentum and peritoneum and ventral mediastinal glands.
951	1,020	890	0·1 mg.	Intraperitoneal	42 days (died)	General tuberculosis; numerous tubercles in liver and spleen; the lungs were closely filled with grey tubercles, caseous in the centre; the cephalic lobes were quite airless.
954	1,570	1,220	·001 mg.	Intraperitoneal	54 days (killed)	Omentum thickened and considerably caseated; the spleen and liver contained moderately numerous submiliary tubercles; a few tubercles up to a hemp seed in each kidney; the lungs were large and filled with irregular translucent nodules with caseous foci.
955	1,200	820	·001 mg.	Intraperitoneal	45 days (died)	Omentum greatly thickened; scattered nodules up to a hemp seed on the visceral and parietal peritoneum; spleen showed moderately numerous yellow miliary tubercles, the liver a few; scattered grey tubercles with caseous centres in the kidneys; the lungs did not collapse and were fairly closely filled with irregular caseous areas. Mediastinal glands enlarged and caseous.
952	1,050	1,270	1·0 mg.	Subcutaneous	54 days (killed)	Softening caseous tumour the size of a pigeon's egg; three yellow miliary tubercles in the spleen; liver normal; kidneys showed nodules up to a small pea in size with little central caseation; the lungs were congested and contained numerous discrete tubercles with caseous centres; caseous foci in mediastinal, bronchial, and left iliac gland.
953	1,020	1,500	0·1 mg.	Subcutaneous	54 days (killed)	Shallow ulcer, 1¼in. in diameter, covered with a caseous scab; left axillary and inguinal glands partly caseous; spleen and kidneys each showed a single tubercle; liver normal; lungs are congested and show sparsely scattered translucent tubercles up to a millet seed.

Rabbits Inoculated with Culture derived from the Lung of Baboon 62.

The culture used was the 5th generation, 20 days old ; the total age of the culture was 91 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Methods of Inoculation.	Duration of Life.	Post-mortem Result.
874	2,100	1,630	0·1 mg.	Intravenous	Died, 19 days	General tuberculosis of the usual type.
873	1,850	1,360	0·1 mg.	Intraperitoneal	Died, 10 days	Omentum thickened ; fairly numerous tubercles on peritoneum ; a few minute grey tubercles in spleen and the kidneys ; moderately numerous tubercles in liver and lungs ; ventral mediastinal glands partly caseous.
872	1,500	1,950	1·0 mg.	Subcutaneous	Killed, 108 days	Small clean ulcer in skin, soft caseous nodules in nearest glands ; spleen, liver and left kidney normal, right kidney showed two grey tubercles with caseous centres ; lungs crepitant but slightly congested, contained scattered superficial nodules up to 4 mm. in diameter, soft and caseous in the centre, also a nodule 1 in. by 1½ in. filled with caseo-pus ; other lymphatic glands normal.

Rabbits inoculated with Culture derived from the Lung of Dog 92.

The culture used was the 4th generation, 22 days old ; the total age of the culture was 113 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
1009	1,400	990	1·0 mg.	Intravenous	Died, 14 days	General tuberculosis of the usual acute type.
1010	1,200	750	0·1 mg.	Intravenous	Died, 21 days	Ditto.
1013	1,250	620	1·0 mg.	Intraperitoneal	Died, 20 days	General tuberculosis ; the lungs were crepitant and contained only a moderate number of small tubercles
1011	1,200	850	0·1 mg.	Intraperitoneal	Died, 26 days	General tuberculosis ; tubercles were numerous in all the organs.
1012	1,200	1,540	1·0 mg.	Subcutaneous	Killed, 40 days	Soft caseous subcutaneous tumour size of pigeon's egg ; curdy caseous nodules in nearest glands ; one tubercle in the spleen and a few in each kidney ; a few scattered tubercles in the lungs.

RABBITS INOCULATED WITH CULTURE DERIVED FROM DOG 18.

The culture used for this series was the 9th generation, 20 days ; the total age of the culture was 249 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
410	1170	1730	0·1 mg.	Intravenous	97 days (killed)	The lungs were pinkish and collapsed normally; they showed evenly distributed throughout a moderate number of firm grey miliary tubercles with opaque centres. The left kidney showed in the depth of the cortex two grey miliary tubercles, slightly opaque in the centre. In the right kidney there was a similar tubercle and one of the calyces was filled with curdy caseo-pus. Other organs and all lymphatic glands were normal.
409	1530	1570	1·0 mg.	Intraperitoneal	97 days (killed)	The omentum contained scattered softened yellow nodules, up to 3 mm. in diameter and fairly numerous opaque pinhead sized tubercles; there were a few tubercles on the surface of the large intestine, liver and spleen, and a few groups of small tubercles on the mesentery between the vessels. The lungs contained a few grey miliary tubercles, slightly opaque in the centre, occurring chiefly in groups. The right kidney contained two or three minute grey tubercles in the cortex. The liver and spleen were normal on section. All lymphatic glands were normal.
411	1100	980	0·1 mg.	Intraperitoneal	68 days (died)	Omentum, not thickened, showed numerous spherical pearly white tubercles, smaller than pins' heads. There was a small number of similar tubercles on the mesentery, meso-colon and serous surfaces of the large intestine ; on the mesentery they formed clusters in the centres of the areas between the vessels. The lungs collapsed normally and contained a few grey tubercles, singly and in small groups, with very little central necrosis. Other organs and all the lymphatic glands were normal. The mucous membrane of the stomach was ulcerated.
408	1500	2170	1·0 mg.	Subcutaneous	97 days (killed)	In the subcutaneous tissues of the abdominal wall there was a thin-walled cyst, the size of a sparrow's egg, filled with thick creamy caseo-pus. Nearest glands normal. The right caudal lobe showed under the pleuræ a flattened congested nodule 5 mm. in diameter with a yellow caseous centre ; there was besides a pin-head sized grey tubercle opaque in the centre. There was no sign of tuberculosis elsewhere.
412	1020	1730	0·1 mg.	Subcutaneous	97 days (killed)	In the skin of the abdomen there was a small ulcer communicating with a small cavity with fibrous walls and caseo-purulent contents. In the right lobe of the lung there was a small irregular grey nodule containing minute caseous points. Other organs and all lymphatic glands were normal.
890	1520	1620	10·0 mg.	Intraperitoneal	83 days (killed)	Omentum contained translucent miliary tubercles with calcareous centres: a few similar tubercles on peritoneum: spleen and liver normal: three caseous miliary tubercles in the kidneys ; the lungs, pink and crepitant, showed a few translucent foci, two miliary tubercles and a grey marginal nodule with gritty foci ; lymphatic glands normal.

Strains of Virus B. IX.

Rabbits Inoculated with Culture derived from Chimpanzee 14.

The culture inoculated was the 4th generation, 22 days old ; the total age of the culture was 91 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
742	1,470	1,150	1·0 mg.	Subcutaneous	25 days (died)	In the subcutaneous tissues of the abdominal wall there was a caseous tumour the size of a pigeon's egg ; the neighbouring glands were enlarged and caseous ; there were two miliary tubercles in the lung. The pleural cavities contained a quantity of clear fluid, and the pericardial sac was distended with clear fluid containing a fibrinous clot. The left ventricle was hard, and on section showed the internal part of the wall, together with the musculi papillares, yellow and necrotic ; no tubercle bacilli were found.
743	1,270	980	0·1 mg.	Subcutaneous	103 days (died)	The local lesion was a thin-walled cavity, measuring $3 \times 2\frac{1}{4} \times 1\frac{1}{4}$, containing caseo-pus and firm caseous masses ; the nearest glands were enlarged and caseous ; the lungs did not collapse and were extensively tuberculous ; the kidneys contained sparsely scattered tubercles ; the liver and spleen were normal. The left lobe of the thyroid contained a small tubercle.
744	1,450	920	0·01 mg.	Subcutaneous	132 days (died)	The local lesion was a thin-walled cyst filled with caseo-pus ; one inguinal gland was caseous, the other neighbouring glands were normal ; the lungs contained numerous discrete caseous nodules, but were not so severely affected as usual ; the kidneys contained moderately numerous tubercles, the liver two, and the spleen none. Most of the lymphatic glands were normal. Each lobe of the thyroid contained a caseous focus.
745	1,350	920	0·001 mg.	Subcutaneous	151 days (died)	Shallow ulcer in the skin the size of a shilling ; inguinal glands enlarged and caseous throughout ; liver and spleen normal ; the kidneys contained moderately numerous tubercles ; the lungs were large and almost replaced by irregular caseous masses ; caseous masses on the diaphragm ; a nodule in each lobe of the thyroid.

Rabbits Inoculated with Culture derived from Chimpanzee 18.

The culture inoculated was the 2nd generation, 19 days old ; the total age of the culture was 36 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
993	1,540	1,150	1·0 mg.	Intravenous	16 days (died)	General tuberculosis of the usual type.
994	920	760	0·1 mg.	Intravenous	15 days (died)	
996	1,250	800	1·0 mg.	Intraperitoneal	17 days (died)	General tuberculosis.
995	900	540	0·1 mg.	Intraperitoneal	30 days (died)	General tuberculosis.
997	1,050	1,250	1·0 mg.	Subcutaneous	44 days (killed)	Local tumour the size of a hen's egg, caseo-purulent in the centre ; kidneys and spleen showed a few minute tubercles ; liver normal ; the lungs were pink and crepitant, containing a moderate number of grey tubercles with caseous centres up to the size of a millet seed.
998	840	1,150	0·1 mg.	Subcutaneous	44 days (killed)	The local lesion was a small ulcer with a caseous floor ; nearest glands caseous ; spleen contained a moderate number of yellowish tubercles the size of rape seed ; a few tubercles in each kidney ; the lungs were pink and crepitant with scattered translucent tubercles up to a millet seed.

Rabbits Inoculated with Culture derived from Monkey 110.

The Culture used was the 4th generation, 7 days; all the generations had been grown on glycerin media; the total age of the Culture was 55 days.

Number of Rabbit	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
521	1,380	1,010	1·0 mg.	Intraperitoneal	22 days (died)	General tuberculosis; very numerous minute tubercles on the parietal and visceral peritoneum; lungs beset with small tubercles; the spleen was swollen and showed barely visible grey points; foci not definitely visible in the liver; many lymphatic glands showed caseous foci.
522	1,680	1,550	0·01 mg.	Intraperitoneal	37 days (died)	General tuberculosis; the omentum was thickened and caseating; tubercles were moderately numerous on the parietal peritoneum, more numerous on the mesentery and meso-colon; there were tubercles in all the organs, the lungs being most severely affected; almost all the lymphatic glands were tuberculous.

Rabbits Inoculated with Culture derived from Monkey 116.

The Culture used was the 2nd generation, 18 days old; the total age of the Culture was 27 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
691	1,220	1,030	1·0 mg.	Subcutaneous	100 days (died)	Large ulcerated caseous lesion; adjacent glands caseous; the lungs were enlarged, extensively consolidated, and advanced in caseation; the kidneys contained moderately numerous nodules; liver and spleen normal.
692	950	770	0·01 mg.	Subcutaneous	20 days (died)	Softened caseous local lesion; inguinal glands, slightly enlarged, showed caseous foci in the cortices.
693	850	620	0·001 mg.	Subcutaneous	100 days (died)	The tuberculosis in this animal resembled that in Rabbit 691.

Rabbits Inoculated with Culture derived from Lemur 2.

The Culture used was the 6th generation, 23 days old; the total age of the Culture was 96 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
535	1,580	1,300	1·0 mg.	Intravenous.	14 days (died)	The lungs were crepitant and contained numerous, just visible small grey tubercles with caseous, centres. The spleen was much enlarged; (?) grey points in the pulp. Liver apparently infiltrated with extremely numerous minute grey points. No tubercles seen in the kidneys.
536	1,290	1,090	0·1 mg.	Intravenous	21 days (died)	Lungs crepitant, contained scattered miliary caseous tubercles; spleen (greatly enlarged) and liver beset with extremely numerous minute grey points; there were a few tubercles in the kidneys; bronchial glands showed early caseous foci.
537	1,850	1,290	1·0 mg.	Intraperitoneal	18 days (died)	General tuberculosis of the usual type.

Rabbits Inoculated with Culture derived from Baboon 68.

The culture used was the 3rd generation, 23 days old; the total age of the culture was 57 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
688	1,260	780	1·0 mg.	Subcutaneous	77 days (died)	Local ulcer; adjacent glands contained caseous nodules; lungs enlarged, extensively consolidated and caseated; there were moderately numerous nodules in the kidneys; the spleen and liver were normal; there were some caseous nodules in the walls of the intestines. The thoracic and abdominal (except one lumbar and one iliac) glands were normal.
689	1,560	820	0·01 mg.	Subcutaneous	129 days (died)	Small dry ulcer; neighbouring glands caseous; the lungs were crepitant and contained discrete caseous tubercles and nodules; thoracic glands, liver and spleen normal; moderately numerous caseous nodules in the kidneys; numerous flattened caseating growths on the diaphragm and pleura and large firm nodular caseating masses in the dorsal and ventral mediastina.
690	1,470	1,280	0·0001 mg.	Subcutaneous	187 days (killed)	Small shallow ulcer; nearest glands enlarged and caseous; spleen and liver normal; in the kidneys there were scattered caseous nodules up to one inch in diameter; the lungs were closely beset with grey tubercles up to a hemp seed; firm yellow caseous tumours in dorsal mediastinum, on pleura, pericardium, and pleural surface of the diaphragm.

Rabbits Inoculated with Culture derived from Monkey 126.

The culture was the 4th generation, 22 days; the total age of the culture was 64 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
1016	1,000	730	1·0 mg.	Intravenous	15 days (died)	General tuberculosis; lungs filled with small tubercles.
1015	1,300	1,050	0·1 mg.	Intravenous	19 days (died)	General tuberculosis.
1018	1,340	960	0·1 mg.	Intravenous	25 days (died)	General tuberculosis.
1014	850	—	1·0 mg.	Intraperitoneal	5 days (died)	Slight tuberculosis of omentum; no disease elsewhere.
1017	1,070	780	0·1 mg.	Intraperitoneal	28 days (died)	General tuberculosis of the usual type.

Rabbits Inoculated with Culture derived from Monkey 132.

The culture was the 4th generation, 22 days old; the total age of the culture was 50 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
1022	1,080	—	1·0 mg.	Intravenous	5 days (died)	No naked-eye evidence of tuberculosis.
1019	1,240	1,000	0·1 mg.	Intravenous	16 days (died)	General tuberculosis.
1020	1,440	1,150	0·1 mg.	Intravenous	21 days (died)	General tuberculosis.
1021	870	680	1·0 mg.	Intraperitoneal	23 days (died)	General tuberculosis.
1023	1,100	—	0·1 mg.	Intraperitoneal	5 days (died)	No visible disease.

RABBITS INOCULATED WITH CULTURE DERIVED FROM DOG 60.

The culture used was the 3rd generation, 21 days old, and had been in cultivation 64 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
637	2,610	1,860	0·1 mg.	Intravenous	Died, 28 days	General tuberculosis; lungs closely beset with miliary caseating coalescing tubercles.
638	2,280	1,730	0·1 mg.	Intraperitoneal	Died, 25 days	General tuberculosis; peritoneum profusely covered with small tubercles; lungs closely beset with discrete grey tubercles with caseous centres; numerous small tubercles in liver and kidneys; none visible in the spleen.
639	2,900	1,580	1·0 mg.	Intraperitoneal	Died, 28 days	General tuberculosis of the usual type.
641	1,180	900	0·1 mg.	Subcutaneous	Died, 78 days	General tuberculosis; lungs closely filled with caseating nodules; numerous tubercles in the kidneys, one minute tubercle in the spleen, none in the liver; most of the lymphatic glands were affected.
640	2,830	1,590	1·0 mg.	Subcutaneous	Died, 161 days	Softened caseous nodule the size of a hen's egg; nearest glands caseous; lungs extensively tuberculous; the kidneys contained numerous nodules; the liver a few doubtful tubercles; the spleen was normal; there were numerous tuberculous growths in the parietal pleura and the thyroid body; intestines and gall bladder were tuberculous.

RABBITS INOCULATED WITH CULTURE DERIVED FROM THE LIVER OF DOG 84.

The culture used was the 6th generation, 21 days old; the total age of the culture was 123 days.

Number of Rabbit.	Initial Weight in Grammes.	Final Weight in Grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
1003	1,320	950	1·0 mg.	Intravenous	Died, 17 days	Lungs packed with miliary tubercles; numerous minute foci in spleen (enlarged); liver, and kidneys.
1000	1,050	650	0·1 mg.	Intravenous	Died, 23 days	General tuberculosis; lungs packed with miliary tubercles.
1001	1,150	720	1·0 mg.	Intraperitoneal	Died, 21 days	General tuberculosis of the usual type.
1002	950	680	0·1 mg.	Intraperitoneal	Died, 32 days	General tuberculosis of the usual type.
999	850	1,250	1·0 mg.	Subcutaneous	Killed, 111 days	The local lesion consisted of a large thin-walled cyst containing a little purulent matter, contents having been evacuated through small ulcer in the skin; the nearest glands were partly caseous; the iliac glands consisted of bags of dark yellow caseo-pus; the liver and spleen were normal; the kidneys showed a few grey tubercles; the lungs collapsed, but were congested and contained a few miliary tubercles; in the margins also there were fibro-caseous nodules, one of which was filled with muco-pus. Both ankle joints and several metatarsal joints were distended with mucoid caseous substance containing numerous tubercle bacilli.

RABBITS INOCULATED WITH CULTURE DERIVED FROM THE LUNG OF DOG 50, THROUGH GUINEA-PIG 1708.

The culture used was the 3rd generation, 20 days old and had been in cultivation a total period of 75 days.

Number of Rabbit.	Initial Weight in Grammes.	Final Weight in Grammes.	Dose.	Method of Inoculation.	Duration of Life.	Result.
634	2,460	2,360	0·1 mg.	Intravenous	K. 91 days	The lungs were crepitant and contained a moderate number of flattened grey tubercles with caseous centres situated chiefly on the surface and around the margins of the lobes. Each kidney contained a few grey tubercles in the cortex, slightly opaque in the centre; in the medullary zone of one there was a fine caseous streak. There was one minute tubercle in the heart muscle and sparsely scattered yellow tubercles in the muscle of the back (T.B. were found in the latter).
633	2,730	2,360	1·0 mg.	Intravenous	K. 91 days	The lungs and kidneys in this case were more severely affected than in Rabbit 634. The ventral portions of the cephalic lobes were composed of translucent grey tissue with an irregular caseous network, the rest of the lobes being beset with grey caseating nodules here and there confluent. In the caudal lobes there were similar nodules, which around the margins and on the dorsal surfaces had become confluent. The bronchial glands contained one or two minute yellow tubercles. The kidneys each showed on the surface a moderate number of caseous nodules, largest 2 mm., most of which extended as radial streaks into the medulla, some nearly to the apex of the papilla; in the pelvis of one there was a small collection of caseo-pus. There were two pearly white tubercles in the liver: spleen normal.
636	1,530	930	0·1 mg.	Intraperitoneal	D. 68 days	Omentum slightly thickened and caseating; the parietal and visceral peritoneum showed moderately numerous translucent tubercles with caseous centres. The lungs contained sparsely scattered caseating nodules, up to 3·5 mm. mainly on the surface. Other organs and glands showed no tubercles.
632	1,800	1,300	1·0 mg.	Intraperitoneal	D. 81 days	Slight tuberculosis of peritoneum. The kidneys each showed a few caseous tubercles in the cortex and caseous streaks in the pelvis. The lungs contained fairly numerous irregular caseous areas and nodules with grey margins confined mainly to the outer part and margins of the lungs. There were minute opaque foci scattered about in the muscles (T.B. numerous).
635	1,650	1,950	1·0 mg.	Subcutaneous	K. 91 days	Small flat fibrous local lesion with a small amount of caseation. Two minute foci in one inguinal gland. About 10 small caseous nodules, with grey margins, in the lungs. No tubercles elsewhere.

Strain of Virus B XXII.

RABBITS INOCULATED WITH CULTURE DERIVED FROM THE LUNG OF DOG 72.

The culture was the 2nd generation, 22 days old ; the total age of the culture was 91 days.

Number of Rabbit.	Initial Weight in Grammes.	Final Weight in Grammes.	Dose.	Method of Inoculation.	Duration of Life.	Post-mortem Results.
867	1,500	910	0·1 mg.	Intravenous	26 days (died)	General tuberculosis of the usual type.
868	1,770	1,000	1·0 mg.	Intraperitoneal	19 days (died)	General tuberculosis ; parietal visceral peritoneum closely covered with minute tubercles ; minute foci just visible in spleen (enlarged) and liver.
869	1,690	--	0·1 mg.	Intraperitoneal	8 days (died)	Coccidiosis of liver.
870	1,760	1,160	0·1 mg.	Subcutaneous	55 days (died)	Local lesion size of a hen's egg ; neighbouring glands caseous ; the lungs were crepitant and contained a moderate number of nodules up to 3 mm. ; there were a few miliary tubercles in each kidney ; the spleen and liver were normal.
871	1,660	1.000	1·0 mg.	Subcutaneous	109 days (died)	The local lesion was a shallow ulcer, 1½ in. by ½ in. ; nearest glands partly caseous ; spleen and liver normal ; kidneys showed sparsely scattered tubercles up to a millet-seed ; the lungs were large and extensively caseated.

INFLUENCE OF GLYCERIN ON VIRULENCE.

All the cultures used in the estimation of the virulence of different viruses for rabbits have been isolated and maintained throughout on media free from glycerin. It has been shown that when cultures of the tubercle bacillus are subcultivated on glycerin media for several generations all viruses grow with increased luxuriance, and a series of experiments on rabbits was carried out to ascertain how far this increased saprophytic power had affected the virulence.

Several viruses have been subcultivated for varying periods on glycerin media. Five of these have been tested on rabbits and the virulence of four compared with that of cultures kept on serum alone.

With each of the latter cultures, four rabbits were inoculated subcutaneously with doses of 1·0, 0·1, 0·01, 0·001 mg. respectively. Four of the rabbits died in 15, 17, 22, and 34 days respectively, and one was killed after 34 days; one of the former died of tuberculosis, the inoculation having been partly intraperitoneal; in the others the tuberculosis was limited.

Another rabbit (No. 545) lived for 315 days, gaining 770 grammes in weight, and was killed in apparent good health. The post-mortem examination showed a limited amount of general tuberculosis, of an unusually chronic type.

All the other rabbits died of general tuberculosis, the disease, as usual after subcutaneous inoculation, being most severe in the lungs and kidneys.

The results of these have been analysed and are exhibited in tabular form.

Dose.	Serum Cultures.		Glycerin Serum Cultures.	
	Number of Rabbits.	Average Duration of Life.	Number of Rabbit	Average Duration of Life.
1·0 mg.	3	96 days	4	75·75 days
0·1 mg.	4	76·5 days.	3	103·6 days.
0·01 mg.	4	114·5 days.	3	81 days.
0·001 mg.	2	80·5 days.	3	108 days.

The duration of the experiments in each series of inoculations varied very considerably, and in several instances the rabbits inoculated with the smaller doses have died before those inoculated with the larger doses, for example, in one experiment the rabbit inoculated with 0·001 mg. succumbed before any of the other three inoculated with 1·0, 0·1, and 0·01 mg.

It is clearly impossible, therefore, to draw any conclusion from a comparison of the duration of life of animals inoculated with the same dose from the different cultures.

All that the experiments can be said to show is that the glycerin cultures gave rise to a tuberculosis identical in all its features with that set up by serum cultures.

The fifth culture was inoculated intravenously into four rabbits, the same doses being employed as for the rabbits inoculated subcutaneously. In this experiment no rabbits were inoculated with the culture grown on serum alone, but a comparison of these results with those obtained with an earlier generation of the serum culture demonstrates that the culture grown on glycerin serum had not undergone any diminution in virulence.

TABLES SHOWING THE COMPARATIVE VIRULENCE OF CULTURES GROWN SINCE ISOLATION ON SERUM ALONE AND OF THE SAME CULTURE GROWN FOR A PERIOD ON GLYCERIN MEDIA.

VIRUS B. XI. (RABBIT 66).

Rabbits inoculated with culture grown since isolation on pure serum.

The subculture used was the 16th generation, 20 days old.

The total age of the cultures in each case was 454 days.

Number of Rabbit.	Initial Weight in Grammes.	Final Weight in Grammes.	Dose.	Duration of Life.	Result.
573	2,560	1,550	1·0 mg.	82 days (died)	Very large caseous local lesion, the skin over which was freely moveable; it infiltrated the muscles, but did not involve the peritoneum: adjacent glands enlarged and caseous; lungs enlarged and packed with caseating nodules, confluent in the cephalic lobes; the spleen and liver contained each three small tubercles; the kidneys contained numerous caseating nodules 1 to 3 mm. in diameter; the portal and thoracic glands contained small discrete tubercles; the pleura showed a few flattened caseous nodules, and there were some caseous nodules in the appendix and dilated end of ileum.
564	1,150	1,000	0·1 mg.	71 days (died)	Local caseous ulcer; adjoining glands caseous; liver and spleen normal; kidneys and lungs very closely beset with caseating nodules; in the latter very little functional tissue remained; bronchial and lumbar glands slightly affected; there was one nodule on the pleura.
566	1,070	1,000	0·01 mg.	92 days (died)	Local ulcer; inguinal and iliac glands caseous; the lungs filled the chest and were extensively caseated; the spleen was normal; the liver contained one nodule (T.B.); the kidneys numerous nodules, up to 4 mm.; caseous foci in lumbar and bronchial glands; left lobe of thyroid enlarged and caseous throughout; caseous nodules in centre of a rib and numerous flattened nodules on the pleura.
568	1,260	1,110	0·001 mg.	64 days (died)	Large caseous local lesion infiltrating muscles of abdominal wall and involving peritoneum; skin not adherent; omentum contained a number of discrete caseous tubercles; the lungs were filled with irregular caseous nodules and masses; the kidneys contained a moderate number of caseous nodules; there was one tubercle in the spleen, and most of the abdominal and thoracic

Rabbits inoculated with culture grown for 199 days on pure serum and 255 days on glycerin media.

The subculture used was the 20th generation, 20 days old.

All the inoculations were made subcutaneously.

Number of Rabbit.	Initial Weight in Grammes.	Final Weight in Grammes.	Dose.	Duration of Life.	Result.
574	1,270	980	1·0 mg.	63 days (died)	Local caseous ulcer, 1 in. in diameter; neighbouring glands enlarged and caseous; the lungs filled the chest and were composed practically throughout of caseous nodules and masses; the spleen contained numerous caseous miliary tubercles; liver normal; the kidneys showed very numerous projecting caseous nodules with grey margins, up to 3 and 4 mm. in diameter; the bronchial, portal and mesenteric glands contained caseous tubercles; the appendix and dilated extremity of ileum contained numerous caseous tubercles.
575	1,210	870	0·1 mg.	22 days (died)	Flattened subcutaneous caseous patch, skin adherent, but not ulcerated; inguinal glands contained caseous nodules; sternal glands a little enlarged; no tuberculosis elsewhere; liver enlarged and packed with yellow nodules (psorospermosis).
576	1,330	1,420	0·01 mg.	81 days (died)	Caseous ulcer in abdominal wall; adjoining glands caseous; lungs enlarged and filled with caseous nodules; there were three tubercles in the spleen, two in the heart muscles, and four in the thyroid body, and numerous nodules (up to 1 cm.) in the kidneys; most of the lymphatic glands were affected.
577	1,070	1,150	0·001 mg.	102 days (died)	Large caseous tumour, not ulcerated; lungs and kidneys closely beset with caseating tubercles and nodules; spleen and liver normal; minute caseous tubercles in right lobe of thyroid; many lymphatic glands were tuberculous.

VIRUS B. IV. (Calf 138).

Rabbits inoculated with culture grown since isolation on pure serum.

The subculture used was the 15th generation, 21 days old.

The total age of the cultures in each case was 558 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Duration of Life.	Result.
542	1,090	870	1·0 mg.	115 days (died)	Ulcerated caseous lesion; neighbouring glands enlarged and caseous; lungs enlarged and extensively caseated; there was one tubercle in the spleen; none in the liver; the kidneys contained sparsely scattered nodules, the largest the size of a pea; there were moderately numerous miliary tubercles in the omentum; the right lobe of the thyroid was partly caseous; bronchial glands tuberculous.
543	1,400	900	0·1 mg.	94 days (died)	Large caseous lesion; skin ulcerated; nearest glands caseous; lungs firm and infiltrated with a caseous network; numerous nodules in the kidneys; spleen and liver normal; one tubercle in the thyroid body; caseous foci in mesenteric and ventral mediastinal glands; bronchial glands normal; the omentum contained a few minute tubercles.
544	1,390	1,000	0·01 mg.	103 days (died)	Local ulcer; adjoining glands caseous; lungs filled the chest and were closely beset with coalescing caseating nodules; there were three tubercles in the spleen, four in the liver and numerous projecting nodules in the kidneys; there were two tubercles in the heart muscle and a few in the omentum; most of the lymphatic glands in the body were caseating.
545	1,280	2,050	0·001 mg.	315 days (killed)	The local lesion was an elongated fibrous tumour containing caseo-purulent nodules; yellow calcareous granules in left inguinal gland; left iliac glands formed a mass of cysts filled with caseo-pus; kidneys and liver showed a few tubercles the size of rape-seed, spleen normal; lungs a little congested and œdematous, contained scattered miliary tubercles, some caseous in the centre, and several pea-sized nodules containing caseo-pus; a lumbar and a sternal gland were enlarged and filled with caseo-pus.

Rabbits inoculated with culture grown for 302 days on pure serum and 256 days on glycerine media.

The subculture used was the 19th generation, 21 days old.

All the inoculations were made subcutaneously.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Duration of Life.	Result.
538	1,120	800	1·0 mg.	94 days (died)	Shallow ulcer with caseous margins; inguinal glands partly caseous; lungs crepitant, collapsed normally; they contained scattered caseous nodules and small grey tubercles with opaque centres; liver and spleen normal; the kidneys together contained ten tubercles, largest three mm. in diameter; thoracic and most of abdominal lymphatic glands were normal; there were some miliary tubercles in the omentum.
539	1,410	1,000	0·1 mg.	135 days (died)	Elongated shallow ulcer covered with dry scab; inguinal glands caseous and softened; no tubercles in spleen; a few doubtful grey points in liver; lungs enlarged, tissue extensively replaced by large caseous masses; caseous tubercle in each lobe of thyroid; caseous points in iliac, lumbar and mesenteric glands; other glands normal.
540	1,240	830	0·01 mg.	129 days. (died)	Local ulcer; nearest gland caseous; lungs collapsed moderately well and contained fairly numerous caseous nodules up to 2 or 3 mm. (in this case there was more normal lung tissue than usual); kidneys contained numerous nodules varying much in size; spleen normal; liver, a few doubtful grey points; minute tubercle in right lobe of thyroid.
541	1,200	950	0·001 mg.	114 days (died)	Local ulcer covered with dry scab; nearest glands caseous; other lymphatic glands caseous to a varying extent; lungs closely beset with more or less discrete caseous nodules (moderate amount of crepitant lung tissue); one tubercle in spleen; none in liver; numerous nodules in kidneys; numerous miliary tubercles in omentum; numerous caseous nodules in vermiform appendix and dilated end of ileum; two tubercles in a submaxillary salivary gland; large caseous nodule in one rib, expanding the bone.

VIRUS B. IV. (Calf 118).

Rabbits inoculated with culture grown on serum alone.

The subculture used was the 24th generation, 21 days old.

Rabbits inoculated with culture grown for 490 days on pure serum, and for 263 days on glycerin media.

The subculture used was the 25th generation, 21 days old, and was grown on ox serum.

The total age of the cultures in each case was 753 days. All the inoculations were made subcutaneously.

Number of Rabbit.	Initial weight in grammes	Final weight in grammes.	Dose.	Duration of Life.	Result.	Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Duration of Life.	Result.
550	1,470	1,000	1·0 mg.	91 days (died)	Local ulcer; inguinal glands partly caseous; lungs enlarged and extensively caseated; kidneys contained numerous nodules, up to a pea in size; the spleen, two yellow tubercles; liver normal; numerous miliary tubercles in omentum, and a few flattened growths on the pleura; slight tuberculosis of thoracic and mesenteric glands.	546	1,450	960	1·0 mg.	58 days (died)	Large dense caseous tumour with a small ulcer in the skin; nearest glands caseous throughout; lungs closely beset with coalescing caseating nodules; kidneys contained numerous nodules; numerous tubercles in spleen and liver; bronchial, portal and coeliac glands partly caseous.
551	1 490	920	0·1 mg.	74 days (died)	Local ulcer; inguinal glands very large and caseous throughout; lungs contained moderately numerous caseous nodules; each kidney contained about a dozen; there were four caseous tubercles in the spleen, none in the liver; caseous tubercles in bronchial and mesenteric glands.	547	1,520	1,270	0·1 mg.	86 days (died)	Circular ulcer 1¼ inches in diameter; inguinal glands caseous throughout; lungs crepitant, contained moderately numerous discrete caseating tubercles; scattered miliary tubercles in kidneys; spleen and liver appeared normal; many lymphatic glands were caseous; three minute tubercles in the thyroid body.
552	1,840	1,150	0·01 mg.	90 days (died)	Local ulcer; inguinal glands large, caseous and softened; lungs extensively caseated; numerous caseous nodules, varying from a pin's head to a pea, in the kidneys; liver and spleen normal. Small caseous nodules in bronchial and mesenteric glands; two opaque foci in right lobe of thyroid.	548	1,830	1,230	0·01 mg.	128 days (died)	Large partially softened caseous tumour with a dry ulcer in the skin; nearest glands caseous; lungs closely beset with caseating nodules, confluent in places; the thoracic and most of the abdominal lymphatic glands were normal; spleen and liver normal; moderately numerous nodules in kidneys; left lobe of thyroid one third caseous.
553	1,440	1,450	0·001 mg.	34 days (killed)	Caseating local lesion; inguinal glands partly caseous; right caudal lobe contained one hard caseous tubercle, twice the size of a millet-seed; one tubercle in the spleen; no tubercles elsewhere.	549	1,440	1,130	0·001 mg.	34 days (died)	Flat ulcerated caseous lesion; inguinal glands partly caseous; there was a caseous focus in each iliac gland; the lungs contained two tubercles, one the size of a wheat grain, the other just visible; no tuberculosis elsewhere.

VIRUS B. XVIII. (DIRECT).

Rabbits inoculated with culture grown throughout on pure serum.

The subculture used was the 16th generation, 20 days old.

The total age of the cultures in each case was 316 days.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Duration of Life.	Result.
591	1,400	1,090	1·0 mg.	17 days (died)	Subcutaneous caseous lesion; omentum thickened and caseating; numerous minute tubercles on parietal and visceral peritoneum; sparsely scattered minute tubercles in lungs and minute grey foci in spleen and liver (inoculation partly intraperitoneal).
589	1,270	1,100	0·1 mg.	67 days (died)	Large irregular ulcer and general tuberculosis of the usual type; both the spleen and liver contained caseous tubercles.
590	1,140	1,250	0·01 mg.	173 days (died)	Very large cyst, with caseo-purulent contents in subcutaneous tissues of abdominal wall; neighbouring glands enlarged and caseous; lungs closely beset with coalescing caseating masses; kidneys packed with caseous nodules; liver and spleen appeared normal; omentum retracted and beset with caseating tubercles; tendon of diaphragm covered with firm fibro-caseous masses, 1 cm. in greatest thickness; large caseating masses in dorsal and ventral mediastina, latter completely covering costal cartilages; numerous filmy growths on parietal pleura; most of the lymphatic glands were caseous.
592	1,100	1,150	0·001 mg.	97 days (died)	Large caseous ulcerated lesion and general tuberculosis of the usual type, the lungs being the most severely affected; the liver contained psorosperm nodules, but no tubercles.

Rabbits inoculated with culture grown for 107 days on pure serum and for 209 days on glycerin serum.

The subculture used was the 16th generation, 20 days old, and was grown on ox serum.

All the inoculations were made subcutaneously.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Duration of Life.	Result.
595	1,490	1,150	1·0 mg.	88 days (died)	Large caseous softened lesion, ulcerated on the surface; neighbouring glands tuberculous; lungs crepitant, contained numerous discrete caseating tubercles, the largest the size of a hemp-seed; moderately numerous tubercles in the kidneys; a few grey points in the liver (T.B.) and one yellow tubercle in the spleen; thoracic glands partly caseous.
593	1,270	970	0·1 mg.	90 days (died)	Softened caseous tumour, with a small ulcer in the skin; neighbouring glands caseous; lungs contained a moderate number of discrete caseous nodules; the lung tissue intervening was normal in appearance; moderate number of caseous nodules in kidneys; one yellow tubercle in spleen; liver normal.
594	1,170	1,140	0·01 mg.	86 days (died)	Large softened caseous tumour, ulcerated on the surface; inguinal glands enlarged and caseous; lungs packed with caseating nodules; sparsely scattered nodules, mostly small, in kidneys; spleen enlarged, Malphigian bodies prominent; liver normal; opaque tubercle in thyroid body (T.B.).
596	1,010	680	0·001 mg.	15 days (died)	Small caseous ulcer; nearest glands normal; there was one translucent tubercle in the lungs.

Virus B. V. (G.P. 711).

Rabbits Inoculated with Culture grown for 743 Days on Pure Serum and 166 Days on Glycerin Media.

The subculture used was the 21st generation, 21 days old, and was grown on ox serum.

The total age of the culture was 809 days.

All the inoculations were made intravenously.

Number of Rabbit.	Initial weight in grammes.	Final weight in grammes.	Dose.	Duration of Life.	Result.
790	1,300	950	1·0 mg.	15 days (died)	Lungs collapsed fairly well and contained numerous discrete grey tubercles, the largest the size of a millet seed; the bronchial glands showed opaque caseous foci in the cortex; spleen enlarged, firm, peppered with minute grey foci; there were a few grey points just visible in the liver and one or two minute foci in each kidney; a few opaque foci in cortex of portal and lumbar glands.
791	1,550	910	0·1 mg.	21 days (died)	Lungs crepitant but a little fuller than normal; they contained fairly numerous miliary caseating tubercles; the bronchial glands contained caseous nodules; spleen enlarged, firm and dark red, closely beset with minute yellowish foci; liver speckled throughout with minute grey foci; moderately numerous caseating tubercles in the kidneys; minute caseous tubercles in most of the lymphatic glands.
792	1,750	1,320	0·01 mg.	62 days (died)	The lungs were enlarged, firm, and composed for the most part of grey translucent tissue with an irregular caseous network. Spleen slightly enlarged, contained moderately numerous discrete yellow tubercles; in the cortex of each kidney were numerous grey tubercles, with caseous centres, many of which extended towards the pelvis as radial streaks; liver appeared normal; cortex of portal gland partly caseous; most other lymphatic glands contained caseous tubercles; scattered about the muscles were moderately numerous yellow tubercles up to a pin head in size.
793	1,200	1,000	0·001 mg.	52 days (died)	Lungs were pinkish and crepitant and contained sparsely scattered caseous tubercles, up to 2 mm. in diameter, with grey margins; they were irregular in outline and situated chiefly just under the pleura; the bronchial glands showed a few opaque foci; the mediastinal were partly caseous; there was some turbid fluid in the peritoneal cavity; spleen enlarged, firm and closely beset with irregular greyish white foci, here and there aggregated together; the liver showed numerous minute grey foci and grey submiliary tubercles; kidneys appeared normal; several lymphatic glands contained caseous tubercles.

REPORT ON THE STABILITY OF VIRULENCE OF THE TUBERCLE BACILLUS OF HUMAN ORIGIN IN ARTIFICIAL CULTURE, INCLUDING THE INFLUENCE OF GLYCERIN IN CULTURE MEDIA.

Dr. L. COBBETT.

THE STABILITY OF VIRULENCE OF THE TUBERCLE BACILLUS IN ARTIFICIAL CULTURE.

The Virulence of Tubercle Bacilli from Cultures and in Emulsions of Tuberculous Animal Tissues compared.

It has been repeatedly observed, both here and at Walpole, that to produce tuberculosis of a given severity in an animal a much larger number of tubercle bacilli are required when these are obtained from a culture, even a young one, than when they are contained in an emulsion of tuberculous tissue from an infected animal. Dose for dose, estimated in numbers of tubercle bacilli, cultures are decidedly less virulent than emulsions of tuberculous tissue*. It is difficult to say whether this is because cultures contain a larger proportion of dead bacilli, whether tuberculous tissues contain poisons which cultures do not, whether the tissue matter acts mechanically and protects the bacilli from the attack of the defensive forces of the body, or whether an attenuation of the bacilli takes place when they are transferred from their natural habitat to our artificial culture media.

If the last of these be the true explanation, then one would expect the attenuation to be not altogether sudden and complete, but to extend over the earlier period of cultivation for perhaps several weeks, so that it should be possible to demonstrate the attenuation by comparing the virulence of very early cultures with others which had been growing for a longer period.

No direct experiments of this kind have been made, but seeing that our viruses of Group I. differ in virulence from one another so little that the difference has not been demonstrated by our experiments (H 49, T.C. excepted), it is possible to compare the virulence of recently isolated strains of culture of one virus with those of another which have been longer in cultivation. For this purpose several strains have been raised as quickly as possible, and bacilli from cultures three weeks old have been injected within 5, 7, or 8 weeks of the time when the virus was first sown on artificial media. These can be compared with other strains which have been several years under artificial cultivation.

This comparison is, of course, not an exact one, for not only were different viruses employed, but the calves were not of precisely the same size, and differed much, as we know, in individual susceptibility. Yet the results are very fairly uniform, and had any considerable attenuation taken place during the period of cultivation it could not have failed to make itself known.

* Experience here has shown that to produce a rapidly fatal tuberculosis in the calf by subcutaneous injection about 500,000,000 tubercle bacilli direct from an animal lesion are required, while, if cultures be used, 50 mg. is necessary, 10 mg. being not certainly fatal. (Compare the table of tissue emulsion injections with that of culture injections; Vol. II., Section II.) Since Dr. Eastwood estimates that 10 mg. contains 45,000,000,000 bacilli, the bacilli from recent tuberculous lesions in the animal (guinea-pig, calf, or rabbit) would appear to be equivalent to more than 100 times as many obtained from a young serum culture.

TABLE SHOWING THE RELATION OF THE TOTAL AGE OF CULTURE (time since isolation from animal during which the bacilli have grown in artificial cultivation on pure serum) TO VIRULENCE.

GROUP I. VIRUSES.

All Cultures injected were grown for 3 weeks.

DOSE: 50 MILLIGRAMMES.

Virus.	Direct Source of Culture.	No. of Calf.	Total Duration of Artificial Cultivation.	Duration of Life of Calves.	
H 31. "L.F."	Human cervical glands	519	5 weeks	Died	21 days
	"	521 (30 mg)	"	Dying	36 "
H 59. "L.B."	Human lung	965	7 weeks	"	60 "
H 64. "M.G."	Human mesenteric glands, through G.P. 1849.	1063	2 months	Died	28 "
	Human meninges, through G.P. 1880.	1091	"	Dying	37 "
H 49. "T.C."	Human mesenteric glands, through G.P. 1910.	1115	"	Died	38 "
	Calf 797	931	"	"	21 "
	"	949	"	"	39 "
H 32. "Y.W."	Human mesenteric glands	587	3 months	"	35 "
	" "	589	"	"	40 "
H 16. "J.H."*	Bull 423A (5th passage)	541	"	Very ill	25 "
	" "	559	"	Dying	39 "
H 17. "Sp.B."*	Calf 571 (7th passage)	739	3½ months	"	33 "
	" "	743	"	"	37 "
H 65. "K.B."	Human mesenteric glands, through G.P. 1835.	1093	3⅔ months	Died	30 "
H 28. "C.L."	Human cervical glands	489	4 months	Dying	28 "
	" "	469 (43 mg)	"	Very ill	29 "
H 32. "Y.W."	Human bronchial glands, through Rabbit 166.	837	6 months	Died	33 "
	" " "	849	"	"	23 "
H 29. "M.F."	Human cervical glands, through G.P. 1329.	693	8 months	"	63 "
	" " "	697	"	Dying	35 "
H 10. "B.S."	Calf 113	387	"	"	34 "
H 20. "F.L."	Calf 213	547	"	Died	36 "
	"	561	"	"	33 "
H 19. "S.W."	Heifer 239	577	10 months	"	37 "
	"	597	"	Dying	45 "
H 29. "M.F."	Human cervical glands, through G.P. 1329.	841	11 months	Died	23 "
	" " "	847	"	Very ill	31 "
H 38. "J.M."	Human cervical glands, through G.P. 793.	643	14 months	Died	28 "
	" " "	647	"	"	32 "
H 13. "A.D."*	Calf 321	825	15½ months	"	25 "
H 10. "B.S."	Calf 113	667	16½ months	Well	90 "
H 14. "F.S."	Human mesenteric glands, through G.P. 587.	545	16 months	Died	45 "
	" " "	671	19 months	"	33 "
H 2. "Sp.A."	Heifer 11	569	20 months	Dying	20 "
	"	575	"	"	33 "
H 7. "C.M."	Calf 5, through G.P. 448	563	20 months	Very ill	32 "
	" "	773	25 months	Dying	40 "
	" "	775	"	"	47 "
H 14. "F.S."	Human mesenteric glands, through G.P. 587.	899	2 yrs., 3 months	Died	44 "

* After increase of virulence.

Does Long-continued Cultivation on Artificial Media Attenuate the Tubercle Bacillus?

The evidence of attenuation during artificial cultivation afforded by the calves injected with 50 mg. of bacilli is of the slightest (*see* Table, page 188). The youngest strain, which had only been five weeks under cultivation when injected, killed a calf in almost the shortest period, namely, 21 days. One two months old killed as quickly. But another, cultivated for 20 months, killed even more rapidly, namely, in 20 days. One calf alone survived the period of observation. It had been injected with a strain which had been cultivated for 16½ months. Possibly all these were exceptional animals.

Twenty-eight calves were injected with bacilli from strains which had been less than a year in cultivation; all died or were in a dying condition when killed. Twelve were injected with bacilli from strains more than a year in cultivation; all died or were killed when dying, except the one which was in good condition when killed 90 days after injection. Counting the duration of life of this one as 90 days, the average duration of illness for the two groups works out at 34·6 and 39 days respectively. Strains which had been more than two years in cultivation killed in 40, 44, and 47 days.

Thus the evidence of this table is not opposed to a slight and slowly progressing attenuation.

Table showing the Relation of the Total Age of Culture (time since isolation from animal during which the bacilli have grown in artificial cultivation on pure serum) to Virulence.

Group I. Viruses.

All Cultures injected were grown for 3 weeks.

Dose: 10 Milligrammes.

Virus.	Direct Source of Culture.	No. of Calf.	Total Duration of Artificial Cultivation.	Duration of Life of Calves.	
H 59. "L.B." -	Human lung - - - - -	987	7 weeks	Dying	49 days
	" " - - - - -	991	"	Well	94 "
	Human cervical glands - - -	1037	2 months	Died	41 "
	Human brain, through G.P. 1860 -	1057	"	"	31 "
	Human mesenteric glands, through G.P. 1849.	1077	"	"	31 "
H 64. "M.G."	Human mesenteric glands, through G.P. 1910.	1111	"	Dying	60 "
	" " "	1113	"	Well	86 "
H 49. "T.C."*	Calf 797 - - - - - -	933	"	Died	19 "
	" - - - - - -	929	"	Dying	39 "
H 17. "Sp. B."*	Calf 553 (5th Passage) - - -	901	3 months	Died	54 "
	" " - - - -	919	"	"	31 "
H 65. "K.B."-	Human mesenteric glands, through G.P. 1835.	1085	3⅔ months	Dying	51 "
	" " "	1089	"	Died	38 "
H 32. "Y.W."	Human bronchial glands, through Rabbit 166.	851	6 months	"	47 "
	" " "	855	"	"	34 "
H 29. "M.F."	Human cervical glands, through G.P. 1329.	835	11 months	"	40 "
	" " "	839	"	Dying	69 "
H 28. "C.L." -	Human cervical glands - - -	813	13 months	Died	39 "
	" " - - -	815	"	Dying	48 "
	Human cervical glands, through Rabbit 102.	863	14½ months	Well	90 "
	" " "	877	"	Died	37 "
	" " "	819	"	"	52 "
	" " "	875	"	Well	91 "
H 31. "L.F." -	Human cervical glands - - -	939	14 months	Died	43 "
	" " - - -	947	"	"	72 "
H 13. "A.D."*	Calf 321 - - - - - -	821	15½ months	Dying	50 "
	" - - - - - -	823	"	Well	90 "
H 20. "F.L." -	Calf 213 - - - - - -	879	17 months	Dying	36 "
	" - - - - - -	867	"	Well	90 "
H 19. "S.W."-	Goat 11 - - - - - -	921	18 months	Died	48 "
	" - - - - - -	913	"	Well	90 "
H 38. "J.M." -	Human cervical glands, through G.P. 793.	845	19½ months	Dying	36 "
	" " "	827	"	Well	108 "
H 2. "Sp. A." -	Calf 93, through G.P. 534 - -	889	2 yrs. 3 mos.	Died	53 "
	" " - -	883	"	Well	90 "
H 14. "F.S." -	Human mesenteric glands, through G.P. 587.	893	"	Died	47 "
	" " "	895	"	In fairly good health	90 "
H 10. "B.S." -	Calf 35, through G.P. 725 - -	967	"	Dying	48 "
	" " - - -	969	"	"	48 "
H 7. "C.M." -	Calf 5, through G.P. 448 - - -	1029	3 years	Well	90 "
	" " - - -	1041	"	Died	41 "

* After increase of virulence.

Among the calves injected with 10 mg. of bacilli about 1 in 4 survived for 90 days, and many of these were in good condition and had only a slight amount of tuberculosis when killed. It is perhaps significant that 9 of these less severely affected animals occurred among the 24 calves injected with strains which had been cultivated for more than a year, and two only among the 17 injected with strains which had been cultivated for less than a year.

But these calves were highly-resisting individuals, for their fellows, treated like themselvs, died of acute tuberculosis; it would not be reasonable to expect the exceptional individuals to be distributed equally among two such groups as we are considering. It may therefore be that a capricious distribution of animals of high resistance accounts for this evidence of attenuation.

If we leave out all the calves which survived 90 days, regarding them as exceptionally resistant individuals, and consider only those which died—though doubtless these also differed from one another in susceptibility—we find that of 15 animals injected with strains which had been less, and 15 with strains over a year in artificial cultivation, the average duration of illness was 42·3 and 46·5 days respectively. Here again the evidence is in favour of a slowly progressing attenuation.

Analysis of the Table of Rabbit Injections.

Arranged in the order of the duration of cultivation of the viruses.

Virus.	Strain of Culture used for Rabbit injections.	Total Duration of Artificial Cultivation.	Dose in Milligrammes.			
			10·	1·	0·1	0·01
H 31. "L.F." -	Human cervical glands - - -	5 weeks	–	–	47	–
H 59. "L.B." -	Human lung - - - - -	7 weeks	–	22	?	–
	Human cervical glands - - -	2 months	–	18	69	–
	Human brain - - - - -	,,	–	23	?	–
	Human mesenteric glands - -	,,	–	34	34	–
H 64. "M.G."	,, ,, ,, - -	,,	–	25–21	34	–
H 49. "T.C."*	Calf 797 - - - - - -	,,	15	21–21	24	–
H 14. "F.S." -	Calf 895 - - - - - -	2½ months	–	24	33	–
H 32. "Y.W."	Human mesenteric gland - - -	3 months	–	21	30	–
H 16. "J.H."*	Bull 423A - - - - - -	,,	23	28	60	–
H 17. "Sp.B."*	Calf 571 - - - - - -	3½ months	16	23	37	40
H 65. "K.B." -	Human mesenteric gland - - -	3⅝ months	17	28–30	28	–
H 32. "Y.W."	Human bronchial gland - - -	6 months	32	?	15	27
H 29. "M.F."-	Human cervical gland - - -	7 months	16	18	22	29
	,, ,, ,, - - -	8 months	–	22	30	91
H 20. "F.L." -	Calf 213 - - - - - -	,,	17	18	30	–
H 19. "S.W." -	Heifer 239 - - - - -	10 months	20	19	42	32
H 29. "M.F."-	Human cervical glands - - -	11 months	11	15	?	?
H 2. "Sp.A." -	Calf 91 - - - - - -	13 months	25	39	35	–
H 28. "C.L." -	Human cervical glands - - -	,,	36	40	40	?
H 38. "J.M." -	,, ,, ,, - - -	14 months	15	22	–	–
H 31. "L.F." -	,, ,, ,, - - -	,,	25	31–26	–	–
H 28. "C.L." -	Rabbit 102 - - - - -	14½ months	16	23	23	31
H 13. "A.D."*	Calf 321 - - - - - -	15½ months	?	33-36	?	–
H 10. "B.S." -	Calf 113 - - - - - -	16½ months	21	23	44	78
H 14. "F.S." -	Human mesenteric glands - -	16 months	21	29	57	–
H 20. "F.L." -	Calf 213 - - - - - -	17 months	18	25–19	25	–
H 19. "S.W." -	Goat 11 - - - - - -	18 months	9	17	24	–
H 2. "Sp.A." -	Heifer 11 - - - - - -	,,	17	35	24	–
H 14. "F.S." -	Human mesenteric glands - -	19 months	18	23	23	29
H 38. "J.M." -	Human cervical glands - - -	19½ months	10	17	19	23
H 2. "Sp.A." -	Heifer 11 - - - - - -	20 months	17	23	35	–
H 7. "C.M." -	Calf 5 - - - - - - -	,,	22	27	55	–
	,, - - - - - - -	25 months	16	37	?	39
H 14. "F.S." -	Human mesenteric glands - -	27 months	–	?	25–23	–
H 2. "Sp.A." -	Calf 93 - - - - - -	,,	17	31–42	34	–

* After increase of virulence.

The analysis of the rabbit injections gives no evidence of attenuation during cultivation.

Summary.

Both tables of calf injections agree together in showing that strains of bacilli which have been long in cultivation are slightly less virulent than those which have been more recently isolated. The number of animals is probably too great to admit of this agreement being merely accidental. We must, therefore, conclude that a slowly progressing attenuation does take place during long continued cultivation. But the striking fact shown by these tables is not the evidence of attenuation, but on the contrary, the slightness of this evidence, which strongly testifies to the remarkable stability of virulence [as compared with some other micro-organisms] of the tubercle bacillus, which hardly becomes appreciably attenuated after several years cultivation. There is no evidence that attenuation is more rapid in the early days of cultivation than it is at a later period. The analysis, therefore, lends no support to the view that bacilli from cultures are *intrinsically* less virulent than those from tissue emulsions.

THE INFLUENCE OF GLYCERIN IN CULTURE MEDIA.

All the experiments already considered in this Report were made with cultures on pure serum. It has been suggested that glycerin in culture media may attenuate tubercle bacilli. This is of importance because some of our Group II. viruses have been injected after growing on glycerin-serum, and this medium has been largely used by me for separating mixtures of eugonic and dysgonic tubercle bacilli, or for attempting such separation from strains which came under suspicion. It was there desirable to investigate whether glycerin in culture media has any attenuating action

Two questions presented themselves—

(1) If two cultures be sown, from a stock hitherto grown on pure serum, on serum with and on serum without glycerin, will there be any difference in the severity of the illness caused by injecting them into animals in equal quantities ?

(2) Does long-continued cultivation on glycerin-media attenuate ?

In order to answer these questions many animals were injected in pairs, one of each pair with a culture grown on serum and the other with one grown on 5 per cent. glycerin-serum

Calf Injections.

In the case of only one virus of Group I., H 7, "C.M.," have comparative injections been made on calves. In that case there was no appreciable difference in the result. Four non-virulent strains were injected, for the purpose, as already described, of controlling those calf injections which have already been made with non-virulent cultures. They show no appreciable difference in the result, whether cultures on pure serum or on serum with 5 per cent. of glycerin were used.

Rabbit Injections.

A much larger number of comparative injections with viruses of Group II. have been made on rabbits. These also show no appreciable difference in pathogenicity between cultures grown on pure serum and those on glycerin-serum. For the sake of brevity the table is omitted.

Comparative injections have been made on rabbits with eleven strains (ten viruses) of bacilli of Group I.

In some cases where life was much prolonged the cause has been found in the fact that the injection was not really intraperitoneal, but only partly intraperitoneal and partly intramuscular, or intracœcal. Omitting these spoilt experiments, there is a clear consensus of evidence that pure serum cultures kill quicker than those on glycerin-serum. The difference is not great, but is fairly constant (with a few exceptions, easily explicable on the hypothesis of variations in individual susceptibility).

TABLE SHOWING COMPARATIVE SUBCUTANEOUS INOCULATIONS OF SERUM AND GLYCERIN-SERUM CULTURES OF VARIOUS VIRUSES INTO CALVES.

A. SERUM CULTURES.

CALVES.

Virus.	Immediate source of Culture.	Dose in Milligrammes.	No. of Calf.	Weight in Kilogrms.	Result.		
	VIRUSES OF GROUP I.						
H 7. "C.M." - -	G.P. 448, from Calf 5.	50·	773	50·34	Killed, when dying,	after 40 days	General tuberculosis, severe.
	VIRUSES OF GROUP II.						
H 44. "D.C." - -	Human cervical glands, direct.	50·	731	46·70	Killed, when well,	after 90 days	Slight tuberculosis.
H 18. "T.T." - -	Calf 405.	50·	645	50·80	„	„ 90 „	Slight tuberculosis.
H 30. "E.M." - -	G.P. 1337, from human bronchial and mesenteric glands.	50·	593	69·84	„	„ 88 „	Slight tuberculosis.
H 11. "E.D." - -	G.P. 1298, from Calf 309.	50·	719	49·40	„	„ 90 „	Local tuberculosis.

TABLE SHOWING COMPARATIVE SUBCUTANEOUS INOCULATIONS OF SERUM AND GLYCERIN SERUM CULTURES OF VARIOUS VIRUSES INTO CALVES.

B.—GLYCERIN SERUM CULTURES.

CALVES.

Virus.	Immediate source of Culture.	Dose in milligrammes.	No. of Calf.	Weight in Kilogrms.	Result.	
	VIRUSES OF GROUP I.					
H 7. "C.M."	G.P. 448, from Calf 5.	50·	**775**	·70	Killed when dying, after 47 days	General tuberculosis, severe.
	VIRUSES OF GROUP II.					
H 44. "D.C."	Human cervical gland, direct.	50·	**705**	52·16	Killed when well, after 90 days	Slight tuberculosis.
H 18. "T.T."	Calf 405.	50·	**683**	58·06	„ „ „ 90 „	Local tuberculosis.
H 30. "E.M."	G.P. 1337 from human bronchial and mesenteric glands.	50·	**599**	63·95	„ „ „ 88 „	Local tuberculosis.
H 11. "E.D."	G.P. 1298 from Calf, 309.	50·	**723**	44·90	„ „ „ 90 „	Slight tuberculosis.

TABLE SHOWING COMPARATIVE INTRAPERITONEAL INOCULATIONS OF PURE SERUM AND GLYCERIN SERUM CULTURES OF VARIOUS VIRUSES INTO RABBITS.

A. SERUM CULTURES—Viruses of Group I.

RABBITS.

Virus.	Immediate Source of Culture.	Dose in Milligrammes	No. of Rabbit.	Weight in grms.	Killed or Died.	Duration of Life in Days	Results.
H 29. "M.F."	G.P. 1329 from human cervical glands.	1·	315	1,020	Died	22	General tuberculosis.
	,,	0·1	316	1,000	,,	30	,, ,,
	,,	0·01	317	840	,,	91	,, ,,
H 7. "C.M."	G.P. 448 from Calf 5	10·	408	1,100	Died	16	General tuberculosis.
	,,	1·	409	1,200	,,	37	,, ,,
	,,	0·1	410	1,020	Killed	94	Chronic tuberculosis of lungs and kidneys. (Injection intramuscular and intracæcal.)
	,,	0·01	411	1,130	Died	39	General tuberculosis.
H 10. "B.S."	G.P. 757 from Calf 113	10·	298	1,390	Died	21	General tuberculosis.
	,,	1·	297	1,390	,,	23	,, ,,
	,,	0·1	296	1,360	,,	44	,, ,,
H 38. "J.M."	G.P. 793 from human cervical glands.	10·	488	1,120	Died	10	General tuberculosis.
	,,	1·	489	1,010	,,	17	,, ,,
	,,	0·1	490	920	,,	19	,, ,,
	,,	0·01	491	1,200	,,	2	,, ,,

TABLE SHOWING COMPARATIVE INTRAPERITONEAL INOCULATIONS OF PURE SERUM AND GLYCERIN-SERUM CULTURES OF VARIOUS VIRUSES INTO RABBITS.

B. GLYCERIN-SERUM CULTURES—Viruses of Group I.

RABBITS.

Virus.	Immediate Source of Culture.	Dose in Milligrammes	No. of Rabbit.	Weight in grms.	Killed or Died.	Duration of Life in Days.	Result.
H. 29. "M.F."	G.P. 1329 from human cervical glands.	1·	**312**	1,260	Died	73	General tuberculosis of a chronic type.
	,,	0·1	**313**	860	,,	41	General tuberculosis.
	,,	0·01	**314**	670	,,	107	,, ,,
H. 7. "C.M."	G.P. 448 from Calf 5.	10·	**404**	1,850	Died	25	General tuberculosis.
	,,	1·	**405**	1,300	,,	7	,, ,,
	,,	0·1	**406**	1,150	Killed	94	Chronic tuberculosis, rather severe in lungs. (Injection intramuscular and intracœcal).
	,,	0·01	**407**	1,150	Died	94	General tuberculosis of a chronic type.
H. 10. "B S."	G.P. 757 from Calf 113	10·	**401**	1,000	Died	17	General tuberculosis.
	,,	1·	**402**	1,020	,,	39	,, ,,
	,,	0·1	**403**	1,540	,,	26	,, ,,
H. 38. "J.M."	G.P. 793 from human cervical glands.	10·	**492**	1,080	Died	22	General tuberculosis. There seemed hardly enough tuberculosis to account for death.
	,,	1·	**493**	1,150	,,	32	General tuberculosis.
	,,	0·1	**494**	1,230	,,	52	,, ,,
	,,	0·01	**495**	970	Killed (when very ill).	78	General tuberculosis, not severe.
	3rd Culture on Glycerin Serum.						
do. do.	G.P. 793 from O.M.	10·	**549**	1,320	Died	26	General tuberculosis, severe acute type.
	,,	10·	**550**	1,020	,,	25	General tuberculosis.
	,,	1·	**551**	1,170	,,	32	,, ,,
	,,	1·	**552**	950	,,	29	,, ,,

A. SERUM CULTURES—Viruses of Group I.

RABBITS—*continued.*

Virus.	Immediate Source of Culture.	Dose in Milligrammes	No. of Rabbit.	Weight in grms.	Killed or Died.	Duration of Life in Days.	Result.
H. 32. "Y.W." -	Rabbit 166 from human bronchial glands.	10·	497	1,050	Died	32	General tuberculosis. Injection partly intracœcal.
	,,	1·	498	1,220	,,	12	Early general tuberculosis, probably accelerated by psorospermosis.
	,,	0·1	499	1,200	,,	15	General tuberculosis.
	,,	0·01	500	1,050	,,	27	,, ,,
H. 20. "F.L" -	Calf 213, direct.	10·	571	1,810	Died	18	General tuberculosis.
	,,	1·	572	1,430	,,	25	,, ,,
	,,	1·	573	1,480	,,	19	,, ,,
	,,	0·1	574	1,220	,,	25	,, ,,
H. 19. "S.W." -	Goat 11, direct.	10·	607	1,120	Died	9	Tuberculous peritonitis.
	,,	1·	608	820	,,	17	General tuberculosis.
	,,	1·	609	920	,,	6	Death probably from cysticercosis.
	,,	0·1	610	870	,,	24	General tuberculosis.
H. 14. "F.S." -	GP. 587, from human mesenteric glands.	1·	619	690	Died	8	Early tuberculosis; ? cause of death.
	,,	1·	621	940	,,	14	General tuberculosis.
	,,	0·1	620	970	,,	25	General tuberculosis, complicated by cysticercosis.
	,,	0·1	622	670	,,	23	General tuberculosis.
H. 17. "Sp. B." (after increase of virulence.)	GP. 1542, from Calf 553.	10·	599	2,830	Died	13	Early general tuberculosis; ? cause of death.
	,,	1·	600	1,560	,,	19	General tuberculosis.
	,,	1·	601	1,310	,,	81	General tuberculosis. (Injection partly intramuscular.)
	,,	0·1	602	1,360	,,	119	General tuberculosis. (Injection intracœcal).
H. 16. "J.H." - (after increase of virulence).	Calf 337, direct.	10·	445	1,120	Died	11	General tuberculosis.
	,,	1·	446	1,020	,,	18	Early tuberculosis. Death perhaps accelerated by psorospermosis.
	,,	0·1	447	1,370	,,	28	General tuberculosis.
do. do.	Calf 317 direct.	1·	356	1,020	Died	24	General tuberculosis.
	,,	0·1	357	810	,,	26	,, ,,

B. GLYCERIN-SERUM CULTURES—Viruses of Group I.

RABBITS—*continued.*

Virus.	Immediate Source of Culture.	Dose in Milligrammes	No. of Rabbit.	Weight in grms.	Killed or Died.	Duration of Life in Days.	Result.
H 32. "Y.W."	Rabbit 166, from human bronchial glands.	10·	501	1,000	Died	14	Early tuberculosis. Death accelerated by psorospermosis. (Injection partly subcutaneous.)
	,,	1·	502	1,400	,,	14	General tuberculosis.
	,,	0·1	503	1,250		26	,, ,,
	,,	0·01	504	1,180	,,	36	,, ,,
H 20. "F.L."	Calf 213, direct.	10·	575	1,420	Died	16	General tuberculosis.
	,,	1·	576	1,450	,,	63	General tuberculosis. (Injection partly intramuscular.)
	,,	1·	577	1,400	,,	25	General tuberculosis.
	,,	0·1	578	1,280	,,	34	,, ,,
H 19. "S.W."	Goat 11, direct.	10·	611	900	Died	31	General tuberculosis, complicated by cysticercosis.
	,,	1·	612	900	,,	30	Tuberculosis. Death due to other causes.
	,,	1·	613	1,040	,,	76	Chronic tuberculosis of peritoneum and lungs. Kidneys affected slightly. Severe cysticercosis.
	,,	0·1	614	960	Killed	90	Chronic general tuberculosis. (Injection partly intracoecal.)
H 14. "F.S."	G. P. 587, from human mesenteric glands.	1·	623	1,040	Died	16	General tuberculosis, early.
	,,	1·	625	890	,,	15	General tuberculosis.
	,,	0·1	624	920	,,	20	,, ,,
	,,	0·1	626	800	,,	35	,, ,,
H 17. "Sp.B." (after increase of virulence).	G. P. 1542, from Calf 553.	10·	603	1,250	Died	15	General tuberculosis, complicated by cysticercosis.
	,,	1·	604	1,190	,,	22	General tuberculosis.
	,,	1·	605	1,150	,,	18	,, ,,
	,,	0·1	606	1,070	,,	59	,, ,,
H 16. "J.H." (after increase of virulence).	Calf 337, direct.	10·	442	1,020	Died	13	,, ,,
	,,	1·	443	1,420	,,	24	,, ,,
	,,	0·1	444	850	,,	38	,, ,,
do. do.	Calf 317, direct.	1·	358	1,140	Died	19	,,
	,,	0·1	359	1,120	,,	45	,, ,,

ANALYSIS OF THE PRECEDING TABLE,

Showing the comparative virulence for the rabbit (indicated by the duration of life) of pure serum and glycerin-serum cultures.

Dose in milligrammes.	H 29. "M.F."		H 7. "C.M."		H 10. "B.S."		H 38. "J.M."		H 32. "Y.W."		H 20. "F.L."	
	S.	G.S.	S.	G. S.	S.	G.S.	S.	G.S.	S.	G.S.	S.	G.S.
10·	–	–	16	25	21	17	10	22	–	–	18	16
1·	22	?73	37	37	23	39	17	32		14	25–19	25
0·1	30	41	–	–	44	26	19	52	15	26	25	34
0·01	91	107	39	94	–	–	23	78	27	36	–	–
	H 19. "S.W."		H 14. "F.S."		H 17. "Sp.B."† [5th Passage.]		H 16. "J.H."† [3rd Passage.]		H 16. "J.H."† [4th Passage.]		AVERAGE.	
	S.	G.S.	S.	G.S.	S.	G.S.	S.	G.S.	S.	G.S.	S.	G.S.
10·	9	31			13	15	11	13		–	**14**	**20**
1·	17	76	14	16–15	19	22–18		24	24	19	**21·7**	**28**
0·1	24	–	23	20–35	–	59	28	38	26	45	**26·**	**37·6**
0·01		–		–	–	–	–	–	–	–	**45**	**79**

S. = Pure serum cultures. G.S. = Cultures on serum + 5 per cent. of glycerin. † After increase of virulence.

The numbers in the columns headed "S" represent the duration of life in days of rabbits injected with different doses of serum cultures; the opposite numbers in the columns headed "G.S." represents the duration of life of rabbits injected with similar doses of glycerin-serum cultures. The average duration of life of the rabbits inoculated with serum and glycerin-serum cultures respectively in each of the four doses used is also shown.

All those animals in which the tuberculosis was seriously complicated by other disease, or in which there was evidence that the injection had not been entirely intraperitonal, are omitted. This accounts for the gaps in the analysis.

The difference between the virulence of the serum and the glycerin-serum cultures is greater with some viruses than with others. Thus, it is great in the case of H 38 "J.M.," and H 19 "S.W.," which are less dysgonic, while it is least in the very dysgonic viruses, H 10 "B.S.," H 14 "F.S.," and H 16 "J.H." (fourth passage).

GUINEA-PIG INJECTIONS.

Comparative injections with cultures on glycerin-serum and pure serum have been made upon guinea-pigs also. Three viruses of Group I. and three of Group II. were employed for this purpose. The results are given in the following tables.

TABLE SHOWING COMPARATIVE INTRAPERITONEAL INOCULATIONS OF PURE SERUM AND GLYCERIN-SERUM CULTURES OF VARIOUS VIRUSES INTO GUINEA-PIGS.

A. SERUM CULTURES—Viruses virulent for bovines.

GUINEA-PIGS.

Virus.	Immediate Source of Culture.	Dose in Milligrammes	No. of G.P.	Weight in grms.	Killed or Died.	Duration of Life in Days.	Result.
H 38. "J.M"	G.P. 793, from human cervical glands.	1·0	580	440	Died	14	General tuberculosis.
	,,	1·0	581	520	,,	16	General tuberculosis. Death probably hastened by other causes.
	,,	0·1	582	470	,,	21	General tuberculosis.
	,,	0·1	583	500	,,	21	,, ,,
H 32. "Y.W."	Rabbit 166, from human bronchial glands.	1·0	590	250	Died	18	General tuberculosis. (Injection partly intracœcal.)
	,,	1·0	591	230	,,	16	General tuberculosis.
	,,	0·1	592	280	,,	16	General tuberculosis, and septic peritonitis.
	,,	0·1	593	230	,,	15	General tuberculosis.
H 17. "Sp. B." (After increase of virulence).	Rabbit 181, from Calf 539	1·0	707	320	Died	16	General tuberculosis.
	,,	1·0	708	420	,,	13	,, ,,
	,,	0·1	709	530	,,	16	,, ,,
	,,	0·1	710	360	,,	25	,, ,,

TABLE SHOWING COMPARATIVE INTRAPERITONEAL INOCULATIONS OF PURE SERUM AND GLYCERIN-SERUM CULTURES OF VARIOUS VIRUSES INTO GUINEA-PIGS.

B.—GLYCERIN-SERUM CULTURES—Viruses virulent for bovines.

GUINEA-PIGS.

Virus.	Immediate Source of Culture.	Dose in Milligrammes	No. of G.P.	Weight in grms.	Killed or Died.	Duration of Life in Days.	Result.
H 38. "J.M."	G.P. 793, from human cervical glands.	1·0	**584**	440	Died	24	General tuberculosis.
	,,	1·0	**585**	510	,,	22	,, ,,
	,,	0·1	**586**	480	,,	38	,, ,,
	,,	0·1	**587**	560	,,	33	,, ,,
H 32. "Y.W."	Rabbit 166, from human bronchial gland.	1·0	**594**	250	Died	17	General tuberculosis.
	,,	1·0	**595**	370	,,	15	,, ,,
	,,	0·1	**596**	230	,,	20	,, ,,
	,,	0·1	**597**	240	,,	20	,, ,,
H 17. "Sp.B." [After increase of virulence.]	Rabbit 181, from Calf 539.	1·0	**703**	450	Died	14	General tuberculosis.
	,,	1·0	**704**	420	,,	16	,, ,,
	,,	0·1	**705**	380	,,	31	General tuberculosis (injection partly subperitoneal).
	,,	0·1	**706**	390	,,	25	General tuberculosis.

TABLE SHOWING COMPARATIVE INTRAPERITONEAL INOCULATIONS OF PURE SERUM AND GLYCERIN-SERUM CULTURES OF VARIOUS VIRUSES INTO GUINEA PIGS.

A. SERUM CULTURES—Viruses slightly virulent for bovines.

GUINEA-PIGS.

Virus.	Immediate Source of Culture.	Dose in Milligrammes	No. of G.P.	Weight in grms.	Killed or Died.	Duration of Life in Days.	Result.
H 50. "P.H."	G.P. 1490 from human lung.	1·	572	520	Died	21	General tuberculosis.
	,,	1·	573	320	,,	23	,, ,,
	,,	0·1	574	420	,,	23	,, ,,
	,,	0·1	575	380	,,	27	,, ,,
H 25. "A.T."	Calf 417 direct	1·	747	380	Died	20	General tuberculosis.
	,,	1·	748	330	,,	28	,, ,,
	,,	0·1	757	320	,,	31	,, ,,
	,,	0·1	758	310	,,	29	,, ,,
H 26. "K.M."	G.P. 1326 from human kidney.	1·	615	300	Died	22	General tuberculosis.
	,,	1·	616	240	,,	27	,, ,,
	,,	0·1	617	320	,,	27	,, ,,
	,,	0·1	618	250	,,	24	Tuberculous (injection ? partly intracæcal).

TABLE SHOWING COMPARATIVE INTRAPERITONEAL INOCULATIONS OF PURE SERUM AND GLYCERIN-SERUM CULTURES OF VARIOUS VIRUSES INTO GUINEA PIGS.

B. GLYCERIN-SERUM CULTURES—Viruses slightly virulent for bovines.

GUINEA-PIGS

Virus.	Immediate Source of Culture.	Dose in Milligrammes	No. of G.P.	Weight in grms.	Killed or Died.	Duration of Life in Days.	Result.
H 50. "P.H." -	G.P. 1490, from human lung.	1·	**576**	420	Died	29	General tuberculosis.
	„	1·	**577**	480	„	95	General tuberculosis (injection partly subcutaneous).
	„	0·1	**578**	480	„	28	General tuberculosis.
	„	0·1	**579**	360	„	28	„ „
H 25. "A.T." -	Calf 417, direct	1·	**759**	330	Died	28	General tuberculosis.
	„	1·	**760**	480	„	28	„ „
	„	0·1	**762**	360	„	31	„ „
H 26. "K.M." -	G.P. 1326, from human kidney.	1·	**611**	250	Died	25	General tuberculosis. Kidneys affected.
	„	1·	**612**	250	„	36	General tuberculosis.
	„	0·1	**613**	260	„	31	General tuberculosis. Kidneys rather extensively affected.
	„	0·1	**614**	250	„	34	General tuberculosis.

ANALYSIS OF THE PRECEDING TABLES,

Showing the comparative virulence for the Guinea-pig (indicated by the duration of life) of pure serum and glycerin-serum cultures.

A. Viruses Virulent for Bovines.

Dose in milligrammes.	H 38. "J.M."		H 32. "Y.W."		H 17. "Sp. B."* (3rd Passage.)		Average.	
	S.	G.S.	S.	G.S.	S.	G.S.	S.	G.S.
1·0	14 & 16	24 & 22	16	17 & 15	16 & 13	17 & 15	**15·**	**18·3**
0·1	21 & 21	38 & 33	15	20 & 20	16 & 25	20 & 20	**19·6**	**25·**

B. Viruses Slightly Virulent for Bovines.

Dose in milligrammes.	H 50. "P.H."		H 25. "A.T."		H 26. "K.M."		Average.	
	S.	G.S.	S.	G.S.	S.	G.S.	S.	G.S.
1·0	21 & 23	29	20 & 28	28 & 28	22 & 27	25 & 36	**23·5**	**29·2**
0·1	23 & 27	28 & 28	31 & 29	31	27	31 & 34	**27·4**	**30·4**

S. = Pure serum cultures. G.S. = Cultures on serum + 5 per cent. of glycerin. * After increase of virulence.

The numbers in the columns headed "S" represent the duration of life in days of guinea-pigs injected with different doses of serum cultures; the opposite numbers in the columns headed "G.S." represent the duration of life of guinea-pigs injected with similar doses of glycerin-serum cultures. The average duration of life of the guinea-pigs inoculated with serum and glycerin serum cultures respectively in each of the two doses used is also shown. All those animals in which the tuberculosis was seriously complicated by other disease, or in which there was evidence that the injection had not been entirely intraperitoneal, are omitted from the analysis.

In these experiments, as in those with the rabbits, the serum cultures were on the whole a little more rapidly fatal than those on glycerin-serum. This difference is equally marked with the viruses of the two groups.

The difference in the duration of the disease caused by serum and glycerin-serum cultures respectively, both in rabbits and guinea-pigs, is not constant, but the longer survival of the animals injected with glycerin-serum cultures occurred too frequently to be altogether the result of accident, and one must therefore conclude that the injection of bacilli grown upon serum cultures causes a slightly more severe disease than that produced by bacilli from glycerin-serum cultures. The difference, however, is too slight to materially affect the results of the experiments made to ascertain the virulence of our viruses.

Does long-continued Cultivation on Glycerin Media attenuate?

Having shown that glycerin-serum cultures are slightly less virulent than serum cultures, the next point to consider is whether the difference in virulence is due to a difference in the chemical products formed in the two media, or whether it is a true attenuation. If the latter, one would expect attenuation to be progressive, that is, to become more marked the longer the culture was kept on glycerin-serum, and one would not expect any return of virulence if the culture were resown on pure serum. If, however, the difference in virulence be due to a difference in the chemical products of bacterial growth on the two media, one would not expect any further change of virulence after the first glycerin-serum culture, as long as the strain remained on that medium, but one would expect a return of virulence if the strain was again grown upon pure serum.

Experiment with the Virus H 10, "B.S."

With the object of investigating these points, a virulent strain of culture was taken, and a series of cultures started on glycerin-serum.

The glycerin-serum cultures were resown every three weeks, and on every alternate occasion of sowing cultures were also sown on pure serum from the last of the glycerin-serum series. Thus one obtained a series of cultures on glycerin-serum, and another on pure serum after growth for three, five, seven and nine generations on glycerin-serum. The latter were injected, when three weeks old, in measured doses, into rabbits, as were also the first and last cultures of the glycerin series. The strain was grown on glycerin-serum for ten generations and during a period of eight months.

The strain used was that belonging to Virus H.10. "B.S.," and had been obtained from a guinea-pig injected from Calf 113. It had been already grown for thirteen generations on pure serum.

VIRUS H 10. "B.S."

CULTURE DERIVED FROM CALF 113, THROUGH G.P. 757.

[Strain 16½ months old at commencement of experiment]

CULTURES ON PURE SERUM (BOVINE).

Culture 3 weeks old, sown 22-2-05 from pure Serum Culture. Inoculated 15-3-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
298	10·	Died	21	G.T.
297	1·	„	23	G.T.
296	0·1	„	44	G.T.
295	0.01	„	78	G.T.

Sown 17-5-05 on Serum, from the 3rd Culture on Glycerin-Serum. Inoculated 7-6-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
452	10·	Died	37	G.T. (Injection partly intramuscular).
453	1·	„	13	G.T. Death perhaps hastened by Psorospermosis.
454	1·	„	28	G.T.
455	0·1	„	92	Chronic tuberculosis of Lungs. (Injection intra-cœcal).

Sown 30-6-05 on Serum, from the 5th Culture on Glycerin-Serum. Inoculated 21-7-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
525	10·	Died	73	G.T. (Injection probably partly intracœcal).
526	1·	„	53	G.T.
527	1·	„	21	G.T.
528	0·1	„	1	Death due to Psorospermosis.

Sown 7-8-05 on Serum, from the 7th Culture on Glycerin-Serum. Inoculated 28-8-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
555	10·	Died	19	G.T.
556	10·	„	17	G.T.
557	1·	„	24	G.T.
558	1·	„	28	G.T.

Sown 24-10-05 on Serum, from the 9th Culture on Glycerin-Serum. Inoculated 14-11-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
682	10·	Died	80	G.T. (Injection partly subcutaneous).
683	1·	„	29	G.T. Not very severe.
684	0·1	„	29	G.T.
685	0·01	„	43	G.T. (Injection partly intracœcal).

CULTURES ON 5 PER CENT. GLYCERIN-SERUM (BOVINE).

Culture 3 weeks old, sown 27-4-05, from the 1st culture on Glycerin-Serum. Inoculated 18-5-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
401	10·	Died	17	G.T.
402	1·	„	39	G.T. (Injection partly subperitoneal.)
403	0·1	„	26	G.T.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.

Sown 24-10-05 on Glycerin-Serum, from the 9th Culture on the same medium. Inoculated 14-11-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
678	10·	Died	16	Early G.T.
679	1·	„	21	G.T.
680	0·1	„	25	G.T.
681	0·01	„	38	G.T.

G.T. = General Tuberculosis.

ANALYSIS OF THE RESULTS OBTAINED WITH THE VIRUS H 10. "B.S."

Dose in Milligrammes.	Serum Culture Control.	2nd Culture on Glycerin-Serum.	Serum Cultures after 3, 5, 7, and 9 generations on Glycerin-Serum.				10th Culture on Glycerin Serum.
			3	5	7	9	
10	21	17	–	–	19 and 17	–	16
1·	23	–	28	53 and 21	24 and 28	29	21
0·1	44	26	–	–	–	29	25
0·01	78	–	–	–	–	–	–

In the analysis of the preceding table the animals are arranged in transverse lines according to doses. The numbers represent the duration of illness (in days). Therefore, if there were any attenuation, the duration of illness should increase as any given line was read from left to right.

The experiment shows that even after having been grown for ten generations on glycerin-serum, and during a period of 8 months, there was no increase in the duration of illness and therefore no attenuation. All those animals in which the tuberculosis was seriously complicated by other disease, or in which there was evidence that the injection had not been entirely intraperitoneal, are omitted. This accounts for the gaps in the analysis.

EXPERIMENT WITH THE VIRUS H 7. "C.M."

A similar experiment was made with the virus H.7, "C.M." It differs from the foregoing in the following particulars. Each culture of the glycerin series was injected into rabbits, and from it a pure serum culture was made, and this also was injected into rabbits. Thus we have a double series of rabbit injections, one with cultures on glycerin-serum, and the other with cultures on pure serum, each after a longer or shorter period of growth upon glycerin-serum.

The strain used for this experiment was that obtained from Calf 5 through Guinea-pig 448, and the series of glycerin-serum cultures was commenced from the 15th culture on pure serum.

VIRUS H. 7 "C.M."

CULTURE DERIVED FROM CALF 5, THROUGH G.P. 448.

[Strain 25 months old at commencement of experiment]

Rabbits inoculated intraperitoneally.

CULTURES ON PURE SERUM (BOVINE).

Culture 3 weeks old, sown 28-4-05, from pure Serum Culture. Inoculated 19-5-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
408	10·	Died	16	G.T.
409	1·	,,	37	G.T.
410	0·1	Killed	94	Chronic tuberculosis of lungs and kidneys. (Injection intramuscular and intracœcal.)
411	0·01	Died	39	G.T.

CULTURES ON 5 PER CENT. GLYCERIN-SERUM (BOVINE)

Culture 3 weeks old sown, 28-4-05, from pure Serum Culture. Inoculated 19-5-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
404	10·	Died	25	G.T.
405	1·	,,	37	G.T.
406	0·1	Killed	94	Chronic tuberculosis rather severe in lungs. (Injection intramuscular and intracœcal.)
407	0·01	Died	94	G.T. of a chronic type.

Sown 19-5-05, from Glycerin-Serum Culture of 28-4-05.

Inoculated 9-6-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
460	10·	Died	12	G.T.
461	1·	,,	6	Very early tuberculosis of omentum. Few T.B. in lungs. Death from other causes.
462	0·1	,,	27	G.T.
463	0·01	,,	47	G.T.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
464	10·	Died	12	G.T.
465	1·	,,	31	G.T. Lungs more particularly affected.
466	0·1	,,	33	G.T.
467	0·01	,,	47	G.T.

Sown 9-6-05, from Glycerin-Serum Culture of 19-5-05.

Inoculated 1-7-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
505	10·	Died	11	G.T.
506	1·	,,	94	G.T. (Injection intramuscular.)
507	0·1	,,	39	G.T.
508	0·01	,,	74	G.T. of a chronic type.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
509	10·	Died	37	G.T.
510	1·	,,	16	Very slight tuberculosis. Cause of death not determined.
511	0·1	,,	52	G.T.
512	0·01	,,	98	G.T.

Sown 1-7-05, from Glycerin-Serum Culture of 9-6-05.

Inoculated 22-7-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
533	10·	Died	11	Early G.T.
534	1·	,,	28	G.T.
535	0·1	,,	54	G.T.
536	0·01	,,	1	Psorospermosis.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
529	10·	Died	5	Early tuberculous peritonitis. Death due to psorospermosis.
530	1·	,,	21	G.T.
531	0·1	,,	77	G.T.
532	0·01	,,	1	Psorospermosis.

Sown 22-7-05, from Glycerin-Serum Culture of 1-7-05.

Inoculated 12-8-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
545	10·	Died	16	G.T.
546	1·	,,	87*	G.T.
547	0·1	,,	6	Psorospermosis.
548	0·01	,,	94	G.T.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
541	10·	Died	25	G.T.
542	1·	,,	30	G.T.
543	0·1	,,	49	G.T.
544	0·01	,,	142	Chronic Tuberculosis.

Sown 12-8-05, from Glycerin-Serum Culture of 22-7-05.

Inoculated 2-9-05.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
579	10·	Died	19	G.T.
580	1·	,,	33	G.T.
581	1·	,,	48	G.T., chronic; injection partly intramuscular.
582	0·1	,,	59	G.T. chronic.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
583	10·	Died	17	G.T.
584	1·	,,	46	G.T. (injection partly intramuscular.)
585	1·	,,	19	G.T.
586	0·1	,,	4	Psorospermosis.

* Needle became detached during injection, and the greater part of the dose was lost. NOTE.—"G.T." = General Tuberculosis.

ANALYSIS OF THE RESULTS OBTAINED WITH THE VIRUS H 7. "C.M."

SERUM CULTURES.

Dose.	Serum Culture [Control].	Serum Cultures after 1, 2, 3, 4 and 5 generations on Glycerin Serum.					Average of the last 5 columns.
		1.	2.	3.	4.	5.	
10 mg.	16	12	11	11	16	19	13·8 days.
1 mg.	37	–	–	28	–	33	30·5 „
0·1 mg.	–	–	39	54	–	59	50·7 „
0·01 mg.	39	47	74	–	–	–	60·5 „

GLYCERIN-SERUM CULTURES.

Dose.	Generation of Culture on Glycerin Serum.						Average.
	1st.	2nd.	3rd.	4th.	5th.	6th.	
10 mg.	25	12	37	–	25	17	23·2 days.
1 mg.	37	31	–	21	30	19	27·6 „
0·1 mg.	–	33	52	77	49	–	52·7 „
0·01 mg.	94	47	98	–	–	–	79·7 „

The analysis is arranged like the last. All cases complicated by severe psorospermosis or other intercurrent disease are omitted.

It will be observed that the difference in duration of illness between the rabbits which received glycerin-serum and those which received pure serum cultures is far more marked in those which received 10 mg. doses than in those which received smaller quantities. This in itself is in favour of the difference being due to a difference in toxic products. But further it will be seen that no *progressive* diminution of virulence occurred on glycerin-serum and that the serum cultures which were sown from the series of glycerin-serum cultures were as virulent as the original ones which were sown from the strain which had always grown on pure serum.

EXPERIMENT WITH THE VIRUS H 28 'C.L."

If any further evidence is needed to show that glycerin in the culture medium does not attenuate, it is afforded by an experiment with the virus H.28, "C.L." A strain derived from human cervical glands through rabbit 102 was sown on serum and glycerin-serum, and parallel series of cultures on these media were kept growing for over a year. At the end of this period serum cultures were sown from both, and calves and rabbits injected with them. Each strain was injected into two calves in doses of 10 mg. One of these animals unfortunately was found at death to be suffering from obstruction of the ureters. This somewhat interferes with the comparison but it may be stated that each strain proved fatal to one calf, and caused but slight tuberculosis in the other.

In the case of the rabbits the serum culture from the glycerin-serum series actually killed three out of four animals a little quicker than did that sown from the serum series; but this was doubtless a coincidence. One may, however, conclude that the experiment afforded no evidence whatever of the supposed attenuating power of glycerin.

VIRUS H 28. "C.L."

Strain of culture derived from human cervical glands through Rabbit 102.

CALVES.

SERUM SERIES.

After growing for one year on pure Serum.

A 21-day old serum culture was used for injection.

No. of Calf.	Dose in mg.	Result.	
863	10·	Killed when well, after 90 days	G.T. slight.
877	10·	Dying* „ 37 „	G.T. severe.

GLYCERIN-SERUM SERIES.

After growing for one year on Glycerin-Serum.

A 21-day old serum culture, sown from a glycerin-serum culture, was used for injection.

No. of Calf.	Dose in mg.	Result	
875	10·	Killed when well, after 91 days	Slight tuberculosis
819	10·	Died „ 52 „	G.T. severe.

* Complicated by obstruction of ureters by calculi.

RABBITS.

SERUM SERIES.

After growing for one year on pure Serum.

A 21-day old serum culture was used for injection.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
559	10·	Died	16	G.T.
560	1·	„	23	G.T.
561	1·	„	23	G.T.
562	0·1	„	31	G.T.

GLYCERIN-SERUM SERIES.

After growing for one year on Glycerin-Serum.

A 21-day old serum culture, sown from a glycerin-serum culture, was used for injection.

No. of Rabbit.	Dose in mg.	Killed or Died.	Duration of Life in Days.	Result.
563	10·	Died	12	G.T. acute.
564	1·	„	24	G.T.
565	1·	„	20	G.T.
566	0·1	„	29	G.T.

SUMMARY.

There can, I think, be no doubt that when grown on pure serum, cultures of viruses belonging to Group I. on the whole prove rather more rapidly fatal to rabbits and guinea-pigs than when they have been grown on serum with glycerin. The difference is most marked with large doses. It is neither progressive nor permanent; strains grown for months or a year on glycerin-serum are no less virulent than the same strain sown for one generation only on that medium. Transference to pure serum restores the original virulence. Therefore, one must conclude that glycerin causes no true attenuation of the bacilli; and that the reason that, dose for dose, serum cultures are a little more rapidly fatal than glycerin-serum cultures is, in all probability, because the latter are deficient in some toxic product which is present in the former.

Note.—G.T. = General Tuberculosis.

REPORT ON THE MINUTE NON-PROGRESSIVE TUBERCLES FOUND IN CALVES INJECTED WITH TUBERCLE BACILLI OF HUMAN ORIGIN WHICH ARE UNABLE TO PROVOKE A PROGRESSIVE INFECTION IN THIS SPECIES OF ANIMAL.

Dr. L. COBBETT.

BLYTHWOOD

REPORT ON THE MINUTE NON-PROGRESSIVE TUBERCLES FOUND IN CALVES INJECTED WITH TUBERCLE BACILLI WHICH ARE UNABLE TO PROVOKE A PROGRESSIVE INFECTION IN THIS SPECIES OF ANIMAL.

In addition to the lesions* constantly found in calves subcutaneously injected with viruses of human group ii, namely, those incapable of causing progressive tuberculosis in these animals there were found on close inspection often one or more, rarely many, minute tubercular foci in the larger viscera or in the lymphatic glands.

In this Report these little tubercles are described, and their incidence among the injected calves is analysed with the object of elucidating their significance. Two questions present themselves :

(1) Are they characteristic of certain viruses, which are thus shown to be of somewhat higher virulence than other viruses which do not produce them ?

(2) Or if not, what determines their presence in some calves, and their absence from others ?

These minute tuberculous foci appeared in the lymphatic glands as opaque, ochre-coloured, calcareous points. They were usually few in number, but in one or two instances they were rather numerous. (*e.g.* in Calf 619, H. 12, H .N.).

Among the great organs they were most commonly found in the lungs, and there appeared as translucent grey points, darker than the surrounding tissue. They were often so small that they were not easily seen, and sometimes they were only found after the lungs had been allowed to dry a little. At most they were as large as a small pin's head. They were either entirely translucent, looking like little specks of glass, or they had a minute central opacity. They were hard, and could easily be picked out on the point of a knife where they appeared as sharply defined spherical bodies resembling little beads of glass, or rather transparent pearls. When crushed on a cover glass they could be distinctly felt to crack.

When these little bodies appeared in the liver or spleen, which they did but rarely, they were similar to those in the lung. Only they then appeared as pale bodies on a darker ground, owing, I suppose, merely to contrast.

They were usually few in number; often two or three only were found in a single organ; but sometimes they were more numerous, and in four instances they occurred in somewhat large numbers.

That these bodies were tuberculous in origin cannot be doubted, because tubercle bacilli were sometimes found in them. That they were not early tubercles capable of progressing there can equally be no doubt, for they were found months after injection in calves which were obviously doing well, and had long ago recovered from any disturbance of health caused by the injection. Histological examination has shown that they were not in an active stage of formation; and there can be no doubt that they were tubercles which had very early commenced to retrogress.

This was not at first clearly recognised, and in one or two calves in which they occurred in relatively large numbers, notably in Calf 365, H. 23, J.P., Calves 165 and 405, H. 18, T.T., and Calf 185, H. 9., C.T., they were mistaken for progressive tubercles in an early stage. Further experience during the last two years has convinced me that I was in error in so thinking, and a recent examination of the preserved specimens has shown how very minute the lesions were in even the more severe cases.

(*) These consisted of a caseous and softened local lesion enclosed in a fibrous capsule, and of calcareous nodules in the nearest lympathic glands ; often of nothing more. These caseous abscesses often ulcerated through the skin and discharged their content. The latter must have often been swallowed by the calves, and yet lesions attributable to this cause were seldom seen. The lesions in Calves 473 and 481, H. 30, E.M., may have had this origin.

These four calves are therefore now regarded by me as showing in their organs the minute lesions of retrogressive type, so frequently seen in calves injected with viruses which are not capable of causing in them a progressive tuberculosis, and only exceptional in possessing them in unusual numbers.

The origin of these little tubercles seems to be attributable to bacilli which passively escape from the seat of subcutaneous injection. For the evidence of this the report on the distribution of non-virulent tubercle bacilli in the body of the calf after subcutaneous injection may be consulted [*See* p. 219]. In that Report it is shown that after subcutaneous injection, even of viruses incapable of causing a progressive infection, tubercle bacilli get distributed all over the body, and that, as a rule, they disappear without leaving any sign of their former presence. Occasionally, however, the minute translucent foci are found three months after injection, and may even then contain a few tubercle bacilli.

These little tubercles are evidence that the disease has made some amount of progress at a number of foci distant from the seat of injection, however completely that progress may have ceased before the animal came to be examined. The question then may be put thus : Is this short-lived development of tubercles in glands and organs rendered possible by any special quality of virulence on the part of the bacilli, or by some peculiar susceptibility of the individual animals in which they appear ? If the former, then one would expect these little tubercles to occur after the injection of certain viruses rather than of others. If the latter then one would expect the calves in which they occur to be distributed equally among all the feebly virulent viruses ; or rather as equally as the laws of probability will allow.

To answer this question then one needs to know whether any of the feebly virulent viruses are conspicuous among others for their power to cause these little tubercles, or whether all feebly-virulent viruses alike are liable to produce them. Accordingly a table has been drawn up including all the calves which have up to the present time shown these lesions, and a list giving for each virus the proportion of animals so affected to the total number injected subcutaneously with it.

In the table the tubercles in the organs are distinguished from those in the glands, and among the latter, the thoracic, mesenteric, and other lymph glands distant from the seat of injection are recorded separately. Moreover, the numbers of tubercles in each of these groups is roughly indicated : thus two or three or very few such foci are indicated by a single x, moderate and large numbers by x x and x x x respectively.

The table shows that these little tubercles have occurred in one or more of these situations and in larger or smaller numbers in nearly half the calves injected subcutaneously with viruses of group ii, and that these calves are widely distributed. All the viruses which have never produced progressive tuberculosis in bovine animals have caused these lesions, with the exception of seven (out of thirty-one). With each of these no more than two calves were used ; when larger numbers were injected with one virus some have always been affected, and the ratio of these to those which escaped is remarkably constant.

If we separate the viruses according to their seat of origin in the human being we find that no category is particularly distinguished for producing them, and that instances in which they were especially numerous occur among the lung, mesenteric gland, and joint viruses.

On the other hand it is only right to point out that the incidence of these little tubercles is rather greater among the calves injected with cervical and mesenteric gland

viruses than among those injected with lung or joint viruses; and these latter, it will be remembered, have, unlike the former, never yet yielded one virulent for cattle. But the difference is but slight (41 and 42·4 as compared with 50 per cent.), and it may be doubted whether this is more than accidental. On the whole, then, the conclusion seems justified that these little tubercles are not evidence of a superior grade of virulence in the virus that produced them.

The next question is: Why do these little tubercles occur in some animals and not in others injected with the same virus? The explanations which suggest themselves, namely, variations of dose, and individual susceptibility, and some accidental peculiarity of the mode of injection, will now be considered.

Dose we may dismiss at once, because the lesions have been caused by all kinds of doses, having followed such small ones as 13,000 and 10,000 bacilli in the case of H. 16 "J.H." and H. 12 "H.N." and having been absent after such colossal doses as 5,000 million (H. 16 "J.H"), 3,000 million (H 12 "H.N."), as well as 10,000 and 5,000 million (H. 26 "K.M."), and 5,000 million (H. 8 "S.C.").

As to individual susceptibility, there can, I think, be no doubt that great differences occur among calves. Of this there is a large amount of evidence, and I will only mention now the results of the 10 m.g. culture injections, which, in the case of two calves injected with the same virus, have often proved rapidly fatal to one, and left the other to live out its three months, without serious illness, and with a very moderate amount of disease at the end of this period. These injections were made with virulent viruses, but it seems reasonable to suppose that the same differences of susceptibility would come into play in the case of the feebly-virulent viruses also. (*)

But while differences of individual susceptibility appear as the most probable explanation of these little tubercles, I think I ought to point out that there are differences also in the circumstances of subcutaneous injections which may be not without influence. The possibility of some of the material entering a vein at the moment of injection cannot be seriously entertained. I desire rather to refer to differences which depend on the exact depth of the injection. Thus on occasions the injection has been made partly into the muscle, and even when this is not the case, differences in the plane of subcutaneous connective tissue into which the fluid enters may cause great differences in the amount of tension to which it is subjected. Thus, for example, at times one experiences a good deal of resistance to the entry of fluid, and in such cases the little swelling which results is prominent, circumscribed, and very tense; at other times there is little or no resistance and the swelling is flat and soft. Again the fluid may run up a considerable distance from the point of the needle, forming an elongated cord-like swelling. This happens, I think, when the needle enters the sheath of the subjacent muscle. In these varying circumstances it does not seem unreasonable to think that the difference of tension, at least, may make some difference to the passive absorption and consequent dissemination of tubercle bacilli.

However this may be, it seems obvious that each of these little tubercles must develop at a spot where a considerable number of tubercle bacilli have become arrested, for the report, on the distribution of bacilli already referred to, made it clear that the little tubercles by no means always develop wherever the bacilli have wandered to. Considerable numbers of bacilli at a single spot seem only explicable on the hypothesis that a phagocyte with a large cargo of them had run aground there. Probably wherever isolated bacilli, or small groups are arrested they are destroyed. But in the case of these larger aggregations some development of new tissue (tubercle formation), takes place before the invaders are destroyed. Thus the existence of these tubercles would appear to depend upon the presence of overloaded phagocytes, together with the inability of the animal to promptly destroy the bacilli. Whatever be the significance of the former, the latter is the result of some defect of the protective functions of the body. But it is hard to say which is the dominant factor.

LOUIS COBBETT.

(*) A fuller statement of the evidence of differences of individual susceptibility will be found in the Report on the influence of (i) the variation in the capacity for resistance to tuberculosis of different individual animals, and (ii) the number of tubercle bacilli injected, on the severity of an experimental infection [*See* p. 247].

LIST OF CALVES INJECTED SUBCUTANEOUSLY WITH VIRUSES OF GROUP II., IN WHICH LITTLE NON-PROGRESSIVE TUBERCLES WERE FOUND.

X = One or more minute foci. XX = Fairly numerous minute foci. XXX = Very numerous minute foci.

Virus. [The probable primary seat of disease in man is indicated in brackets].	No. of Calf.	Tissue Emulsion or Culture.	Dose.	Non-progressive minute Tubercles in			
				Organs.	Thoracic Glands.	Mesenteric Glands.	Other distant Glands.
H. 17. "Sp.B." † [Lung] -	265	T.	50,000	X	X		
	277	T.	76,000	X	X		
	391	T.	41,600,000	X	X		
	601	C.	50 mg.			X	
H. 22. "F.W." [Lung] - -	359	T.	3,204,000		X		
	439	T.	100,000,000		X		
	755	C.	50mg.	X			
	759	C.	50mg.	X			
H. 23. "J.P." [Lung] - -	365	T.	103,093,000	XXX	X	X	X
	421	T.	8,166,000	X	X	X	
	625	C.	50mg.	X	X	X	X
H. 25. "A.T." [Lung] - -	449	T.	87,273,000		X		X
H. 50. "P.H." [Lung] - -	843	C.	50mg.	X		X	
H. 51. "H.M." [Lung] - -	833	C.	50mg.	X			
H. 45. "F.M." [Bronchial gland]	763	C.	50mg.	X		X	
H. 35. "C.B." [Bronchial gland]	605	C.	50mg.	X	X	X	
H. 27. "B.D." [Cervical gland]	675	C.	50mg.	X			
	677	C.	„	X			
H. 37. "O.J." [Cervical glands]	627	C.	50mg.	X			
	629	C.	„			X	
H. 44. "D.C." [Cervical glands]	705	C.	50mg.	X		XX	
	731	C.	„		X	XX	XX
H. 33. "R.T." [Cervical glands]	595	C.	50mg.	X			

† Before increase of virulence.

List of Calves Injected Subcutaneously with Viruses of Group II. in which Little Non-progressive Tubercles were found—*continued.*

Virus. [The probable primary seat of disease in man is indicated in brackets.]	No. of Calf.	Tissue Emulsion or Culture	Dose.	Non-progressive minute Tubercles in			
				Organs.	Thoracic Glands.	Mesenteric Glands.	Other distant Glands.
H. 8. "S.C." [Mesenteric glands].	275	T.	12,550,000		X		
	323	T.	„		X		
	423	T.	6,146,000		X		
	657	C.	50mg.	X			
	673	C.	50mg.	X			
H. 12. "H.N." [Mesenteric glands].	123	T.	13,000	X	X		X
	135	T.	„	X	X	X	
	287	T.	103,968,000	X	X	X	X
	329	T.	2,766,000		X		
	351	T.	3,140,266,000		X	X	
	619	C.	50mg.		X	XX	XX
H. 18. "T.T." [Mesenteric glands].	165	T	4,479,000	XXX	X	X	
	405	T	269,917,000	XXX	T.B.	T.B	
	429	T	2,814,000,000	X		X	
	645	C	50 mg.	X			
H. 30. "E.M." [Mesenteric gland].	473	T	4,756,800,000			XXX	
	481	T	„	X	X	XXX	X
	593	C	50 mg.	X	X		
H. 36. "M.D." [Mesenteric glands].	687	C	50 mg.	XXX		X	
H. 46. "H.W." [Knee-joint]	777	C	50 mg.			X	
H. 47. "S.B." [Hip-joint]	803	C	50 mg.	X		X	
H. 9. "C.T." [Wrist joint]	183	C	¾ c.c. of bacilli.		X		
	185	C	„	XXX	X		X
	201	T	3,883,000		X		
	331	T	133,824,000	X		X	X
	733	C	50 mg.			X	
	737	C	50 mg.	X	X	X	
H. 11. "E.D." [Elbow joint]	307	T	479,733,000	X	X		
	723	C	50 mg.				

List of Calves Injected Subcutaneously with Viruses of Group II. in which Little Non-progressive Tubercles were found—*continued.*

Virus. [The probable primary seat of disease in man is indicated in brackets].	No. of Calf.	Tissue Emulsions or Culture.	Dose.	Non-progressive minute Tubercles in			
				Organs.	Thoracic Glands.	Mesenteric Glands.	Other distinct Glands.
H. 15. "I.W." [Ankle-joint]	193	T.	3,761,000	X	X		
	195	T.	„		X		
	367	T.	68,166,000	XX	X	X	
	413	T.	136,960,000		X	XX	
H. 16. "J.H."* [Knee-joint]	157	T.	10,000		X		
	527	C.	50mg.			X	
H. 26. "K.M." [Kidney]	437	T.	4,936,000	X	X		
	865	C.	50mg.	X		X	
	871	C.	„	X		X	X

* Before increase of virulence.

TABLE SHOWING THE DISTRIBUTION OF THE CALVES WITH DISSEMINATED NON-PROGRESSIVE TUBERCLES AMONG THE DIFFERENT VIRUSES INCAPABLE OF CAUSING PROGRESSIVE TUBERCULOSIS IN BOVINE ANIMALS (GROUP II).

Virus. Name.	Primary in.	Ratio of Number of Calves with disseminated tubercles to total number injected with each virus.	
H. 17. "Sp.B."*	Lung	4 out of 10	14 out of 33, or 42·4 per cent.
H. 22. "F.W."	„	4 „ 7	
H. 23. "J.P."	„	3 „ 7	
H. 25. "A.T."	„	1 „ 4	
H. 48. "W.P."	„	0 „ 2	
H. 51. "H.M."	„	1 „ 1	
H. 50. "P.H."	„	1 „ 2	
H. 45. "F.M."	Bronchial gland	1 „ 2	2 out of 4, or 50 per cent.
H. 35. "C.B."	„	1 „ 2	
H. 27. "B.D."	Cervical gland	2 „ 4	7 out of 14, or 50 per cent.
H. 37. "O.J."	„	2 „ 2	
H. 34. "C.U."	„	0 „ 2	
H. 39. "M.B."	„	0 „ 2	
H. 44. "D.C."	„	2 „ 2	
H. 33. "R.T."	„	1 „ 2	
H. 8. "S.C."	Mesenteric glands	5 „ 10	19 out of 38, or 50 per cent.
H. 12. "H.N."	„	6 „ 11	
H. 18. "T.T."	„	4 „ 9	
H. 30. "E.M."	„	3 „ 4	
H. 36. "M.D."	„	1 „ 4	
H. 42. "M.R."	Joint	0 „ 2	16 out of 39, or 41 per cent.
H. 43. "F.F."	„	0 „ 2	
H. 46. "H.W."	„	1 „ 1	
H. 47. 'S.B."	„	1 „ 2	
H. 41. "A.S."	„	0 „ 2	
H. 9. "C.T."	„	6 „ 7	
H. 11. E.D."	„	2 „ 6	
H. 15. "I.W."	„	4 „ 9	
H. 16. "J.H."*	„	2 „ 8	
H. 40. "J.G."	Testicle	0 „ 2	
H. 26. "K.M."	Kidney	3 „ 6	

TOTAL INCIDENCE RATIO—ALL VIRUSES: 60 out of 136, or 44·1 per cent.

* Before increase in virulence.

REPORT ON THE DISTRIBUTION OF NON-VIRULENT TUBERCLE BACILLI OF HUMAN ORIGIN IN THE BODY OF THE CALF AFTER SUBCUTANEOUS INJECTION.

Dr. L. COBBETT.

REPORT ON THE DISTRIBUTION OF NON-VIRULENT TUBERCLE BACILLI IN THE BODY OF THE CALF AFTER SUBCUTANEOUS INJECTION.

The dissemination of a disease such as tuberculosis throughout the body from a focus of infection has generally been pictured as an active invasion by the micro-organisms, which in virtue of their virulence break down step by step the successive barriers which are raised against them. This report produces evidence of another kind of dissemination, and describes a passive escape of non-virulent tubercle bacilli from the seat of a subcutaneous injection in the calf, occurring very shortly after that injection, and a wide distribution of the bacilli throughout the body, followed either by their complete disappearance, or by minute lesions only which show no tendency to progress.

These minute lesions have been described in the preceding report, and I need not say more here than that they were either fibrous and very translucent, or calcareous, and so small as to be easily overlooked. They were therefore undoubtedly retrogressive.

Now, these little tubercles, occurring from time to time in the organs of calves which were obviously doing well, were a source of perplexity to me, for I could not understand how the bacilli could have got to these situations unless they possessed the virulence which should have enabled them to cause progressive lesions. Their presence conflicted with the belief that the severity of an infection caused by tubercle bacilli could be measured by the wideness of distribution of the lesions which they produced. For in these cases the distribution had obviously been wide and yet the disease had not progressed, the animals had recovered from any transient disturbance of health from which they once suffered, and had remained well, and grown normally.

Various hypotheses suggested themselves—the bacilli were perhaps in these cases of somewhat higher virulence than the average of the non-virulent class; or the animal exceptionally susceptible. But neither hypothesis reconciled the wideness of distribution with the arrest of progress. Either non-virulent bacilli had succeeded in distributing themselves over the body, or virulent bacilli had become arrested in their activity after they had succeeded in spreading themselves. If some sudden accession of resistent power were known to occur in tubercle as in pneumonia, this latter hypothesis would not have appeared improbable, but in tuberculosis such augmentation of resistance is unknown, and moreover other evidence showed that the viruses in question were of the ordinary non-virulent kind. Gradually it came to appear more and more probable that the dissemination of tubercle bacilli over the body in our experimental animals is not necessarily the active progressive invasion that it has been pictured, but might take place also by a passive absorption, which probably occurs before a barrier of granulation tissue has been built around the bacilli at the seat of injection, and that in the case of the bacilli injected being of feeble virulence for the animal, they either entirely disappear, or where most numerous cause evidence of their presence in a few little glassy or calcareous foci.*

It remained to put this theory to the test of experiment. Now it is obvious, if tubercle bacilli escape in numbers from the seat of injection, and after dissemination about the body disappear without causing any serious lesions, that one must expect the tubercle bacilli to be more numerous in the organs and tissues at some period not long after the injection than at the time when these animals are usually killed, namely three months later. This was actually found to be so in the case of Calf 769, which died of diarrhœa seventeen days after the subcutaneous injection of 50 mg. of a culture of the group II. virus H. 45, "F.M." At the post-mortem examination of this animal an inflammatory patch was found at the seat of injection, and the prescapular and prepectoral glands on the same side were swollen but showed no distinctive signs of tubercle. A scraping from each however, revealed numerous tubercle bacilli, while some were found also in the posterior-thoracic gland, which was normal in appearance. Four films from the lungs were searched but no tubercle bacilli seen. The full notes of the post-mortem examination of this and other calves mentioned here will be found on pages 229–233. At this stage I consulted with Dr. Eastwood, and asked him to co-operate with me in the investigation. To this he cordially agreed, and it was arranged that he should examine histologically the various tissues and organs of the calves while I injected tissue emulsions into guinea pigs. It was obvious that my part of the work could only be expected to show the presence or absence of bacilli in the tissues investigated, and would reveal but little as to their numbers; and it was hoped that the histological investigation would throw light on the method of dissemination which we expected to demonstrate, and by following the numbers of bacilli found in various organs of calves injected simultaneously and killed in succession, give valuable information as to the rise and fall of this dissemination of bacilli, and the relation between their escape from the seat of injection and the development of the local tumour.

Experiments with Virus H. 46.

At this time there happened to be two calves which had recently been injected subcutaneously with 50 mg. of the non-virulent virus H. 46, "H.W." (from a case of joint tuberculosis). In order to get to work as soon as possible it was decided to kill one of these animals about three weeks after injection, when the febrile reaction and the enlargement of the prescapular gland might be expected to be near their maximum, leaving the other as a control, to be killed and examined at the expiry of the usual three months interval.

The first of these calves, No. 767, was accordingly killed on May 23, 1905, twenty-five days after injection, while the second, No. 777, was killed on July 27, and their various organs and tissues were examined in the way agreed upon. Dr. Eastwood's report on this and the other calves which bear on the question under consideration will be found in Volume IV., pp. 13–19. It remains for me to describe the gross lesions of the calves, and the result of injecting the tissue emulsions into guinea-pigs, and as the former are fully given at the end of this report, and the latter clearly shown in the table which follows, it will be unnecessary to give any more than a brief description here.

These two calves, Nos. 767 and 777, had been subcutaneously injected with 50 mg. of three weeks' old serum culture on the right side of the neck on April 28th, 1905.

No. 767 was killed on May 23rd, twenty-five days after injection, when the febrile reaction was just over and the enlargement of the prescapular gland probably at its height. At the postmortem examination there was found a large flat tumour at the seat of injection, caseo-necrotic, and containing a large irregular cavity. It had not however begun to soften in its periphery. It was closely attached to the subjacent muscle and slightly infiltrated the skin. The right prescapular gland was enlarged and consisted partly of a grey translucent tissue and partly of a caseating network. The right prepectoral gland was slightly affected. On the surface of the lungs rather numerous minute translucent grey points could be seen. Some of these had a reddish tinge, and it was

* At one time one questioned whether in these cases some of the injected material might not have got into a torn vein at the moment of injection, but the repeated occurrence of such cases made this suggestion unthinkable.

difficult to distinguish them from the minute hæmorrhages which are of frequent occurrence, even in normal animals. Some were undoubtedly tubercles of the kind frequently found in these cases, for they could be picked out on the point of a knife. Scarcely any trace of opacity could be seen in the centres of any of them. In the liver and kidneys one or two pale specks on the surface were the nearest approach to tubercles which could be seen. All other parts appeared perfectly normal.

Emulsions were made from liver, spleen, lung, and rib marrow, all but the last being rich in tissue, and 1·0 cc. of each was injected into guinea-pigs. Table showing the results of these and similar inoculations will be found at the end of this report.

All the guinea-pigs were found to be tuberculous when killed seven weeks later. One alone, injected with marrow, had tuberculosis of the omental gland only, all the rest had general tuberculosis. There was obviously then a very wide distribution of tubercle bacilli in this calf, involving the bone marrow as well as the principal organs, far wider than the visible lesions would have led one to suppose.

The control Calf, No. 777, was killed ninety days after injection. The lesion at the seat of injection consisted of a fibrous walled cavity with fluid contents, and the prescapular gland on the same side contained two large fibro-calcareous nodules. In several of the mesenteric glands were some small calcareous foci. There was no visible trace of tubercle elsewhere, or vestige of the previous presence of bacilli, unless indeed some little areas of collapse in the lung were of this kind.

Guinea-pigs were injected with emulsions of the same parts as before, care being taken to inject even larger doses. The guinea pigs which received emulsions of spleen, liver, and rib marrow intraperitoneally were perfectly healthy when killed forty-two days later, while those injected with the lung emulsion were found to be tuberculous.

Experiment with Virus H. 26.

The next experiment was made with the virus H 26, "K.M." (obtained from a human kidney), which had proved itself non-virulent for calves and rabbits, and of which the enormous dose of 10,000 million bacilli had already been injected into a calf with only minimal results.

On July 25, 50 mg. of three weeks' old serum culture was injected into each of five calves subcutaneously on the right side of the neck.

No. 873 was killed *three* days later. There was a small soft swelling at the seat of injection, the subcutaneous tissue being a little thicker and firmer than normal, and the surrounding tissue a little œdematous. The prescapular gland on the same side was somewhat swollen and œdematous; everything else appeared perfectly normal.

Some 3 or 4 c.c. of blood from the left iliac vein were injected into a guinea pig, without producing tuberculosis. Tissue emulsions of spleen, liver, and lung, as well as of bone marrow and popliteal gland (taken as a representative of the peripheral lymphatic glands) caused tuberculosis in the guinea pig. Only in the case of the animal injected with the popliteal gland was the tuberculosis slight, in all others it was general. The doses were large, and the emulsions with the exception of that of the bone marrow were rich in tissue.

Calf 881 was killed *eight* days after the injection. There was a small swelling at the seat of injection chiefly caused by a thickening of the subcutaneous tissue, which was firm and grey and infiltrated with shot-sized spots of softened matter resembling muco-pus. The right prescapular gland was somewhat swollen, but showed no sign of tubercle. All other parts were normal.

The tissue emulsions of this calf which were injected into guinea pigs were more numerous than before, and a larger dose of blood was given. The only guinea pig which escaped tuberculosis was one of two which were injected with an emulsion of rib marrow, the other was tuberculous. That which received 11 c.c. of blood from the heart was found when killed to have six tubercles in its spleen. Those injected with emulsion of spleen, liver, lung, kidney, thoracic gland, and popliteal gland, all were found to have general tuberculosis. The emulsions, with the exception of that of the marrow, were rich in tissue, and large doses were given.

The third calf, No. 869, was killed *fourteen* days after injection. It had remained quite well, but its temperature had risen a little, (to 40·2°C.) on the 13th day. It had gained 9lbs. in weight.

At the seat of injection was a tumour which measured 4 × 3 × 1¼in. In its centre was an irregular cavity containing muco-purulent material around which was dense œdematous tissue; the subjacent muscle was not infiltrated, but the skin over the tumour was distinctly altered in appearance. The prescapular gland was considerably enlarged. It was firm, elastic and œdematous, but there was no appearance characteristic of tubercle. The prepectoral was as large as a large pea, very similar in appearance to the prescapular but yellower. All other organs and glands were normal in appearance.

Injections of tissue emulsions were made into guinea pigs as before. Between 2 and 3 c.c. of blood only was injected and this caused no infection. The rib marrow again infected one guinea pig only out of two; the spleen, liver, lung, kidney, long thoracic and popliteal glands caused general tuberculosis; the doses were large and the emulsions except that of the rib marrow rich in tissue as before.

The two remaining calves were killed three months after injection. No. 871 had developed a very large swelling and gland, and had been obviously ill a fortnight after the injection, with rapid respiration, staring coat, and high temperature lasting two or three weeks. Later on it became thin, but afterwards picked up, and gained 27 lbs. The average rate of gain during the whole period had been 2lbs. per week. It had been quite well for a month or more before it was killed.

At the seat of injection there was the thin-walled fluid tumour characteristic of a non-virulent injection. There was no infiltration of skin or muscle. The prescapular gland was a little enlarged and almost entirely occupied by a calcareous mass. The prepectoral was also enlarged and very similar in appearance. There were a good many little glassy tuberculous foci in the lungs, few of which had any central opacity, and two rather larger shot-sized tubercles. There were one or two very minute pale specks in each kidney. The suprarenals each contained one or two small tubercles. Minute calcareous foci were found in small numbers in the bronchial and other thoracic glands; the mesenteric, the left supra-pharyngeal and submaxillary, both precrural, popliteal, gluteal, and ischiatic glands. So wide a distribution of non-progressive tubercles is unusual, but other examples have occurred, for example, in 619, H. 12, "H.N."

Tissue emulsions were made and injected into guinea pigs as before. Those injected with blood (13cc). died in a few hours, and one of those injected with marrow died in two days. The rest were killed after fifty-three days (*see* table.) The second guinea-pig injected with marrow was healthy, as were those injected with kidney. Those injected with lungs, liver and spleen had tuberculosis more or less generalised, but in no case severe. These guinea pigs were less affected than those which received emulsions from the more recently affected calves. One may therefore conclude that the bacilli had disappeared from the marrow and kidney, but were still present, though in reduced numbers, in the lungs, liver and spleen. The presence of living non-virulent bacilli in the organs other than the lungs after three months must, I think, be regarded as exceptional, judging from Calf 777, "H.W.", and 865 "K.M." which follows, and remembering that the animal in question presented an unusual number of minute lesions visible to the naked eye.

Calf 865 was killed about the same time. It had been well almost ever since the injection, except for a transient disturbance of health about the third week, and a period of slight fever which lasted about a fortnight. Its average rate of gain during the whole period was 4 lbs. per week. The tumour at the seat of inoculation had softened, ulcerated, and discharged its contents.

At the seat of injection was found a little depressed opening in the skin, leading to a patch of dense brownish-pink tissue packed with little calcareous grains.

The prescapular gland on the same side was a little enlarged and contained some large calcareous nodules, one of which was commencing to soften. The right prepectoral was normal. In the mesenteric glands a little

yellow calcareous focus was found here and there; everything else was normal, unless indeed some little grey specks in the lungs were the legacy of the tubercle bacilli which had been there. They were more minute by far than the glassy tuberculous foci so often described, and they would not have been thought worthy of mention in any other case.

Tissue emulsions were made as before and injected into guinea pigs, care being taken that the doses should be larger than those used in the three first calves of this experiment. Those injected with blood (12cc.) died in a few hours. One guinea-pig injected with lung died after nine days and showed no signs of tuberculosis. One of those injected with popliteal gland died after fifty days, and was found to have general tuberculosis. The remaining guinea-pigs were killed on the 51st day. The one injected with lung was found to have slight tuberculosis, thus supporting the suspicion that the very minute grey specks seen in that organ at the post-mortem examination of the calf were tuberculous in origin. Those injected with spleen, liver, kidney, and rib marrow were found to be perfectly healthy. On the other hand all those injected with glands, namely the popliteal, long thoracic, bronchial and hepatic were tuberculous.

The presence of tubercle bacilli in some of the apparently healthy organs and glands of these two calves, and of Calf 777, which, it will be remembered, had been injected with a virus incapable of producing a progressive, and indeed anything more than a minimal tuberculosis in bovine animals, and which had recovered from any temporary illness from which they may have suffered when they were killed 3 months after injection, was surprising. It became clear that non-virulent tubercle bacilli which get passively disseminated after a subcutaneous injection do not entirely disappear within 3 months.

On the other hand a comparison of the distribution of bacilli in these animals with that in those killed a few days or weeks after injection will leave no doubt that bacilli so disseminated become less numerous as time goes on, and although they have not everywhere disappeared at the end of 3 months, they at least have done so from many parts of the body.

It has been said that one of the calves killed after three months suffered from an illness of unusual severity as compared with other animals injected with similar doses of viruses of Group II. It is possible, therefore, that the distribution of bacilli and their persistence in this animal was greater than usual; and it may be interesting to collect together in a table all the other guinea-pig injections made from time to time with the organs of bovine animals which had been injected with viruses of Group II. (*See* page 228.)

These injections were made for various purposes; some with the object of finding out whether a suspicious focus was tuberculous or no; some with the direct intention of ascertaining whether or no bacilli were to be found in apparently healthy organs; and others again—not always successfully—to demonstrate the non-tuberculous origin of some intercurrent disease, such as pneumonia, or carnification of the lung.

It will be seen that the presence of tubercle bacilli in these organs was by no means uncommon. Probably, however, the frequency is somewhat exaggerated by these experiments which were made, not in every case, but exceptionally for one of the reasons given above, consequently the animals from which the emulsions were taken are not quite fair samples of those injected with viruses not virulent for bovines, but were, some of them at least, selected on account of the presence of more or less suspicious foci, or for some other reason.

However, when one has made all due allowance for this, the findings of tubercle bacilli are still numerous, and one cannot doubt *that tubercle bacilli are frequently present in the organs and glands of bovine animals several months after they have been injected with bacilli which belong to the human Group II. Often they are contained in minute retrogressive tuberculous foci; but not unfrequently in organs which appear perfectly healthy to the naked eye.*

The negative result obtained in the case of the few experiments made with the organs of animals infected by feeding, seems to suggest that passive dissemination of bacilli does not take place in the absence of solution of continuity of tissue.

Summary and Conclusions.

The little glassy or calcareous tubercles often seen in the lungs, and more rarely in other organs of calves injected with tubercle bacilli which failed to produce in them a progressive infection, gradually led to the conviction that there must be *a passive* escape of tubercle bacilli into the circulation; for if they could only have made their way to these distant parts by *active invasion*, that is by overcoming the resistance offered to them by the animal organism, it seemed impossible to understand why they should then have failed to develop there. These experiments have revealed the presence of tubercle bacilli practically all over the body within three days of a subcutaneous injection of 50 mg. of a non-virulent culture, thus demonstrating the fact of passive distribution which had been inferred from the presence of these little glassy tubercles, and at the same time explaining their existence and showing that they afforded no evidence of virulence, and were in no sense due to the *active* invasion of the body by the bacilli which caused them.

It is not my intention to discuss the mode of dissemination, except to say that ultimately it must be by the blood. The very wideness of distribution points to this, and in one calf indeed, the blood from the heart itself infected a guinea-pig. Whether the escape of the bacilli from the seat of injection is by the lymphatic channels, and whether they are simply washed away in the stream of fluid, or carried by phagocytes are questions which fall rather into Dr. Eastwood's province than mine. But I should like to refer to one point which may be thought to have some bearing on the question, namely the difference between the local lesion produced by a virulent and that by a non-virulent virus. One of the characteristic features of the tumour caused at the seat of a subcutaneous injection in the calf by a virulent virus is that it widely infiltrates surrounding tissues, and innumerable caseous foci are found in the neighbouring skin, while the muscle often for a considerable distance contains caseous streaks and patches. On more than one occasion the lymphatics connecting the seat of injection in the neck with the nearest prescapular gland have been found thickened to the size of a small quill by a caseous deposit. On the other hand, if the virus is of the non-virulent kind the tumour after three months is sharply circumscribed and encased in a fibrous capsule often of considerable thickness, and with this capsule it can be easily dissected away from surrounding structures. One cannot but be reminded of the similar difference between a malignant and a benign tumour.

There seems no reason to doubt that the infiltration in the former case takes place by that process of active invasion which was alluded to at the opening of this report, and the fact that in the case of injection of *non-virulent* bacilli the tumour is sharply circumscribed seems to show that this *active* dissemination does not take place unless the virus is virulent, and that the little visceral lesions, which may follow the injection of a non-virulent virus, must be due to an absorption which is purely passive.

The passive distribution of tubercle bacilli from the seat of injection, appears to me to afford a reasonable explanation of the influence of dose in determining the severity of an infection caused by micro-organisms subcutaneously injected. For it is obvious that the numbers of bacilli *passively* distributed in the manner here described will depend largely upon the quantity injected. Whereas on the other hand, if they could only get out by a process of *active invasion* the numbers which thus gained access to distant parts would depend mainly on their activity or virulence, and but little on the original numbers injected; and yet we have repeatedly seen the injection of small numbers, even of very virulent bacilli, fail to cause a general infection.

It is not suggested that a dissemination of micro-organisms, comparable to what takes place when large doses of culture are *injected*, necessarily plays an important part in tuberculous, or other infections under natural conditions, when the number of invading bacilli must

necessarily be small. It is not my intention to discuss here the question, whether it ever plays any, and if so what part under these conditions. The interest of the observations recorded in this report lie, at the moment, in the light which they throw on the consequences of artificial infections such as are constantly being produced here. But the explanation which they offer of the occurrence of apparently isolated tuberculous lesions in distant parts, *e.g.* in the hip, will not be overlooked.

LOUIS COBBETT.

GUINEA PIGS INOCULATED WITH VARIOUS TISSUES OF

Calf 767 (H. 46, "H.W.").

[Killed 25 days after injection.]

Intraperitoneal Injections.

No. of Guinea-pig.	Material injected.	Dose.	Duration of Life, in Days.	Killed or Died.	Result.*
1544	E. of Liver	1·0 cc.	49	Killed.	G.T.
1545	E. of Liver	"	"	"	"
1546	E. of Spleen	"	"	"	"
1547	E. of Spleen	"	"	"	"
1548	E. of Lung	"	"	"	"
1549	E. of Lung	"	"	"	"
1550	E. of Rib Marrow	"	"	"	T. of Omental Gland only.
1551	E. of Rib Marrow	"	"	"	G.T.

These emulsions, with the exception of that from the rib marrow, were very rich in tissue.

GUINEA PIGS INOCULATED WITH VARIOUS TISSUES OF

Calf 777 (H. 46, "H.W.").

[Killed 90 days after injection.]

Intraperitoneal Injections.

No. of Guinea-pig.	Material injected.	Dose.	Duration of Life in Days.	Killed or Died.	Result.
1619	E. of Lung	2·0 cc.	42	K.	G.T.
1620	E. of Lung	"	"	"	"
1621	E. of Spleen	"	"	"	Healthy.
1622	E. of Spleen	"	"	"	"
1620	E. of Liver	"	"	"	"
1624	E. of Liver	"	"	"	"
1625	E. of Rib Marrow	1·0 cc.	"	"	"

These emulsions, with the exception of that from the rib marrow, were very rich in tissue.

* T. = Tuberculous or Tuberculosis.
S.T. = Slight Tuberculosis.
G.T. = General Tuberculosis.
No T. = No Tuberculosis.

GUINEA-PIGS INOCULATED WITH VARIOUS TISSUES OF

Calf 873 (H. 26, "K.M.").

[Killed 3 days after injection.]

Intraperitoneal Injections.

No. of Guinea-pig.	Material injected.	Dose.	Duration of Life, in Days.	Killed or Died.	Result.
1626	E. of Popliteal Gland - -	7·0 cc.	45	Killed	Slight G.T.
1627	E. of Rib Marrow - -	3·0 to 4·0 cc.	,,	,,	G.T.
1628	E. of Spleen - - -	10·0 cc.	,,	,,	,,
1629		,,	,,	,,	,,
1630	Blood from left Iliac Vein -	3·0 to 4·0 cc.	,,	,,	Healthy.
1631	E. of Liver - - - -	6·0 cc.	,,	,,	G.T.
1632		,,	,,	,,	,,
1633	E. of Lung - - - -	15·0 cc.	,,	,,	,,
1634		,,	,,	,,	,,

These emulsions, with the exception of that from the rib marrow, were very rich in tissue, the object being to inject as large a dose as possible.

GUINEA-PIGS INOCULATED WITH VARIOUS TISSUES OF

Calf 881 (H. 26. "K.M.").

[Killed 8 days after injection.]

Intraperitoneal Injections.

No. of Guinea-pig.	Material injected.	Dose.	Duration of Life in Days.	Killed or Died.	Result.
1635	Blood from Heart - -	11·0 cc.	41	Killed	6 tubercles in Spleen.
1636	E. of Rib Marrow - -	5·0 cc.	,,	,,	T.
1637		,,	,,	,,	No T.
1638	E. of Popliteal Gland - -	10·0 cc.	,,	,,	G.T.
1639	E. of Thoracic Gland - -	4·0 cc.	,,	,,	,,
1640	E. of Lung - - - -	8·0 cc.	,,	,,	,,
1641		,,	,,	,,	,,
1642	E. of Liver - - - -	10·0 cc.	,,	,,	,,
1643		8·0 cc.	,,	,,	,,
1644	E. of Spleen - -	10·0 cc.	,,	,,	,,
1645		8·0 cc.	,,	,,	,,
1646	E. of Kidney - - -	4·0 cc.	,,	,,	,,
1647		5·0 cc.	,,	,,	,,

These emulsions, with the exception of that from the rib marrow, were very rich in tissue, the object being to inject as large a dose as possible.

GUINEA-PIGS INOCULATED WITH VARIOUS TISSUES OF

Calf 869 (H. 26, "K.M.").

[Killed 14 days after injection.]

Intraperitoneal Injections.

No. of Guinea-pig.	Material injected.	Dose	Duration of Life, in Days.	Killed or Died.	Result
1648	Blood from Heart and Iliac Vein	2·0 to 3·0 cc.	35	Killed	No T
1649	E. of Rib Marrow - -	6·0 cc.	„	„	T.
1650		4·0 cc.	„	„	No T.
1651	E. of Spleen - - -	9·0 cc.	„	„	G.T.
1652		„	„	„	„
1653	E. of Liver - - - -	9·0 cc.	„	„	„
1654		„	„	„	„
1655	E. of Thoracic Gland - -	5·0 cc.	„	„	„
1656	E. of Kidney - - -	9·0 cc.	„	„	S.T.
1657		„	„	„	„
1658	E. of Lung - - - -	6·0 cc.	„	„	G.T.
1659		„	„	„	„
1660	E. of Popliteal Gland - -	6·0 cc.	„	„	„

These emulsions, with the exception of that from the rib marrow, were very rich in tissue, the object being to inject as large a dose as possible.

GUINEA-PIGS INOCULATED WITH VARIOUS TISSUES OF

Calf 871 (H. 26, "K.M.").

[Killed 91 days after inoculation.]

Intraperitoneal Injections.

No. of Guinea-pig.	Material injected.	Dose.	Duration of Life.	Killed or Died.	Result.
1744	Blood from Jugular Vein -	13·0 cc.	A few hours.	Died	—
1745	E. of Rib Marrow - -	6·0 cc.	2 days.	„	—
1746		6·0 cc.	53 „	Killed	Healthy.
1749	E. of Lung - - - -	10·0 cc.	„ „	„	T.
1750		10·0 cc.	„ „	„	„
1751	E. of Liver - - - -	9·5 cc.	„ „	„	„
1752		9·5 cc.	„ „	„	„
1753	E. of Spleen - -	8·0 cc.	„ „	„	S.T.
1754		8·0 cc.	„ „	„	„
1755	E. of Kidney - - -	6·0 cc.	„ „	„	Healthy.
1756		6·0 cc.	„ „	„	„

These emulsions, with the exception of that of the rib marrow, were very rich in tissue, the object being to inject as large a dose as possible.

Guinea Pigs inoculated with various Tissues of

Calf 865 (H. 26 "K.M.").

[Killed 93 days after inoculation.]

Intraperitoneal Injections.

No. of Guinea-Pig.	Material Injected.	Dose.	Duration of Life.	Killed or Died.	Result.
1763	Blood from Left Jugular Vein	12·0 cc.	24 hours	Died	—
1764		12·0 cc.	⅛ day		—
1765	E. of Liver	10·0 cc.	51 day	Killed	Healthy.
1766		10·0 cc.	" "	"	"
1767	E. of Lung	15·0 cc.	9 "	Died	No T.
1768		15·0 cc.	51 "	Killed	S.T.
1769	E. of Spleen	8·0 cc.	" "	"	Healthy.
1770		8·0 cc.	" "	"	"
1771	E. of Kidney	6·0 cc	" "	"	"
1772		6·0 cc.	" "	"	"
1773	E. of Rib Marrow	6·0 cc.	" "	"	"
1774		6·0 cc.	" "	"	,
1775	E. of Popliteal Gland	10·0 cc.	" "	"	G.T.
1776		10·0 cc.	50 "	Died	"
1777	E. of Long Thoracic Gland	10·0 cc.	51 "	Killed	"
1778	E. of Bronchial Gland	10·0 cc.	" "	"	"
1779	E. of Hepatic Gland	8·0 cc.	" "	"	"

These emulsions, with the exception of that of the rib marrow, were very rich in tissue, the object being to inject as large a dose as possible.

From time to time during the course of the work here, guinea-pigs have been injected with emulsions of glands distant from the seat of injection and of organs of bovine animals which have been inoculated with the tubercular viruses of human Group II. Sometimes these have been made for the purpose of elucidating the nature of some suspicious focus; sometimes with the direct object of seeing whether the bacilli were to be found in the spleen or some other part, at others to demonstrate (not always successfully) the non-tubercular origin of intercurrent disease, such, for example, as pneumonia, or carnification of lung following an old blocking of a bronchus.

As these experiments add some additional evidence of passive dissemination to that already brought forward, they are collected together into the following table. As this kind of experiment was not made as a rule, but only for one of the reasons given above, it is probable that the calves from which the tissue emulsions came were not average samples of those injected with viruses non-virulent for bovines, but were on the whole taken from those rather more severely affected than their fellows, or from animals suffering from some other disease of more or less severity.

The frequency with which tubercle bacilli were found in these emulsions of organs then must not be taken as an exact measure of the frequency of passive dissemination in calves which have been subcutaneously injected. These experiments are only referred to here as additional evidence of the fact that such dissemination does occur.

It is a point of some interest that the animals fed with viruses of human Group II. afforded no evidence of passive dissemination though they developed some small amount of tuberculosis in the mesenteric glands. There is not enough evidence of this kind to warrant the drawing of any conclusions, but it would appear that passive dissemination does not occur unless the continuity of tissue is broken, as in subcutaneous injection.

INJECTION OF GUINEA-PIGS WITH TISSUES (OTHER THAN LOCAL LESION AND NEAREST GLANDS) OF CALVES OR HEIFERS WHICH HAD BEEN INJECTED WITH HUMAN VIRUSES OF GROUP II.

I.—Subcutaneous Injections.

No. of Calf or Heifer.	Virus.	No. of Days between injection and killing of Bovine Animal.	Organ or Gland used for injection of Guinea Pig.	Appearance of Organ or Gland at Post-Mortem.	Nos. of Guinea Pigs inoculated.	Result.
			Injections of Lung.			
177	H. 8, "S.C."	54	Lung	Collapsed area of lung. No tubercles seen in the organ	765–766	T.
413	H. 15, "I.W."	70	„	Small collapsed area of lung. No tubercles seen in the organ	1348–349	T.
443	H. 11, "E.D."	72	„	Right anterior lobe collapsed and carnified. No tubercles seen in organ	1366–367	No T.
737	H. 9, "C.T."	87	„	Few calcareous foci found in the organ	598	T.
619	H. 12, "H.N."	83	„	Anterior lobes carnified. No tubercles seen in the organ	1513–514	T.
			Injections of Liver or Hepatic Glands.			
185	H. 9, "C.T."	40	Liver	Minute indefinite pale foci seen on surface	761–762	T.
265	H. 17, Sp. B.	107	Hepatic gland	A few calcareous grains in cortex	1048–49	T.
277	„ „	89	Liver	One pinhead tubercle only	1087	No T.
			Hepatic gland	Several yellow calcareous foci	1086	T.
			Injections of Spleen.			
207	H. 2, Sp. "A"	54	Spleen	Apparently normal	821–822	No T.
331	H. 9, "C.T."	66	„	„ „	1250–251	T.
367	H. 15, "I.W."	56	„	„ „	1244–245	T.
157	H. 16, "J.H."	67	„	„ „	749–750	No T.
265	H. 17, Sp. "B"	107	„	Two small calcareous tubercles	1046–47	T.
277	H. 17, Sp. "B"	89	„	Apparently normal	1085	T.
279	H. 21, "G.B."	91	„	Three tubercles seen	1114	T.
			Injections of Thoracic Glands.			
275	H. 8, "S.C."	59	Peribronchial gland	Cortex infiltrated with calcareous foci	1131	T.
269	H. 12, "H.N."	114	Bronchial gland	Apparently normal	1066–67	No. T.
277	H. 17, Sp. "B"	89	Thoracic gland	A few yellow foci in cortex	1088	T.
227	„ „	114	„	Apparently normal	1056	T.
131	H. 18, "T.T."	139	Bronchial gland	Congested; no tubercles seen	870	T.
279	H. 21, "G.B."	91	Thoracic gland	Minute calcareous foci in cortex	1115	T.

II.—Feeding Experiments.

No. of Calf or Heifer.	Virus.	No. of Days between injection and killing of Bovine Animal.	Organ or Gland used for injection of Guinea Pig.	Appearance of Organ or Gland at Post Mortem.	Nos. of Guinea Pigs inoculated.	Result.
			Injections of Liver.			
51	H. 4 "M.L."	248	Liver	Three minute pale foci seen	497	No T.
			Injections of Spleen.			
51	H. 4 "M.L."	248	Spleen	Apparently normal	498	No T.
57	H. 6 "U.H."	146	„	„ „	491–492	No T.
171	H. 17, Sp. "B."	154	„	„ „	902–903	No T.

EARLY DISSEMINATION OF TUBERCLE BACILLI IN A RAT.

In a rat, which received an intraperitoneal injection of culture from human spleen "H. 54, C.W." on 4-10-05., and which died without ascertained cause thirteen days later, tubercle bacilli were found in large numbers on microscopic examination of the spleen and liver pulp. A few were found also in the lungs, but none in the kidney. There was no lesion visible to the naked eye, except in the omentum which was drawn up and contained opaque yellowish-white collections, which, however, seemed to consist chiefly of the material injected.

Calf 769 (H. 45, "F.M.").

SUBCUTANEOUS inoculation on right side of neck of culture derived from human bronchial glands, through Guinea-Pig 1,137.

DOSE.—50 milligrammes.

DATE OF INOCULATION.—April 19th, 1905.

Weights :—

April 19th, 1905.—1 cwt. 0 qrs. 5 lbs.
May 6th, 1905.—0 cwt. 3 qrs. 10 lbs.
Total loss of weight :—23 lbs.

DIED of diarrhœa, May 6th, 1905 (17 days after inoculation).

POST-MORTEM EXAMINATION.

General Condition.—Very thin.

Seat of Inoculation.—At the seat of inoculation, on the right side of the neck, is a patch, 2½in. x 2in., composed of inflammatory tissue, soft and commencing to break down.

Right Prescapular Gland } Are slightly swollen, but
Right Prepectoral Gland } show no sign of tubercle.

Thoracic Glands.—Show no sign of tubercle.

Lungs.—Several dark points are seen, one of which only has a minute opaque centre. Of the rest, two, reddish and slightly raised, are situated on the visceral pleura. The lungs show superficial congestion about their bases.

Pleura
Heart
Omentum
Peritoneum
Liver
Hepatic Glands
Spleen
Kidneys
Suprarenals
Tongue
Pharynx
Palate
Tonsils
Posterior-Pharyngeal Lymphatic Glands
Submaxillary Lymphatic Glands
Hyoid Lymphatic Glands
Subparotid Lymphatic Glands
} Normal.

Intestines.—The lower part of the small intestine, and the large intestine, are greatly congested, and show patches of bright red colour.

Mesenteric Glands } Normal.
Testes }

Various Lymphatic Glands :—

Left Prescapular
Axillary
Precrural
Popliteal
Gluteal
Ischiatic
Renal
Lumbar
Iliac
Pubic
} Normal.

MICROSCOPIC EXAMINATION.

Yellow Speck from Intestine
Grey Firm Specks from Lung (1)
(2)
(3)
(4)
} No tubercle bacilli.

Scraping from Prescapular Gland } Tubercle bacilli
Scraping from Prepectoral Gland } fairly numerous.
Scraping from Long Thoracic Gland Tubercle bacilli few.

COMMENT ON CALF 769. H 45. "F.M." May 6th, 1905.

"This calf suffered for some time from, and ultimately succumbed to, diarrhœa. At the post-mortem examination the tubercular disease was found circumscribed as usual when viruses of the non-virulent kind are injected. The new fact revealed by this case was the considerable number of bacilli in the prescapular, prepectoral, and long thoracic glands. The two former were somewhat swollen, but showed no naked eye changes characteristic of tubercle, while the latter was normal in size. These bacilli were found then in a general scraping of the gland, while in ordinary cases, even when tubercular lesions are found in the thoracic glands, tubercle bacilli are limited to the visibly affected areas, and are even so very scanty. There were therefore in this case a much larger number of bacilli in the long thoracic gland than usual, and the question arises whether the tuberculosis was making a more severe progress in this case than usual on account of the intermittent disease, or whether a similar distribution of bacilli occurs in other cases, followed by their subsequent disappearance. The former hypothesis is not supported by other cases, where intercurrent disease has existed, while the latter is in harmony with the finding of minute arrested tubercles in small numbers in some of the animals injected with non-virulent viruses.

Calf 767 (H. 46, "H.W.").

SUBCUTANEOUS inoculation on right side of neck of culture derived from Guinea-pig 1444, inoculated with emulsion of tuberculous human knee joint.

DOSE.—50 Milligrammes.

DATE OF INOCULATION.—April 28th, 1905.

Weights :—

April 28th, 1905.—1 cwt. 0 qrs. 15 lbs.
May 23rd, 1905.—1 cwt. 0 qrs. 18 lbs.
Total gain of weight : 3 lbs.

KILLED.—May 23rd, 1905 (25 days after inoculation), at a time when the febrile reaction was just over, and the enlargement of the prescapular gland probably at its height.

POST-MORTEM EXAMINATION.

General Condition.—Good.

Seat of Inoculation.—The tumour at the seat of inoculation, on the right side of the neck, is large, flat, and fluctuating. It measures 6 in. x 3½ in. It is closely attached to the subjacent muscle, and slightly infiltrates the skin.

On section it is composed of caseo-necrotic tissue, and has large ragged cavity in its centre, full of caseous flakes suspended in a serous fluid.

Right Prescapular Gland.—Measures 3 in. x 1½ in. x 1¾ in. It is firm and elastic, and consists partly of translucent grey tissue, and partly of a caseating network. Its capsule is much thickened, and it is very adherent to the surrounding tissues.

Right Prepectoral Gland.—The size of a large pea, is firmer than normal, and in part of its cortex numerous yellow points could be faintly seen.

Thoracic Glands.—Normal.

Lungs.—On the surface of the lungs rather numerous minute grey translucent points can be seen. Some of these have a reddish tinge, and it is difficult to distinguish them from the minute hæmorrhages, which are of frequent occurrence. Some are undoubtedly tubercles, of the kind frequently found in these cases, for they can be picked out on the point of a knife. Scarcely any trace of opacity can be seen in the centres of any of them, and when crushed they do not break under the knife.

Pleura, *Heart*, *Omentum*, *Peritoneum* — Normal.

Liver.—One or two pale specks on the surface is the nearest suggestion of tubercles that can be seen.

Hepatic Glands, *Spleen* — Normal.

Kidneys.—A pale speck, similar to those seen in the liver, was seen in each kidney.

Suprarenals, *Tongue*, *Pharynx*, *Palate*, *Tonsils*, *Posterior Pharyngeal Lymphatic Glands*, *Submaxillary Lymphatic Glands*, *Hyoid Lymphatic Glands*, *Subparotid Lymphatic Glands*, *Intestines*, *Mesenteric Glands*, *Eyes*, *Testes* — Normal.

Various Lymphatic Glands:—
Left Prescapular, *Axillary*, *Precrural*, *Popliteal*, *Gluteal*, *Ischiatic*, *Renal*, *Lumbar*, *Iliac*, *Pubic* — Normal.

MICROSCOPIC EXAMINATION.

Focus from Lung.—No tubercle bacilli.

COMMENT ON CALF 767.

In some respects the local lesion and nearest gland resembled those found in cases of acute general tuberculosis, but the muscle, though adherent, was not infiltrated, as it invariably is in these latter cases, and the central cavity was much larger, showing a greater tendency to softening and breaking down. The lesions in the lungs were no more than are frequently seen in calves inoculated with non-virulent viruses, unless indeed there were rather more of them. They were very small, and, unlike those in calves killed ninety days after inoculation, they were not calcified.

In choosing this calf, one was influenced by the longer rise of temperature and by the larger size of the prescapular gland. But really there was little to choose between the animals, and the other had lost two pounds in weight, while the one killed had gained three. The remaining calf therefore is a fair control, and it must not be thought that the one killed was any more likely to develop general tuberculosis than the other.

Calf 777 (H. 46, "H.W.").

SUBCUTANEOUS inoculation on right side of neck of culture derived from Guinea-pig 1444, inoculated with emulsion of tuberculous human knee joint.

DOSE.—50 milligrammes.

DATE OF INOCULATION.—April 28th, 1905.

Weights,—
April 28th, 1905.—1 cwt. 0 qrs. 4 lbs.
July 27th, 1905.—1 cwt. 0 qrs. 6 lbs.
Total gain of weight.—2 lbs.

KILLED when in good health, July 27th, 1905. (90 days after inoculation.)

POST-MORTEM EXAMINATION.

General Condition.—Good.

At the Seat of Inoculation on the right side of the neck is a prominent fluctuating swelling measuring 4 x 2½ x 2in., and weighing 6½ ozs.

It consists of a fibrous walled cavity containing softened caseous débris.

There has been no external discharge.

Right Prescapular Gland is moderately enlarged, and contains two large fibro-calcareous nodules, the larger of which is already considerably softened.

Right Prepectoral Gland.—Not enlarged, is normal on section.

Lungs.—The lungs show several small areas of collapse, but are otherwise normal.

Thoracic Glands.—are not enlarged and are normal in structure.

Heart, *Pleura*, *Omentum and Peritoneum*, *Liver*, *Hepatic Glands*, *Spleen*, *Kidneys*, *Suprarenals*, *Tongue*, *Pharynx*, *Palate*, *Tonsils*, *Posterior-Pharyngeal, Submaxillary, Hyoid and Subparotid Lymphatic Glands* — Normal.

Mesenteric Glands.—The hooked mesenteric gland, which lies at the angle of the mesentery, contains many minute points visible throughout its capsule. These when picked out on the point of a knife and crushed between coverslips, are found to be slightly gritty. A few neighbouring glands also have some of these foci.

Small Intestine.—In the small intestine, about 1½ yards from the ileo-caecal valve, the part corresponding with the gland most affected, are seen a small number of whitish spots, quite superficial and easily scraped off

with a knife, unlike tubercles as usually seen. The large intestine is normal.

Testes.—Normal.

Various Lymphatic Glands:—

Axillary and Prescapular (left)
Precrural and Popliteal
Gluteal and Ischiatic
Renal
Lumbar
Iliac
Pubic
} Normal.

Microscopic Examination.

Foci from Mesenteric Gland.—Tubercle bacilli.

Foci from Small Intestine.—No tubercle bacilli.

Comment on Calf 777.

The minimal lesions usual when non-virulent viruses are injected were found in this animal.

This animal is the fellow to Calf 767, which was killed 25 days after inoculation in order that the various organs and tissues should be examined for tubercle bacilli. It therefore forms the control to the experiment just mentioned.

Calf 873 (H. 26, "K.M.").

Subcutaneous inoculation, on right side of neck, of culture derived from human kidney, through guinea-pig 1326.

Date of Inoculation.—July 25th, 1905.

Dose.—50 milligrammes.

Killed.—July 28th, 1905 (3 days after inoculation), in order to determine the distribution of bacilli at this period.

Post-mortem Examination.

General Condition.—Good.

Seat of Inoculation.—At the seat of inoculation, on the right side of the neck, is a small soft swelling. On section the tissues seem to be a little oedematous, and the subcutaneous tissue thickened and firmer than normal.

Right Prescapular Gland.—Is somewhat swollen and oedematous.

Thoracic Glands
Lungs
Heart
Pleura
Peritoneum
Liver
Hepatic Glands
Spleen
Kidneys
Suprarenals
Pharynx
Tongue
Tonsils
Intestines
Mesenteric Glands
Testes
} Normal.

All lymphatic glands were examined, and found normal.

Calf 881 (H. 26, "K.M.").

Subcutaneous inoculation on right side of neck of Culture derived from human kidney, through Guinea-pig 1326.

Date of Inoculation. –July 25th, 1905.

Dose.—50 milligrammes.

Weights.

July 25th, 1905.—0 cwt., 3 qrs., 15 lbs.

August 2nd, 1905.—0 cwt., 3 qrs., 26 lbs.

Total gain of weight.—11 lbs.

Rate of gain per week.—10 lbs.

Killed when very well, August 2nd, 1905 (8 days after inoculation), in order to determine the distribution of bacilli at this period.

Post-mortem Examination.

General Condition.—Good.

Seat of Inoculation.—At the seat of inoculation, on the right side of the neck, is a small swelling 2½″ × 2″.

Section shows a reddish grey and rather firm tissue under the skin, about half an inch thick, infiltrated with spots of the size of shot, of softened caseous matter resembling muco-pus.

Right Prescapular Gland.—Is somewhat swollen, but shows no sign of tubercle.

Lungs
Thoracic Gands
Heart
Pleura
Peritoneum
Liver
Hepatic Glands
Kidneys
Suprarenals
Spleen
Tongue
Pharynx
Palate
Tonsils
Posterior-Pharyngeal Glands
Submaxillary Glands
Hyoid Glands
Subparotid Glands
Intestines
Mesenteric Glands
Mamma
Axillary Glands
Prescapular Gland (Left)
Precrural Glands
Popliteal Glands
Gluteal Glands
Ischiatic Glands
Renal Glands
Lumbar Glands
Iliac Glands
} Normal in appearance.

Microscopic Examination.

No Microscopic Examination was made.

Calf 869 (H. 26, "K.M.").

Subcutaneous inoculation on right side of neck of Culture derived from human kidney, through Guinea-pig 1326.

Date of Inoculation.—July 25th, 1905.

Dose.—50 Milligrammes.

Weights.

July 25th, 1905.—0 cwt. 3 qrs. 0 lbs.
August 8th, 1905.—0 cwt. 3 qrs. 9 lbs.
Total gain of weight.—9 lbs.

Killed—When very well, in order to determine the distribution of bacilli, August 8, 1905 (14 days after inoculation).

Post-mortem Examination.

Seat of Inoculation.—At the seat of inoculation is a tumour, measuring 4″ x 3″ x 1¼″. At its centre is an irregular cavity, containing muco-purulent material, around which is dense oedematous tissue. The subjacent muscle is not infiltrated, but the skin over the tumour is distinctly altered in appearance.

Right Prescapular Gland.—Is enlarged (long axis three and a quarter inches), firm, elastic, and oedematous. There is no appearance characteristic of tubercle.

Prepectoral Gland.—Is the size of a large pea. It is very similar to the prescapular, but of a yellower colour.

Lung, *Thoracic Glands*, *Liver*, *Portal Glands* — Showed nothing abnormal.

Spleen, *Kidneys*, *Suprarenal*, *Testes*, *Intestine*, *Mesenteric Glands*, *Pharynx*, *Posterior-Pharyngeal Glands*, *All Peripheral Glands* — Normal.

Microscopic Examination.

Emulsion of Liver, *Emulsion of Lung*, *Emulsion of Kidney* — No tubercle bacilli seen.

Calf 871 (H. 26. "K.M.").

Subcutaneous inoculation, on right side of neck, of Culture derived from human kidney, through Guinea-pig 1326.

Date of Inoculation.—July 25, 1905.

Dose.—50 Milligrammes.

Weights.

July 25th, 1905.—0 cwt. 3 qrs. 19 lbs.
October 24th, 1905.—1 cwt. 0 qrs. 18 lbs.
Total gain of weight.—27 lbs.
Rate of gain per week.—2 lbs.

Killed.—When well, October 24th, 1905 (91 days after inoculation).

Post-mortem Examination.

Seat of Inoculation.—At the seat of inoculation is a large, rounded, fluctuating swelling, consisting of a fibrous wall and fluid contents, with caseo-necrotic masses of various sizes. At the posterior corner are some small calcareous nodules. It measures 3¾in. x 2⅛in. x 3 in., and weighs 9 ozs.

Prescapular Gland.—Measured 2⅛in. x 1⅝in. x 1in., and weighs 2 ozs. It was almost entirely occupied by a large and very calcareous mass, but a portion of the soft brown gland substance still remained.

Prepectoral Gland.—Was as large as a small cherry, and was also calcareous.

Bronchial Glands and Other Thoracic Glands — Were not enlarged. Here and there a small yellow calcareous focus could be found.

Lungs.—Looked quite healthy, but on close inspection a good many minute translucent glassy tubercles could be seen. The largest of these was only one millimetre in diameter. Few of them had any central opacity. In addition two rather large shot-like tubercles were seen.

Liver.—Normal.

Hepatic Glands.—Showed one or two little doubtful points.

Spleen.—Normal.

Kidneys.—Normal, except for one or two very minute pale specks.

Suprarenals.—Each contained one or two small tubercles.

Testes, *Tongue*, *Pharynx*, *Tonsils*, *Larynx* — Normal.

Intestines.—Normal, except for some pultaceous yellow points (with no tubercle bacilli).

Left Posterior Pharyngeal Gland.—Had two or three small calcareous grains.

Right Submaxillary Gland, *Right Post-Pharyngeal Gland*, *Hyoid Glands*, *Subparotid Glands* — Normal.

Mesenteric Glands.—Almost without exception contained little yellow calcareous grains.

The following peripheral glands contained a few little yellow calcareous grains:—

Precrural.
Popliteal.
Gluteal.
Ischiatic.

The following were normal:—

Iliac.
Axillary.
Renal.
Lumbar.

Microscopic Examination.

Calcareous Grains from the Popliteal Gland.—Tubercle bacilli few.

Calcareous Grains from the Long Thoracic Gland.—Tubercle bacilli. (Two seen.)

Glassy Tubercles from Lungs, *Larger Tubercle from Lung* — No tubercle bacilli.

Large and small Tubercles from Lung (mixed).—Tubercle bacilli few.

Emulsion of Spleen, *Emulsion of Liver*, *Emulsion of Rib Marrow*, *Yellow Pultaceous Matter from Intestine* — No tubercle bacilli.

Comment on Calf 871.

The animal developed a very large swelling and gland, and a fortnight after injection was obviously ill, with rapid respiration and staring coat. There was the usual rise of temperature, lasting two or three weeks. Later on it became thin, but afterwards picked up, and gained 27 lbs. It had been quite well for a month or more before it was killed.

The lesions seen were like those often before found in animals injected with non-virulent viruses. Thus there are numerous parallels to the lung lesions, and tubercles in the suprarenals have been fairly common, *e.g.*, 135, H.12, "H.N.," and many others.

To find a parallel to so wide a distribution of calcareous grains in the peripheral lymphatic glands one must go to 619, H.12, "H.N." 625, H.23, "J.P." The peculiarity of this calf is that it combines so many of these occasional lesions.

It is odd that while many peripheral glands were more or less affected, the renal and lumbar, which with virulent bacilli are usually more severely affected than the others, in this case escaped.

Calf 865 (H. 26. "K.M.").

Subcutaneous inoculation, on right side of neck, of Culture derived from human kidney, through guinea-pig 1326.

Date of Inoculation.—July 25th, 1905.

Dose.—50 milligrammes.

Weights.
July 25th, 1905.—0 cwt. 3 qrs. 12 lbs.
October 26th, 1905.—1 cwt. 1 qr. 9 lbs.
Total gain of weight.—1 qr. 25 lbs.
Rate of gain per week.—4 lbs.

Killed.—When well, October 25th, 1905 (93 days after inoculation).

Post-mortem Examination.

General Condition.—Good.

Seat of Inoculation.—At the seat of inoculation, on the right side of the neck, is a little opening in the skin, and beneath it is a pinkish-brown patch of dense brownish tissue packed with little calcareous grains.

Right Prescapular Gland.—Is a little enlarged, two and a quarter inches in length. It contains some large calcareous nodules, and one in process of softening.

Right Prepectoral Gland.—Normal.

Lungs.—Normal. (Here and there one could see some little grey specks, more minute by far than those which have frequently been described.)

Bronchial Glands
Thoracic Glands
Peribronchial Glands
} Normal.

Pleura.—On the costal lining is a little pedunculated body the size of a grape seed. (No tubercle bacilli in it).

Heart
Peritoneum
Liver
Hepatic Glands
Spleen
} Normal.

Kidneys
Suprarenals
Testes
Pancreas
Thyroid
Tongue
Pharynx
Larynx
Posterior Pharyngeal Lymphatic Glands
Normal.
Submaxillary Lymphatic Glands
Hyoid Lymphatic Glands
Subparotid Lymphatic Glands
Intestines

Mesenteric Glands.—Here and there in these glands was a little yellow calcareous focus.

Various Lymphatic Glands:—
Left Prescapular
Axillary
Precrural
Popliteal
Gluteal
Ischiatic
Renal
Lumbar
Iliac
Pubic
Normal.

Microscopic Examination.

Calcareous Foci from Mesenteric Glands (All that could be found).—Tubercle bacilli.

Pedunculated Body from Pleura.—No tubercle bacilli.

Comment on Calf 865.

Minimal lesions after 50 milligrammes of a feebly virulent virus.

The little grey points in the lungs would not have been thought worthy of notice if it were not considered that they might possibly be the legacy of the bacilli which escaped into the lung.

REPORT ON THE DISSEMINATION OF TUBERCLE BACILLI IN DIFFERENT SPECIES OF ANIMALS AFTER SUBCUTANEOUS INOCULATION.

Dr. **A. STANLEY GRIFFITH** and Dr. **F. GRIFFITH.**

ROYAL COMMISSION ON TUBERCULOSIS.

ON THE DISSEMINATION OF TUBERCLE BACILLI IN DIFFERENT SPECIES OF ANIMALS AFTER SUBCUTANEOUS INOCULATION.

It has been shown by Dr. Cobbett that slightly virulent tubercle bacilli are rapidly distributed throughout the body of a calf after subcutaneous inoculation, and can be demonstrated in the tissues by inoculating guinea-pigs.

The object of these experiments has been to ascertain whether a similar distribution takes place after subcutaneous inoculation of highly virulent tubercle bacilli, not only in the calf but also in other species of animals.

In five experiments bacilli of low bovine virulence were used.

Observations have been made on eight different species of animals, namely, the monkey, calf, pig, cat, rabbit, guinea-pig, rat and fowl.

The following table gives in brief the dose of culture and the duration of life in the case of each animal used in the investigation.

Species.	Number of Animals.	Dose of Culture.	Duration of Life.
Monkey - - -	1	20 mg.	24 hours.
Calf - - -	1	44 mg.	24 hours.
Pig - - - -	5	20 mg.	1, 3, 5, 8, and 14 days.
Cat - - - -	3	10 mg.	24 hours.
Rabbit - - -	3	20 (1) and 10 mg. (2)	24 (2) and 48 (1) hours.
Guinea-Pig - -	3	10 mg.	24 (2) and 48 (1) hours.
Rat - - - -	2	10 mg.	24 and 48 hours.
Chicken - - -	1	10 mg.	24 hours.

The subcutaneous tissues of the groin, back, or ear were chosen as the site of inoculation in order to obviate, during the removal of the organs, any possible chance of contamination by the culture inoculated.

The animals were killed by chloroform, and in every case blood was immediately withdrawn from the right ventricle by means of a glass pipette. A small quantity was distributed over the surface of each culture tube, while 2 cc., mixed with an equal amount of citrate of soda solution, were reserved for inoculating into a guinea-pig. The portions of the various organs from which cultures were made and guinea-pigs inoculated were removed with aseptic precautions.

The egg medium, which has been found the most reliable of all media for the production of primary cultures, was used exclusively, and the tubes were incubated for periods of two months.

The experiments show that tubercle bacilli are distributed over the body of an animal within 24 hours after subcutaneous inoculation, and this dissemination occurs in every species inoculated, whether the bacilli are virulent or only slightly virulent for that species.

That no inconsiderable number of bacilli escapes into the circulation is shown by the ease with which cultures were in all cases obtained from the spleen and by the production of tuberculosis in every guinea-pig inoculated with blood.

In the series of pigs killed at intervals the number of bacilli in the blood was greatest 24 hours after inoculation, and appeared gradually to diminish subsequently.

Seeing that in all species of animals the blood stream plays so important a part in the immediate distribution of tubercle bacilli over the body, the variations in the anatomical distribution of the disease which have been observed in different species after subcutaneous inoculation can best be explained on the hypothesis that different tissues vary in their susceptibility to tuberculosis.

It is interesting to observe in the case of the chicken the rapidity with which bacilli, which are incapable of setting up tuberculosis in that animal, are distributed over the body.

A. Stanley Griffith.

F. Griffith.

RHESUS MONKEY 150.

Inoculated subcutaneously between the shoulders on June 7, 1906, with 20 mg. of culture from the original material of Virus XXVI.

Killed June 8, 1906, 24 hours after inoculation.

Distribution of Bacilli.

Material Used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood	3	None.	2,071	Died 8 days	No tuberculosis.
			2,072	Killed 33 days	Discrete tubercles in omentum; slight tuberculosis of spleen and of most of abdominal lymphatic glands.
Spleen	3	Numerous colonies on all the tubes.	2,073	Killed 33 days	General tuberculosis.
			2,074	Died 19 days	Slight tuberculosis.
Marrow	2	2	—	—	—

CALF 438.

Inoculated subcutaneously on the left side of the neck on June 22, 1906, with 44 mg. of culture of Viruses XXIX. and XXX.

Killed June 23, 1906, 24 hours after inoculation.

P.M.:—Organs and glands perfectly normal to the naked eye.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood	3	None	2,097	Killed 27 days	General tuberculosis; scattered discrete tubercles in omentum.
			2,098	Killed 27 days	Slight tuberculosis of omentum, spleen, liver and of several lymphatic glands.
Lung	—	—	2,099	Killed 27 days	General tuberculosis.
			2,100	Killed 27 days	General tuberculosis.
Spleen	3	13	2,101	Killed 27 days	General tuberculosis.
			2,102	Killed 27 days	No tuberculosis.
Liver	2	4	2,105	Killed 27 days	General tuberculosis.
Kidney	—	—	2,106	Killed 27 days	Slight tuberculosis of omentum, liver and spleen and of several lymphatic glands.
Popliteal gland	—	—	2,108	Killed 27 days	
Marrow	2	1	2,107	Killed 27 days	
Muscle	—	—	2,103	Killed 27 days	One tubercle in omentum; pyloric gland partly caseous; portal gland slightly affected.
			2,104	Killed 27 days	No tuberculosis.
Prescapular gland	2	1	—	—	—

PIG 144.

Inoculated subcutaneously under the skin of the ear on May 4, 1906, with 20 mg. of culture from Virus B. V.

Killed, May 5, 1906, 24 hours after inoculation.

P.M.—There was a small patch of necrosis in the subcutaneous tissues of the ear and no sign of disease elsewhere

Distribution of Bacilli.

Material Used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood	2	3	1,988	Killed 32 days	General tuberculosis.
Lung	—	—	1,991	Killed 32 days	Slight general tuberculosis; discrete tubercles in omentum.
Spleen	3	5	1,993	Died 32 days	General tuberculosis; discrete tubercles in omentum.
Liver	—	—	1,994	Died 12 days	Subacute infection; no tuberculosis.
Muscle	—	—	1,989	Killed 32 days	No tuberculosis.
			1,990	Killed 32 days	No tuberculosis.
Marrow	1	None	—	—	—

PIG 146.

Inoculated subcutaneously under the skin of the ear on May 2, 1906, with 20 mg. of culture from Virus B.V.

Killed, May 5, 1906; 3 days after inoculation.

P.M.—In the subcutaneous tissues of the ear there was a yellow patch of necrosis, larger than in Pig 144, undergoing softening. The nearest lymphatic gland was slightly enlarged. There was no sign of disease in any other part of the body.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood	2	None	1,995	Died, 23 days	Two or three minute transparent tubercles in omentum; pseudo-tuberculosis of spleen and liver. Microscopical. ? T.B. in omentum; none in liver or spleen.
Lung	—	—	1,998	Killed, 39 days	General tuberculosis.
			1,999	Died, 39 days	General tuberculosis.
Spleen	2	3	2,000	Died, 24 days	Numerous discrete tubercles in omentum; tuberculosis of spleen and liver and many glands. T.B. found in a splenic gland.
Liver	—	—	2,001	Died, 25 days	General tuberculosis. T.B. in the pyloric gland.
Muscle	—	—	1,996	Died, 39 days	Caseous nodule in muscle wall; a few tubercles in omentum and on peritoneum; slight tuberculosis of spleen, liver and lungs.
			1,997	Died, 24 days	The omentum contained four translucent miliary tubercles; no tuberculosis elsewhere. T.B. found in omental tubercle.
Marrow	—	1	—	—	—

PIG 148.

Inoculated subcutaneously under the skin of the ear on April 30, 1906, with 20 mg. of culture from Virus B. V.
Killed, May 5, 1906, 5 days after inoculation.

P.M.—In the subcutaneous tissues of the ear there was a small swelling composed of yellow necrotic breaking down substance. The nearest gland was enlarged and œdematous. There was no sign of disease elsewhere.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	2	None	2,002	Died, 41 days	Small caseous nodule in muscle wall; nearest glands tuberculous; two tubercles in the omentum and one on the mesentery.
Lung - -	—	—	2,005	Died, 28 days	General tuberculosis.
			2,006	Died, 26 days	General tuberculosis.
Spleen - -	3	2	2,007	Died, 38 days	General tuberculosis; discrete caseous nodules in the omentum.
Liver - -	—	—	2,008	Died, 29 days	General tuberculosis; the omentum contained about a dozen miliary tubercles.
Muscle - -	—	-	2,003	Killed, 41 days	Caseous nodule in lung; bronchial glands greatly enlarged and partly caseous; spleen enlarged, beset with miliary tubercles; portal gland caseous; no disease of omentum.
			2,004	Died, 26 days	No tuberculosis.
Marrow - -	1	None	—	—	—

PIG 142.

Inoculated subcutaneously under the skin of the ear on April 28, 1906, with 20 mg. of culture from Virus B. V.
Killed May 6, 1906, 8 days after inoculation.

P.M. :—Caseo-necrotic local tumour; the glands at the root of the ear were enlarged and showed on section very early streaks of necrosis. The lungs showed fairly numerous minute grey tubercles, the largest about 0·5 mm. The liver showed under the capsule a few minute grey points. Other organs and glands appeared normal.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	2	None	2,009	Died 12 days	One grey tubercle in omentum; pyloric gland slightly enlarged; no tubercle bacilli found.
Lung - -	—	—	2,012	Died 20 days	Early general tuberculosis.
			2,013	Died 20 days	General tuberculosis.
Spleen - -	3	13	2,014	Died 31 days	General tuberculosis.
Liver - -	1	Numerous	2,015	Died 12 days	No naked eye evidence of tuberculosis.
Left precrural gland - -	—	—	2,016	Died 41 days	General tuberculosis.
Muscle - -	—	-	2,010	Died 49 days	Omentum normal; small pea-sized caseous nodule in lung; bronchial glands enlarged, firm and partly caseous; tubercles in spleen.
			2,011	Killed 49 days	Two minute grey foci in the omentum; caseous nodule in the lung; bronchial glands enlarged and partly caseous; tubercles in the spleen.
Marrow - -	1	None	—	—	—

PIG 150.

Inoculated subcutaneously under the skin of the ear on April 25, 1906, with 20 mg. of culture from Virus B. V.

Killed, May 9, 1906, 14 days after inoculation.

P.M.—In the subcutaneous tissues of the ear there was a caseo-necrotic tumour with a cavity in the centre communicating externally through a small opening. At the base of the ear near the angle of the jaw there was a large firm gland which on section showed a yellowish or yellowish-white necrotic network in a matrix of grey translucent tissue ; two neighbouring glands, smaller in size, were similarly affected, but in a less advanced stage of necrosis. The lungs were closely beset with grey tubercles, varying a little in size, the largest not quite 1 mm. in diameter ; a good many of the larger tubercles had minute central opacities. The liver substance contained sparsely scattered grey tubercles, the largest 0·5 mm. in diameter ; there was also one tubercle, a little over 1 mm. in diameter, with a yellow centre. The kidneys contained fairly numerous greyish-white tubercles, the largest 1 mm. in diameter. A portal gland showed a few greyish foci. Other organs and glands appeared normal.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	3	None	2,017	Killed, 34 days	No tuberculosis.
Lung - -	—	—	2,020	Died, 33 days	General tuberculosis ; omentum much thickened ; tubercles on peritoneum.
			2,021	Died, 32 days	General tuberculosis ; omentum much thickened ; numerous tubercles on peritoneum.
Spleen - -	3	1	2,022	Died, 27 days	General tuberculosis.
Liver - -	1	3	2,023	Died, 32 days	General tuberculosis.
Left precrural gland.	—	—	2,324	Died. 32 days	Tuberculosis of omentum (slight), spleen, liver, sternal glands, and most of the abdominal lymphatic glands.
Muscle - -	—	—	2,018	Died, 25 days	One ventral mediastinal gland was caseous (T.B.). There was no disease elsewhere.
			2,019	Killed, 34 days	No tuberculosis.
Marrow - -	2	None	—	—	—

CAT 70.

Inoculated subcutaneously in the left groin, on April 26, 1906, with 10 mg. of culture of Virus B. V.

Killed April 27, 1906, 24 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	8	8	1,973	Died, 24 days	Slight tuberculosis.
Spleen - -	2	1	—	—	—
Marrow - -	1	None	—	—	—

CAT 72.

Inoculated subcutaneously between the shoulders on May 14, 1906, with 10 mg. of culture from a slightly virulent virus.

Killed, May 15, 1906, 24 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	2	None	2,029	Killed, 30 days	Slight tuberculosis.
Spleen - -	2	Numerous	—	—	—
Lung - -	2	None	—	—	—

KITTEN 90.

Inoculated subcutaneously in the left groin on June 6, 1906, with 10 mg. of culture derived from the original material of Virus B. XXVI.

Killed, June 7, 1906, 24 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally.)		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	3	—	2,068	Killed 34 days	Slight tuberculosis, tubercle bacilli found in an omental tubercle.
			2,069	Died 19 days	There was one tubercle in the omentum, in which tubercle bacilli were found, and no sign of disease elsewhere.
Spleen - -	2	20	2,070	Killed 34 days	Slight general tuberculosis.
Marrow - -	2	None	—	—	—

Inguinal glands: A smear preparation from an inguinal gland of the kitten showed numerous tubercle bacilli.

RABBIT 926.

Inoculated subcutaneously between the shoulders on June 26, 1906, with 20 mg. of culture of a slightly virulent virus.

Died, June 27, 1906, in less than 24 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pig Inoculated.		
	Number of Tubes sown.	Number of Colonies observed.	Number.	Duration of Life.	Result.
Spleen - -	3	3	2,117	Died, 42 days.	General tuberculosis.

RABBIT 832.

Inoculated subcutaneously in the left groin on April 25, 1906, with 10 mg. of culture of Virus B. V.

Killed, April 26, 1906, 24 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	2	None	1,972	Killed, 28 days	There was a caseous nodule in the muscle wall and two miliary tubercles in the omentum; one iliac gland was enlarged but not opaque; there was no disease elsewhere. T.B. found in nodule in muscle wall.
Spleen - -	1	Contaminated	—	—	—
Marrow - -	1	None	—	—	—

RABBIT 809.

Inoculated subcutaneously in the left groin on March 22, 1906, with 10 mg. of culture of a slightly virulent virus.

Killed, March 24, 1906, 48 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	2	None	1,953	Killed, 34 days	General tuberculosis.
Spleen - -	1	6	—	—	—
Marrow - -	1	None	—	—	—

GUINEA-PIG 1970.

Inoculated subcutaneously in the left groin, on April 25, 1906, with 10 mg. of culture of Virus B. V.

Killed, April 26, 1906, 24 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	2	None	1,971	Killed, 28 days	Slight tuberculosis.
Spleen - -	2	1	—	—	—
Marrow - -	1	Contaminated	—	—	—

GUINEA-PIG 1948.

Inoculated subcutaneously in the left groin on March 22, 1906, with 10 mg. of culture from a slightly virulent virus.

Killed, March 24, 1906, 48 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood -	2	None.	1,952	Killed 34 days	General tuberculosis.
Spleen -	1	20	—	—	—
Marrow - -	1	2	—	—	—

GUINEA PIG 1949.

Inoculated subcutaneously in the left groin on March 22, 1906, with 10 mg. of culture from a slightly virulent virus.

Killed, March 23, 1906, 24 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea Pig.	Duration of Life.	Result.
Blood - -	2	None	1,950	Killed, 35 days	Slight general tuberculosis.
Spleen - -	1	2	—	—	—
Marrow - -	1	Contaminated	—	—	—

RAT 89.

Inoculated subcutaneously in the thigh on December 12, 1905, with 5·0 mg. of culture from Virus B. XVIII.

Killed December 13, 1906, 24 hours after inoculation.

Material used.	Cultures.		Guinea-Pig Inoculated (Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	–	None	1,800	K. 22 days	Slight tuberculosis.
Spleen - -	1	10	—	—	—
Marrow - -	–	None	—	—	—

RAT 90.

Inoculated subcutaneously in the thigh on December 12, 1905, with 5·0 mg. of culture from Virus B. XVIII.
Killed, December 14, 1906, 48 hours after inoculation.

Material used.	Cultures.		Guinea-Pig Inoculated (Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	2	1	1,805	D. 14 days	No tuberculosis.
Spleen - -	1	46	–	–	—
Marrow - -	1	None	—	—	—

CHICKEN 52.

Inoculated subcutaneously in the left groin on May 2, 1906, with 10 mg. of culture from Virus B. V.
Killed, May 3, 1906, 24 hours after inoculation.

Distribution of Bacilli.

Material used.	Cultures.		Guinea-Pigs Inoculated (all Intraperitoneally).		
	Number of Tubes sown.	Number of Colonies observed.	Number of Guinea-Pig.	Duration of Life.	Result.
Blood - -	2	7	1,987	Killed, 35 days	Slight tuberculosis.
Spleen - -	2	13	—	—	—
Liver - -	1	Numerous	—	—	-
Marrow -	1	None		—	—

REPORT ON THE INFLUENCE OF INDIVIDUAL SUSCEPTIBILITY AND OF DOSE ON THE SEVERITY OF AN EXPERIMENTAL INFECTION BY TUBERCLE BACILLI OF HUMAN ORIGIN.

Dr. L. COBBETT.

REPORT ON THE INFLUENCE ON THE SEVERITY OF AN EXPERIMENTAL INFECTION OF

(1) THE VARIATION IN THE CAPACITY FOR RESISTANCE TO TUBERCULOSIS OF DIFFERENT INDIVIDUAL ANIMALS, AND

(2) THE NUMBER OF TUBERCLE BACILLI INJECTED.

The object of the majority of the injections made here into various kinds of animals has been to ascertain as exactly as possible the comparative virulence of different tubercular viruses. Before exact conclusions could be drawn from these experiments, it was necessary to know what effect upon the result might be caused by differences in (1) the capacity for resistance of different individual animals, and (2) the quantity of tubercle bacilli injected. It also became of importance to know the smallest dose which could be used without these differences in capacity for resistance having a dominating influence upon the result.

(1) The Variation in the Capacity for Resisting Tuberculosis among Individuals of the Same Species of Animal.

(a) *Influence of Age.*

From some of the many experiments carried out at this farm evidence may be gathered confirmatory of the generally received opinion that among the individuals of a given species the capacity for resistance to tuberculosis increases with age. There can, I think, be no doubt that among bovines, calves are distinctly more susceptible than adults. Of this there is plenty of evidence. Moreover, younger calves are more susceptible than older ones. As a rule when two calves were given the same injection they were approximately of the same age and weight. When occasionally this was not so, the younger animal became more rapidly and severely affected. As examples of this one may refer to Calves 113 and 35 [H10.BS], one of which was nearly double as large as the other when injected, and became very much less severely affected with tuberculosis. Also the two calves, 531 and 555, may be compared with the young bull, 611 [H17. Sp. B], Calf 563 with the young bull, 525 [H 7.C.M.], and Calf 387 with Heifer 249 [H10 B.S.] There are no examples of an older animal being more severely affected than a younger one which received the same dose.

(b) *The Influence of Sex. Pregnancy and Lactation.*

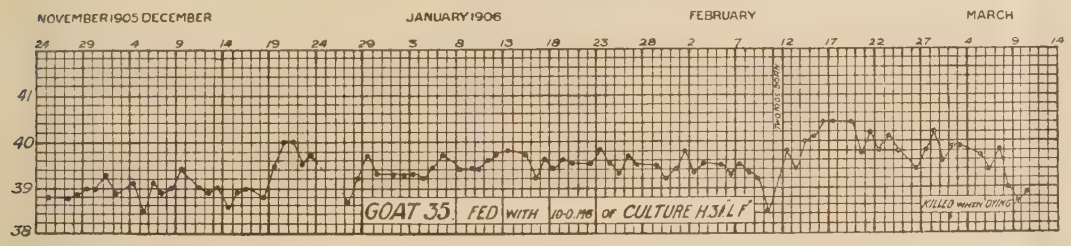
The influence of sex, on the susceptibility to tuberculosis.

Among calves no difference was observed in the capacity of the two sexes to resist tuberculosis.

A goat, No. 35, which was pregnant when fed with a virus of the human Group I, developed a progressing tuberculosis, and was in poor condition at parturition, 79 days after infection. Immediately after this it rapidly became worse, and a month later was killed when death was imminent. The temperature chart of this animal shows clearly the somewhat sudden change which took place after parturition. No cause for this change was observed other than parturition, unless indeed it was some affection of the udder owing to the fact that the kids were dead, and so could not take the milk of their mother. But the udder never became full, so that it was not even necessary to milk it by hand, and nothing whatever was noticed to be wrong with it during life. At the post-mortem examination both it and the uterus were found quite healthy.

The influence of pregnancy and the puerperium.

Chart of Goat 35, *shewing rise of temperature after parturition,*



The course of the disease in this animal supports the common belief that the resistance to tuberculosis is high during pregnancy and suddenly falls to a lower level after parturition. Unfortunately there was no non-pregnant goat infected at the same time with which to compare this animal.

There is little or no evidence confirmatory or otherwise of this belief to be gathered from the heifers which were pregnant when infected with tuberculosis. Of such there were six. In none of these did the condition of the animal undergo any marked change after parturition during the short period that most of them were allowed to live.

It is difficult to say whether pregnancy conferred any additional resistance on these animals, because in only two instances did non-pregnant animals receive identical injections, and so become available for comparison; and in these, unfortunately, the control animals were of a different breed, being Jerseys, while the heifers in calf were Shorthorns. The facts are as follows.

In the Shorthorn Heifer, No. 233, which calved two months after an experimental infection, and was killed four months later, the tuberculosis was minimal, like that in the non-pregnant Jersey Heifer [No. 247], which received an identical injection. It only differed in that in the former animal it had extended to the mammary gland.

In the next pair the tuberculosis was severe, and it was distinctly less acute in the Shorthorn [No. 235], which was in calf, than in the Jersey [No. 243], which was not. This may, of course, be explained on the hypothesis that pregnancy confers some extra capacity for resistance, but on the other hand there is some reason to think the Jersey is racially more susceptible to tuberculosis than the Shorthorn. In evidence of this belief one uncomplicated experiment only can be quoted, namely, that of the Shorthorn Heifer 231 and the Jersey Heifer 237, both injected subcutaneously with a tissue emulsion containing a small dose of the Virus H10. B.S. Neither animal was in calf. The Shorthorn remained well, and developed minimal scattered lesions only, while the Jersey developed severe tuberculosis.

Racial susceptibility; Jerseys and Shorthorns compared.

Thus from the experimental animals here one is not able to gather any sound evidence of the belief that pregnancy is associated with an increased capacity for resistance to tuberculosis. On the other hand, there is no evidence to the contrary.

While one is not able to arrive at any conclusion based on our own experiments as to the influence of pregnancy on the *general* susceptibility to tuberculosis, one can nevertheless point out some rather striking instances which show that those organs which then undergo a sudden increase in functional activity are remarkably liable to become affected during that period.

Susceptibility of organs undergoing a sudden accession of functional activity during pregnancy.

Susceptibility of the Mammary Gland.

The evidence is stronger in the case of the uterus than the mammary gland. Let us take the latter first. I have already mentioned that in Heifers 235 and 247

simultaneously injected with the same material tuberculosis was minimal, and differed in the two animals in that it had affected the mammary gland in the one which was pregnant when inoculated, but not in the other. But since the former lived four months after parturition it is possible that the mammary gland became affected during lactation. Another instance of tuberculosis spreading to the mammary gland in a pregnant heifer occurred in No. 251, in which likewise the tuberculosis elsewhere was minimal. In three other pregnant heifers, Nos. 235, 245, and 259, subcutaneously injected, the tuberculosis did not reach the mammæ. Nor did it reach the uninjected quarters in the pregnant cow, No. 295, which had been injected in the other quarters of the udder. In two of the former the disease elsewhere was severe and steadily progressing. In the others it was minimal.

Tuberculosis of the mamma occurred in a non-pregnant heifer, No. 239, which received a subcutaneous injection of a virus of Group 1. It developed very little tuberculosis elsewhere, but the udder became distinctly enlarged, and yielded a secretion as though it were about to become functionally active, and in this secretion tubercle bacilli were present. It was killed six months after injection, and at the post-mortem examination the gland was found hypertrophied, but there were no tubercles seen in it. Secretion taken from the milk ducts after death caused tuberculosis in guinea pigs which were injected with it.

Tuberculosis of the mammary gland does not commonly occur in calves, or in heifers which have not become pregnant, even among those which develop very acute general tuberculosis. In addition to the above-mentioned animal, it has indeed only occurred here four times, namely, in Heifer 249, and in Calves 477, 575, and 597, all injected subcutaneously. Seven other non-pregnant heifers which received a subcutaneous injection of a virus of Group 1 developed general tuberculosis, but the mammary gland remained normal.

To sum up—among nine heifers which were not pregnant when injected with a virulent virus, tuberculosis of the mammary gland occurred in two, while among five pregnant heifers it occurred twice. In one cow which was pregnant, which received an intramammary injection, tuberculosis did not spread to the non-inoculated quarters of the udder. Therefore the evidence that the mammary gland becomes more susceptible to tuberculosis during pregnancy is not very strong. The evidence that the uterus does so is far clearer.

The following table contains the tests made upon guinea pigs with the milk of [1] the five heifers which calved during an experimental infection produced by a subcutaneous inoculation; and [2] the uninoculated quarters of the udder of Cow 295, which calved after an intramammary injection of the other quarters; and includes also the test made upon guinea pigs with the mammary secretion of Heifer 239, which was not pregnant. The milk of Heifer 249 was not tested.

GUINEA-PIGS INJECTED WITH MILK OF HEIFERS PREGNANT WHEN INFECTED WITH TUBERCULOSIS.

Virus.	Number of bovine.	Interval between testing of milk and birth of calf.	Quarter of udder from which milk was taken.	Number of guinea-pig.	Killed or Died.	Duration of life in days.		Result.
						Intrap.	Subcut.	
H 14. "F.S."	Heifer 235	19 days after	L.F.	70	K.	79	—	No tuberculosis.
			„	71	„	—	79	„
			L.H.	72	„	79	—	„
			„	73	„	—	79	„
			R.F.	74	„	79	—	„
			„	75	„	—	48	„
			R.H.	76	„	79	—	„
			„	77	„	—	48	„
H 19. "S.W."	Heifer 233	14 days after	L.F.	93	K.	77	—	No tuberculosis.
			„	94	„	—	77	„
			L.H.	95	„	77	—	„
			„	96	„	—	58	„
			R.F.	97	„	77	—	„
			„	98	„	—	77	„
			R.H.	99	„	58	—	Tuberculous.
			„	100	„	—	34	„
		54 days after	L.F.	206	K.	61	—	No tuberculosis.
			„	207	„	—	61	„
			L.H.	208	„	61	—	Tuberculous.
			„	209	„	—	61	No tuberculosis.
			R.F.	210	„	61	—	„
			„	211	„	—	61	„
			R.H.	212	„	61	—	Tuberculous.
			„	213	„	—	28	„
		125 days after	L.F.	314	K.	95	—	No tuberculosis.
			„	315	„	—	95	„
			L.H.	316	„	95	—	Tuberculous.
			„	317	„	—	95	„
			R.F.	318	„	95	—	No tuberculosis.
			„	319	„	—	95	„

GUINEA-PIGS INJECTED WITH MILK OF HEIFERS PREGNANT WHEN INFECTED WITH TUBERCULOSIS—*cont.*

Virus.	Number of bovine.	Interval between testing of milk and birth of calf.	Quarter of udder from which milk was taken.	Number of guinea-pig.	Killed or Died.	Duration of life in days.		Result.
						Intrap.	Subcut.	
H 16. "J.H."	Heifer 245 -	6 days after -	L.F.	452	K.	64	—	No tuberculosis.
			„	453	„	—	64	„
			L.H.	454	„	64	—	„
			„	455	„	—	64	„
			R.F.	456	„	64	—	„
			„	457	„	—	64	„
			R.H.	458	„	64	—	„
			„	459	„	—	64	„
H 29. "M.F."	Heifer 251 -	3 days after -	L.F.	463	K.	55	—	No tuberculosis.
			„	464	„	—	55	„
			L.H.	465	D.	3/24	—	Cause of death unknown.
			„	466	K.	—	55	Tuberculous.
			R.F.	467	D.	54	—	„
			„	468	K.	—	55	„
			R.H.	469	D.	54	—	„
			„	470	K.	—	55	„
H 10. "B.S."	Heifer 259 -	2 days after -	L.F.	661	D.	95	—	No tuberculosis.
			„	662	K.	97	—	„
			L.H.	663	„	97	—	„
			„	664	„	97	—	„
			R.F.	665	„	97	—	„
			„	666	„	97	—	„
			R.H.	667	„	97	—	„
			„	668	„	97	—	„
		3 days after -	L.F.	669	K.	96	—	No tuberculosis.
			„	670	„	96	—	„
			L.H.	671	„	96	—	„
			„	672	„	96	—	„
			R.F.	673	„	96	—	„
			„	674	„	96	—	„
			R.H.	675	„	96	—	„
			„	676	„	96	—	„
H 10. "B.S."	Cow 295 - - Non-inoculated quarters only.	95 days before -	L.H.	38	K.	51	—	No tuberculosis.
			„	39	„	—	51	„
			R.F.	40	„	51	—	„
			„	41	„	—	51	„
		1 day before -	L.H.	274	K.	52	—	No tuberculosis.
			„	275	„	—	52	„
			R.F.	276	„	52	—	„
			„	277	„	—	52	„

GUINEA-PIGS INJECTED WITH SECRETION OF THE UDDER OF A HEIFER NOT PREGNANT WHEN INFECTED WITH TUBERCULOSIS.

Virus.	Number of bovine.	Interval between testing of milk and birth of calf.	Quarter of udder from which milk was taken.	Number of guinea-pig.	Killed or Died.	Duration of life in days.		Result.
						Intrap.	Subcut.	
H 19. "S.W."	Heifer 239	Not pregnant - Secretion obtained from udder after death.	L.F.	116	D.	7	—	No tuberculosis.
			L.H.	117	„	18	—	Tuberculous.
			R.F.	118	K.	19	—	„
			R.H.	119	D.	51	—	„

The Influence of Lactation on the Susceptibility of the Mammary Gland.

There is no complete evidence bearing on the susceptibility of the mammary gland to tuberculosis during *lactation*, owing to the absence of control injections of cows with resting udders.

But the very rapidly fatal tuberculosis which followed the injection of the milk ducts in some of our cows both here and at Walpole might be thought to be in favour of a great susceptibility of this organ when functionally active. On the other hand, when very small doses were used, as in Cow 74 at Walpole, and Cow 295 at Blythwood, the tuberculosis which followed was slow and in the latter case limited; so that on the whole the evidence on this point is inconclusive.

Susceptibility of the Pregnant Uterus.

The non-pregnant uterus has never been found affected with tuberculosis, no matter how severe the disease was elsewhere. The uterus has invariably been found healthy in heifer calves, and was so in the nine yearling heifers injected with viruses of Group 1, which did not become pregnant. On the other hand, three out of six heifers which were pregnant when injected developed tuberculosis of the uterus, and gave birth to tuberculous calves.† This is a remarkably high proportion, and is the more striking because in none of the three cases was the tuberculosis very acute, and in one, while the mammary gland was also affected, there was very little tuberculosis elsewhere.

Two of these heifers, No. 235 and 245, had slowly but steadily progressing severe tuberculosis of the perlsucht type, affecting chiefly the lungs, when killed 176 and 209 days respectively after subcutaneous injection. Another [No. 251] was in good condition when killed 138 days after subcutaneous injection, and nine days after calving. She was found to have a caseous nodule in the mammary gland, and active tuberculosis in the cotyledons of the uterus. The tuberculosis elsewhere was minimal.

Detailed information concerning these animals is given in the following table.

† See Report on congenital tuberculosisat Blythwood Farm, page 261.

TABLE SHOWING THE INFLUENCE OF PREGNANCY UPON TUBERCULOSIS OF UTERUS AND MAMMARY GLAND, GIVING ALL THE HEIFERS WHICH CALVED DURING AN EXPERIMENTAL INFECTION.

No. of mother.	Mode of inoculation.	Virus.	Interval between inoculation of mother and birth of calf.	Interval between birth of calf and death of mother.	Condition of great organs.	Condition of mamma.	Result of milk injections into guinea-pigs.	Condition of uterus.	Result of post-mortem on calf.
Shorthorn heifer 235	S.	H.14. "F.S."	153 days	23 days	Very advanced Perlsucht disease of lungs and pleura. Few tubercles in kidneys and one suprarenal. Minute ulcers in gut, and caseous tubercles in mesenteric glands.	Normal	All four quarters tested 19 days after birth of calf. No tuberculosis.	Early caseation in the cotyledons of the uterine mucous membrane.	Caseous patches and foci in hepatic glands. Emulsions of these glands, of liver, and of a pneumonic lobule in the lung, caused tuberculosis in guinea-pigs.
„ „ 233	S.	H.19. "S.W."	55 days	126 days	Very slight tuberculosis. Several caseous tubercles in thoracic glands and spleen. Three tubercular bodies on lung, loosely attached.	Induration of right hind quarters, in which tubercles are sparsely scattered.	All four quarters tested on several occasions. Guinea-pigs injected from both hind quarters became tubercular, those from fore quarters remained healthy.	Normal	Normal throughout.
Jersey heifer 245 -	S.	H.16. "J.H."	193 days	16 days	Extensive Perlsucht disease of lungs and pleura. Abdominal organs but slightly affected.	Normal	All four quarters tested 6 days after birth of calf. No tuberculosis.	Uterus appeared normal, but tubercle bacilli were found in a cotyledon.	Few foci in hepatic gland, in which tubercle bacilli were found.
„ „ 251 -	S.	H.29. "M.F."	129 days	9 days	Minimal tuberculosis. Small local lesion; calcareous foci in prescapular and thoracic glands. One tubercle in heart.	A caseous mass in left fore quarter. Other quarters appeared normal.	All four quarters tested 3 days after birth of calf. The milk of all the quarters except the left fore infected the guinea-pigs.	The cotyledons were large. From one, when cut, purulent material oozed out. This contained T.B. Besides this, numerous yellow foci were seen in the mucous membrane, in which T.B. were found.	Numerous early caseating foci in hepatic glands. Few early tubercles in lungs, liver, spleen, kidney, thoracic and lumbar glands.
„ „ 259 -	S.	H.10. "B.S."	409 days	4 days	Minimal tuberculosis. Calcareous nodule at seat of inoculation, one in prescapular gland. Two doubtful tubercles in left bronchial gland.	Normal	All four quarters tested on two occasions. Guineapigs remained healthy.	Normal	Normal throughout.
„ cow 295 - -	M.	H.10. "B.S."	178 days	1 day (died)	Small caseous tubercles, and a few larger nodules in the inoculated quarters of the mamma. A few pinhead foci in supra-mammary gland. No tuberculosis elsewhere.	Non - inoculated quarters normal.	All four quarters tested on several occasions. The milk from inoculated quarters only caused tuberculosis.	Normal	Normal throughout.

When one considers these three instances of tuberculosis of the pregnant uterus, giving an incidence of no less than 50 per cent. among the pregnant animals, and contrasts it with the total absence of tuberculosis of that organ among the heifers which were not in calf when injected, many of which were much more severely and acutely affected than these three, one cannot, I think, escape the conclusion that pregnancy greatly increases the susceptibility of the uterus to tuberculosis.

Variation in the capacity for resistance of differential organs.

Before leaving this part of the subject it may be interesting to mention that, quite apart from pregnancy or lactation, the relative susceptibility of the different organs often appeared different in different animals. Thus of the animals which succumbed to acute tuberculosis some died of respiratory trouble; and in them the brunt of the disease was found to have fallen upon the lungs. Others died without dyspnœa being a marked symptom; and in these the lungs still contained a large amount of functional tissue, but the liver and spleen were more affected than in the animals first mentioned. A good example of this difference is afforded by Calves 967 and 969, H10. B.S., which both received 10 milligrammes of the same culture, and both of which had reached a dying condition on the same day, the one with very severe disease of the lungs, the other with plenty of functional lung tissue left, but with more severe disease of other organs than its fellow. Many other instances might be mentioned.

(c) *The Influence of Intercurrent Disease on Susceptibility to Tuberculosis.*

During the course of our work there have been many animals infected with tuberculosis, and some of them have suffered also from other diseases and intercurrent disorders. It is, of course, commonly believed that some diseases, as for example influenza, or even some slight departure from the normal state of health, diminishes the power of resisting tuberculosis. It is therefore of interest to inquire whether these animals infected with intercurrent disease showed any diminished power of resisting tuberculosis. This inquiry is rendered possible because in almost every case of experimental infection of the calf two animals were given identical doses. If then one suffered from an intercurrent disorder, it is possible to compare the course of the tuberculosis in it with that in its fellow. One must, of course, be on one's guard against differences which are due to differences of inherent individual susceptibility, by which I mean differences which are independent of any temporary departure from health. This subject will be dealt with in the next section.

Among the cases of intercurrent disease or disorder which affected one of two animals which received identical injections were many of more or less protracted diarrhœa, sometimes severe enough to cause a marked check to growth, and in one case [Calf 769. H45. F. M.] even to cause death. There were also many cases of more or less extensive carnification of the anterior parts of the lung, caused presumably by an old bronchial obstruction and consequent collapse. In one case there was death from pneumonia [Calf 619. H12. H. N.]. In another the ureters were blocked by gravel and there was acute hydronephrosis of both kidneys. In yet another [Calf 351. H12. H. N.] gravel caused several attacks of renal colic, the nature of which were not at first understood, and ultimately led to blocking of the urethra and extravasation of urine. In one calf an intravenous injection of an emulsion of a softened tubercular tumour from another calf caused both tuberculosis and pyœmia.

Only one of these animals [Calf 877. H28. C. L.] was injected with a virus of the human Group I., which is virulent for bovines. This was the calf which had hydronephrosis from blocking of the ureters. It died a little sooner than its fellow, having general tuberculosis, but death was thought to be due directly to the state of the urinary apparatus.

The other calves, which have been mentioned as having suffered from some intercurrent disease, were all injected with viruses of the human Group II. Even the intercurrent disease did not enable the latter to cause a progressive tuberculosis, nor did it have, as a rule, any obvious effect on the course of the tuberculosis. In Calf 769, which died of diarrhœa 17 days after subcutaneous injection of the virus H45. F. M., tubercle bacilli, it is true, were found widely distributed, but subsequent investigation showed that this was no exception to the general rule. (See the report on the distribution of non-virulent bacilli in the calf. p. 219.) In Calf 955, which had received a subcutaneous injection of 100 milligrammes of culture of Virus H.56. "F. T.", there was found rather extensive carnification of the anterior lobes of both lungs, and atrophy of a considerable portion of one kidney, associated with a deposit of triple phosphate in the corresponding calyces. The animal was never well, and scarcely gained in weight during three months, and yet the tubercular disease had not extended beyond the prescapular lymphatic gland, and was no more severe than in the fellow calf, which had remained well, and grown normally. In Calf 619, which died of pneumonia 83 days after subcutaneous injection of 50 mg. of culture, many little calcareous foci were found, but as these were retrogressive lesions and the pneumonia was recent, they could not be ascribed to its influence, and moreover it seems only reasonable to regard this as one of the more susceptible animals, among which such lesions are not uncommonly produced by viruses of the human Group II. Calf 351 is an interesting instance, because the dose had been enormous (3,140,000,000 bacilli), and in spite of this and the chronic disease of the kidney, the tuberculosis had made no progress beyond the glands nearest the seat of injection. In 409, which had received an intravenous injection, it is possible that the pyœmia may have assisted the tubercle bacilli to multiply in the body, for they were certainly very numerous everywhere—more numerous, I think, than one usually finds after an intravenous injection of a virus of the human Group II.—and had extended to the pyæmic joints.

Many of the rabbits suffered from severe coccidiosis of liver, which sometimes proved fatal; others from an extreme degree of infection with cysticerci, which not infrequently was found to have caused more or less chronic peritonitis and perihepatitis. Neither of these conditions so weakened the resistance of the animal as to enable viruses of the human Group II. to cause a progressive infection, nor did it appear that they had any influence on the severity of an infection produced, either by viruses of this group, or of the virulent Group I.

On the whole it may be said that there is but little evidence to be derived from our experiments here, in favour of the view that the course of tuberculosis is greatly influenced by intercurrent disease, or temporary disorders. On the contrary, it would appear that the resistance to invasion by tubercle bacilli is of a special kind, and not dependent to any great extent upon the *general* state of health of the body.

The evidence of this is, however, far from conclusive. The cases of intercurrent disease in which the virulent viruses of Group I. have been used are too few in number. Really trustworthy negative evidence could only have been obtained from animals injected with virulent viruses in minimal doses. And of such injections there have not been many. And none of them have presented cases of intercurrent disease.

Is a Diseased Part less able to resist Tuberculosis than a Corresponding Healthy Part of the same Animal?

As soon as it became known that after a subcutaneous injection of a virus of the human Group II. into the calf bacilli are distributed all over the body, and that they slowly disappear, it was obvious that one might put to the test of experiment the question whether the tubercle bacilli are more readily destroyed in a healthy than in a diseased part. The most convenient means of doing this was afforded by the frequent instances of carnification of lung. One might inject guinea pigs with approximately equal quantities of emulsion of carnified and normal lung from a calf which had previously been injected subcutaneously with one of the viruses of the human Group II.

This was done in two instances, namely, from Calf 955 and Calf 941. In the former case both the emulsions caused tuberculosis in the guinea pig, that made from the apparently normal part of the organ causing death first. In the latter case the emulsion of apparently normal lung caused tuberculosis, while that made from from the carnified lung killed the guinea-pigs which received it with acute peritonitis ten days later, without any tubercle bacilli being found in the exudation.

Thus the carnified lung does not appear to be more susceptible to tuberculosis than normal lung; but it must be admitted that up to the present the evidence on this point is of little or no value.

Variation in Capacity for resisting Tuberculosis in Normal Individual Animals.

Having dealt with the differences in the capacity for resisting tuberculosis, if any, which are due to differences of age, sex, and condition of general health, as well as with the differences of resistance exhibited by different organs in varying states of functional activity and of health, we now pass on to consider those differences which appear in healthy individuals of the same age, differences which cannot be attributed to any disturbance of general health, nor to any defect in any particular part or organ, which cannot be explained, but which are known as differences in the individual capacity for resistance. For, as we recognise that different species of animals differ in resistance capacity, so also do different individuals, though not to the same extent.

Differences of resisting power of a more or less marked kind among individuals have come to light from time to time among the large numbers of calves and other animals which have been injected in pairs, each individual of a given pair receiving exactly the same dose as the other, and being of the same age and weight. In such cases if there is no intercurrent disease, and the conditions under which the animals are kept are identical, one must attribute any differences in the course and severity of the disease to differences of individual resistance capacity.

I do not propose to dwell on the numerous instances where viruses of the human Group II. were used; for since these any incapable of setting up a progressive tuberculosis in any calf, they do not serve to bring out individual differences of resistance capacity in these animals strongly. Concerning these it is sufficient to recall the fact that many calves developed a few or more little translucent non-progressive tubercles in their lungs or other organs, while their fellows did not, and that the presence of these little bodies was thought to indicate a lower degree of resistance power than the average.

Evidence of Variation in the Capacity of Resistance to Tuberculosis among Calves afforded by Animals Injected with *Tissue Emulsions* of the Human Group I.

Passing on then to the virulent viruses of Group I., we may consider first the evidence afforded by calves injected with *tissue emulsions.*

The following table contains all the instances of two animals receiving precisely similar injections of this kind. The only possible comparison is between the two individuals of the same pair, because different pairs received not only different viruses, but different doses also. All the pairs of animals are given, whether they show any difference in resistance capacity or no, in order that the judgment may be formed of the relative frequency and importance of these departures from the normal power of resistance. In order to facilitate the study of this table, animals which differ markedly in resistance capacity are printed in italics.

TABLE CONTAINING ALL THE PAIRS OF CALVES, EACH MEMBER OF WHICH RECEIVED AN IDENTICAL DOSE OF A VIRULENT EMULSION OF A TUBERCULOUS TISSUE.

SHOWING THE EVIDENCE OF VARIATION IN THE CAPACITY FOR RESISTING TUBERCULOSIS OF DIFFERENT INDIVIDUAL CALVES.

Virus.	Dose in estimated numbers of bacilli.	No. of calf.	Weight in kilos.	Duration of experiment, and condition at death.	Result.	No. of calf.	Weight in kilos.	Duration of experiment, and condition at death.	Result.
H. 29, "M.F." - -	3,020,400,000	479	58·05	Killed when dying 32 days	G.T. 3	477	83·0	Killed when dying - 34 days	G.T. 3
H. 28, "C.L." - -	901,466,000	513	65·75	Killed when very ill - 25 „	G.T. 2	515	50·80	„ „ „ - 44 „	G.T. 3
H. 32, "Y.W." - -	654,160,000	537	79·38	Killed when dying - 33 „	G.T. 3	533	62·13	„ „ „ - 35 „	G.T. 3
H. 14, "F.S." - -	362,000,000	125	48·50	„ „ „ - 23 „	G.T. 3	121	46·70	Killed when very ill - 36 „	G.T. 3
H. 17, Sp. "B." - -	150,520,000	555	74·85	Died - - - - 58 „	G.T. 3	531	71·21	Killed when dying - - 59 „	G.T. 3
H. 31, "L.F." - -	5,979,000	485	78·02	Killed when well - - 151 „	G.T. 1	491	71·21	Killed when well - - 152 „	G.T. 1
H. 16, "J.H." - -	*3,063,000*	*355*	*48·95*	*Killed when dying - - 43 „*	*G.T. 2*	*337*	*48·05*	*„ „ „ - - 80 „*	*G.T. 1 or 2*
H. 20, "F.L." - -	*2,301,000*	*213*	*92·96*	*Killed when well - - 169 „*	*G.T. 1*	*223*	*88·44*	*„ „ „ - - 169 „*	*L.T.*
„ „ - -	*2,301,000*	*517*	*76·67*	*Showed some illness - 172 „*	*G.T. 2*	*507*	*55·79*	*„ „ „ - - 172 „*	*S.T. or G.T. 1*
H. 19, "S.W." - -	*1,546,000*	*159*	*60·77*	*Killed when ill - - 50 „*	*G.T. 3*	*179*	*62·13*	*„ „ „ - - 114 „*	*S.T.*
H. 2, Sp. "A." - -	*301,000*	*83*	*73·9*	*Killed when dying - - 134 „*	*G.T. 2*	*91*	*82·6*	*„ „ „ - 147 „*	*G.T. 1*
H. 7, "C.M." - -	*250,000*	*141*	*71·21*	*„ „ „ - - 53 „*	*G.T. 3*	*139*	*63·04*	*Killed when ill - - 85 „*	*G.T. 2*
„ „ - -	*48,000*	*105*	*34·9*	*Killed when ill - - 47 „*	*G.T. 3*	*103*	*41·27*	*Killed when well - - 130 „*	*G.T. 1*
„ „ - -	12,000	115	56·69	Killed when well - - 96 „	G.T. 1	117	63·50	Killed when well - - 97 „	G.T. 1
H. 10, "B.S." - -	*5,000*	*113*	*81·64*	*Killed when ill - - 120 „*	*G.T. 2*	*35*	*128·81*	*Killed when well - - 106 „*	*S.T.*
H. 2, Sp. "A." - -	*Unknown*	*153*	*62·58*	*„ „ „ - - 39 „*	*G.T. 3*	*163*	*53·52*	*„ „ „ - - 64 „*	*S.T.*

NOTE.—(*a*) The details of each pair of injections into calves which received identical doses, and which may therefore be compared with one another, are placed in a single line across the page. The italics are to draw attention to the instances where the difference in the result was greatest

(*b*) In this table. { G.T. 1. = General Tuberculosis, slight.
G.T. 2. = „ „ moderate.
G.T. 3. = „ „ severe.

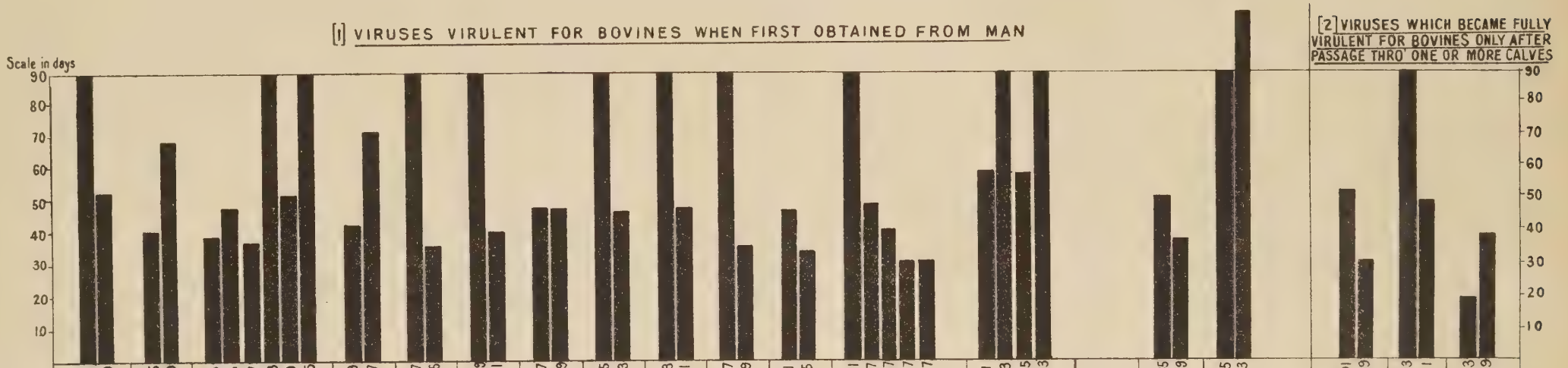
STRAINS OF HUMAN ORIGIN.

CHART SHOWING THE LENGTH OF LIFE in DAYS OF CALVES AFTER SUBCUTANEOUS INJECTION OF 10 MILLIGRAMMES OF CULTURE

[ALL CALVES WHICH REACHED THE 90 DAY LIMIT WERE KILLED WHEN IN GOOD HEALTH, WITH THE EXCEPTION OF Nº 953, (H.49. "T.C") WHICH DIED AFTER 108 DAYS AND OF Nº 1123 (H64 "M.G") WHICH WAS IN POOR HEALTH WHEN KILLED]

[1] VIRUSES VIRULENT FOR BOVINES WHEN FIRST OBTAINED FROM MAN

[2] VIRUSES WHICH BECAME FULLY VIRULENT FOR BOVINES ONLY AFTER PASSAGE THRO' ONE OR MORE CALVES



Nº of Calf	883 889	835 839	813 815	877 863 819 875	939 947	827 845	1029 1041	967 969	895 893	913 921	867 879	851 855	991 987	1037 1057 1077	1111 1113 1105 1123	1085 1089	945 953	901 919	823 821	933 929
Strain	Human Sputum through Calf 93	Human Cervical glands, thro' G.P. 1329	Human Cervical glands direct	Human Cervical glands, through Rabbit 102	Human Cervical glands, direct	Human Cervical glands, thro' G.P. 793	Human Mesenteric glands through Calf 5 G.P. 448	Human Mesenteric glands through Calf 35 G.P. 725	Human Mesenteric glands through G.P. 587	Human Mesenteric glands through Goat 11.	Human Mesenteric glands through Calf 213.	Human Bronchial glands through Rabbit 166	Human Lung direct	Cervical glands direct / Brain through G.P. 1860 / Mesenteric glands through G.P. 1849	Human Mesenteric glands through G.P. 1910 / Human Meninges through G.P. 1880	Human Mesenteric glands through G.P. 1835	Human Mesenteric glands direct	Human Sputum, Calf 553, G.P. 1542 [5th Passage]	Human Bronchial glands, through Calf 321 [2nd Passage]	Human Mesenteric glands, through Calf 797 [1st Passage]
Virus	H.2 "Sp.A"	H.29 "M.F."	H. 28 "C.L."		H.31 "L.F."	H.38 "J.M."	H.7 "C.M"	H.10 "B.S"	H.14 "F.S."	H 19 "S.W"	H 20 "F.L."	H.32 "Y.W"	H.59. "L.B".		H 64 "M.G."	H.65 "K.B"	H.49. "T.C".	H 17 "Sp.B"	H.13 "A.D."	H.49 "T.C"

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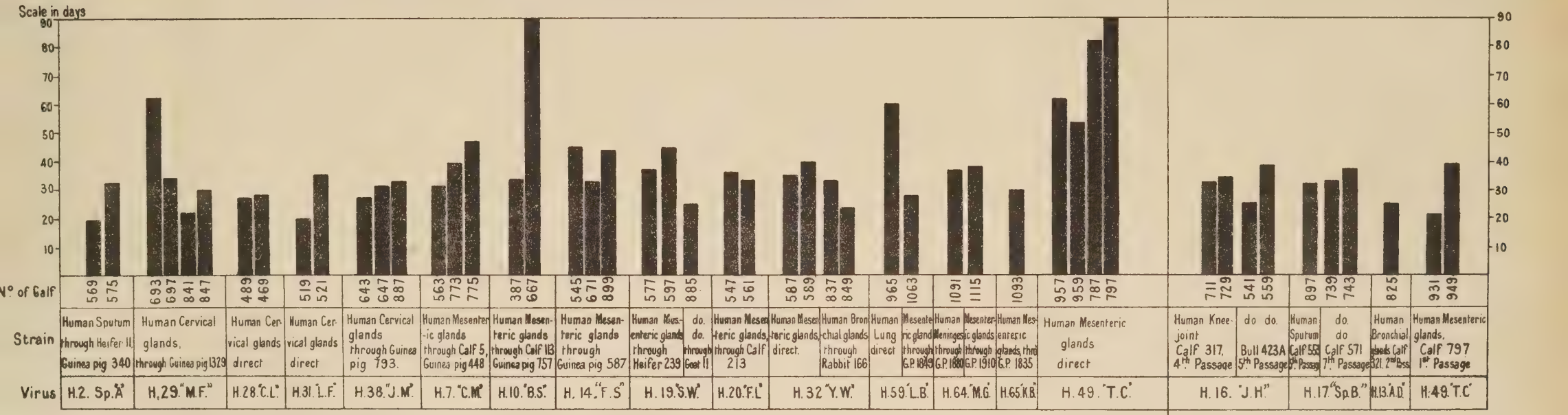
CHART SHOWING THE LENGTH OF LIFE IN DAYS OF CALVES AFTER SUBCUTANEOUS INJECTION OF 50 MILLIGRAMMES OF CULTURE †

HUMAN ORIGIN.

[A] VIRUSES VIRULENT FOR BOVINES WHEN FIRST OBTAINED FROM MAN.

[B] VIRUSES WHICH BECAME FULLY VIRULENT FOR BOVINES ONLY AFTER PASSAGE THROUGH ONE OR MORE CALVES *



N° of Calf	Strain	Virus
569 575	Human Sputum through Heifer II. Guinea pig 340	H.2. Sp."A"
693 697 841 847	Human Cervical glands, through Guinea pig 1329	H.29."M.F."
489 469	Human Cervical glands direct	H.28."C.L."
519 521	Human Cervical glands direct	H.31."L.F."
643 647 887	Human Cervical glands through Guinea pig 793.	H.38."J.M."
563 773 775	Human Mesenteric glands through Calf 5, Guinea pig 448	H.7. "C.M."
387 667	Human Mesenteric glands through Calf 113 Guinea pig 757	H.10."B.S."
545 671 899	Human Mesenteric glands through Guinea pig 587	H. 14."F.S"
577 597 885	Human Mesenteric glands through Heifer 239 / do. do. through Goat II	H. 19."S.W."
547 561	Human Mesenteric glands through Calf 213	H.20."F.L"
587 589	Human Mesenteric glands, direct.	H. 32 "Y.W."
837 849	Human Bronchial glands through Rabbit 166	
965 1063	Human Lung direct / Mesenteric gland through G.P. 1849	H.59."L.B."
1091 1115	Human Meningeal through G.P. 1880 / Mesenteric glands through G.P. 1910	H.64."M.G."
1093	Human Mesenteric glands, thro G.P. 1835	H.65."K.B."
957 959 787 797	Human Mesenteric glands direct	H. 49. "T.C."
711 729	Human Knee-joint Calf 317, 4th Passage	H. 16. "J.H."
541 559	do do. Bull 423A 5th Passage	
897 739 743	Human Sputum Calf 553 5th Passage / do. do Calf 571 7th Passage	H.17."Sp.B."
825	Human Bronchial glands Calf 321. 2nd Pass.	H.13."A.D."
931 949	Human Mesenteric glands, Calf 797 1st Passage	H.49."T.C"

† Calf 512 received 30 milligrammes only, Calf 469, 43 milligrammes only, and Calf 1091, 45 milligrammes only, and Calf 887, 23 milligrammes only. Calf 667 which reached the 90 day limit, was in good health when killed, and was found to have general tuberculosis of moderate severity Calf 797 also was killed when in good health and was found to have slight tuberculosis only. All the others died or were in a dying condition when killed & were found to have severe general tuberculosis.

* H.49."T.C." when obtained from man (see calves 957 to 797) was slightly less virulent for Calves than other virulent viruses The other viruses of this group were originally incapable of causing a progressive tuberculosis in calves.

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The most marked instances of difference in resistance capacity occurred in Calves 153 and 163 [H2, Sp. A]. Calves 159 and 179 [H19, S. W.] and 213 and 223 [H20, F. L.]. The severity of the disease also differed in Calves 35 and 113 [H10, B. S.], but this seemed attributable to difference in age or size.

Nos. 153 and 163 received each 30 c.c. of an emulsion of the tuberculous organs of a guinea-pig. The former was killed when very ill 39 days later, and was found to have severe general tuberculosis. The latter remained well, and when killed 64 days after injection was found to have no lesions beyond the thoracic glands. In the second case Nos. 159 and 179 received each a dose of 10 c.c. of an emulsion of human mesenteric gland not very rich in tubercle bacilli. No. 159 was killed when ill, 50 days after injection, and was found to have severe general tuberculosis. The other was quite well when killed four months after injection, and had only, besides a scar at the seat of injection and a calcareous prescapular gland, a few minute foci scattered in the thoracic, hepatic, and mesenteric glands. In the last mentioned case Calves 213 and 223 each received 10 c.c. of an emulsion poor in bacilli. The former had a moderate amount of disease in the lungs and thoracic glands when killed; the latter only a minimal local lesion and a few calcareous grains in the nearest glands. It must be remembered that these instances represent extreme differences. In them the doses were relatively small. With large doses the results, as a rule, in two calves which received the same dose were very fairly uniform.

Evidence of Variation in the Capacity of Resistance to Tuberculosis among Calves Afforded by Animals Injected with *Cultures* of the Human Group I.

The animals which were injected with culture fall into two groups, namely, those which received 50 mg. and those which received 10 mg. of bacilli subcutaneously. The majority of these animals died of tuberculosis or were killed when they were in a dying condition, the duration of life may therefore be taken as a rough relative measure of the capacity for resistance. In the few cases where the animals were killed when in more or less good general condition three months after injection, this period may be taken as the measure of maximum resistance capacity.

A strict comparison, of course, is only possible between the members of a group all of which received the same dose of the same culture, but since the differences of virulence among the viruses of this group, if they existed, were not marked enough to reveal themselves in our experiments,* H49. T. C. alone excepted, a rough comparison is possible between all the calves which received the same doses.

The evidence as to the existence of variation in capacity for resistance to tuberculosis among these calves is best brought out by the diagrams which appear on p. of this appendix, and in which the duration of life of each calf is shown by a column of proportionate height. If this be consulted it will be seen, among the calves injected with 50 mg. (those injected with H49 T. C. excepted), the duration of life was remarkably uniform, one calf alone living out the period of 90 days*, while all the rest were dying of tuberculosis on an average period of 33·5 days; the shortest being 20 and the longest 63 days.

If one compares together the duration of life of the two individuals of each pair one finds a remarkably close agreement, the widest discrepancy occurring with H10. B. S., where one animal, already mentioned, was killed when in good condition, after three months, and the other when dying on the 34th day.

We may conclude, therefore, that 50 mg. of a culture of the human Group I., when subcutaneously injected, almost invariably produces rapidly fatal tuberculosis in the calf, and is too large a dose to allow individual differences of resistance power to become manifest.

This is not the case when 10 mg. is the dose. Differences of individual resistance capacity are then often great enough to turn the scale from death to recovery. Thus of 41 calves injected with this dose of virulent culture* 30 died, or were killed when dying, in an average period of 44½ days, while 11 were in more or less good condition when killed three months after injection, and in some of these latter the lesions were only of very slight or of moderate extent. Evidence obtained from 10 mg. injections.

Comparing together the duration of life of the two individuals forming each pair, one finds that in eight of the pairs both animals died, or were killed when dying; while in eleven one animal died of general tuberculosis, while the other lived out 90 days, as already stated, and was not very severely affected.

Thus in our experiments with 10 mg. doses the highly resistant individuals were remarkably uniformly distributed among the calves injected with different viruses. This was very fortunate; for it is obvious that if the resistant animals had come together, the results might have been very misleading.

From these considerations it is evident: *That there are considerable differences in individual powers of resistance among different calves;*

That these come more into evidence with 10 *milligramme than with* 50 *milligramme doses;*

That the differences of individual resisting power among calves are such that 10 *milligramme injections cannot be trusted to reveal small differences of virulence among different tubercular viruses.*

(2) THE INFLUENCE OF DOSE.

The influence of the number of tubercle bacilli injected, on the severity of the resulting disease, became apparent at the commencement of our work, and a series of guinea pigs injected by me at Walpole showed that the duration of life after injection bore a very close relation to the quantity of tuberculous material injected, whether this was introduced into the subcutaneous tissue or into the peritoneum. It was not, however, until the results of injecting bovine animals began to accumulate that the full effect of variations of dose became apparent, and it was evident that a tubercular virus which was capable of killing with certainty bovine animals within a few weeks when injected in large doses, would cause only minimal lesions, limited and non-progressive in these animals, if only a small dose were given. It will be sufficient to quote a few instances.

In the case of the culture H. 10 "B.S.," Heifer 249 and Calf 387 were subcutaneously inoculated each with 50 milligrammes of culture, while Heifer 259 and Calf 385 received of the same culture minimal qualities calculated to contain 1,800,000 tubercle bacilli. The two first-mentioned animals developed an acute general tuberculosis, which was rapidly approaching a fatal termination when they were killed 54 and 34 days respectively after injection; the other two, which received the small doses, remained well, and when killed 413 and 202 days later respectively, were found to have only small calcareous nodules at the seat of injection, and a few minute calcareous foci elsewhere.†

The difference of dose was very large in this instance, in the following it was smaller. An emulsion of the prescapular gland of Calf 355 (virus H. 16, "J.H.") was injected subcutaneously in various doses into four calves. The numbers of tubercle bacilli estimated in the qualities of emulsion injected were as follows: Calf 281, 10,900,000; calf 317, 3,000,000; Calf 343, 712,000; and Calf 371, 10,000. In the first three animals acute general tuberculosis resulted, and a fatal termination was approaching when the animals were killed, No. 281 54 days after the injection, and Nos. 317 and 343 in 80 and 68 days respectively. The one which received the smallest dose remained well, and only a few small non-progressive calcareous foci were found in various organs in addition

* The actual culture injected into this animal was probably at fault, for rabbits also injected with it lived much beyond the usual period. That the strain was not attenuated was shown by the injection of calves and rabbits at a later period. These died in the usual time.

* H49 T. C. again excepted.

† For a full description of the post-mortem examinations and other details of the experiments mentioned here, *see* Vol. II. of the Appendix.

to a calcareous local lesion and prescapular gland when the animal was killed 66 days after injection.

In the case of H. 13, "A.D.," three calves were subcutaneously injected with varying quantities of the same emulsion of tubercular tissue from the prescapular gland of Calf 301. The doses were 500 million, 3½ million, and 15 thousand tubercle bacilli respectively. No. 325, which received the largest dose, was killed when dying of general tuberculosis 24 days after injection. No. 321 was killed when very ill 63 days after receiving 3½ million, and was found to have general tuberculosis, while No, 315 which received only 15 thousand, remained well, and when killed 70 days later was found to have no more than a small calcareous lesion at the seat of injection, and very few minute and non-progressive foci elsewhere.

These examples will be sufficient to prove that minute doses do not produce progressive tuberculosis in calves and heifers, even when large doses of the same cause rapidly fatal tuberculosis.

We now pass on to the evidence of the influence of dose which is given by the culture injections. In these the differences were much smaller than in those already mentioned, yet they will be seen nevertheless to have had a marked influence on the result.

In the diagrams already referred to when writing of susceptibility, there are forty-eight calves injected with 50 milligrammes, one with 43, and one with 30 milligrammes, of virulent culture, and forty-three injected with 10 milligrammes of the same. If we exclude the strain H49. "T.C." derived directly from human mesenteric glands, as being probably of slightly lesser virulence than the others, there is, as already stated, no evidence that they differed from each other in virulence, at least they did not do so to an extent which became apparent in the course of these experiments.

In estimating the influence of dose, we may therefore compare the calves injected with 10 milligrammes as a whole with those injected with the larger quantity. The larger dose was fatal to all the forty animals, except one which survived 90 days, while eleven out of the forty-one animals injected with the smaller dose lived three months, and were then in very fair condition. Moreover, with few exceptions, those which died after an injection of 10 milligrammes lived longer than those which received 50 milligrammes of the same culture. The average duration of life of those which succumbed to 50 milligrammes was 33·7 days, while that of those which succumbed to 10 milligrammes was 44·5 days. These differences in the results of injecting 10 and 50 mg. are very clearly shown in the diagrams.

Differences in individual resisting power, however, sometimes had a more powerful result on the severity of the disease than even this difference of dose; and in half the cases where a pair of 10 milligramme calves may be compared with a pair of 50 milligramme calves, the more susceptible of the two former has died in about the same time as the less susceptible of the latter. In one instance the more susceptible 10 milligramme calf died before either of the 50 milligramme animals, while the remaining 10 milligramme calf died at the same time as the remaining 50 milligramme animal.

In rabbits, too, the number of tubercle bacilli injected influences the severity of the disease produced. Rabbits have been injected intraperitoneally with different doses of cultures of all our virulent viruses. These have usually included, 10, 1, 0·1, and 0·01 milligrammes, and the resulting disease, though always fatal, has been the more prolonged, the smaller the dose. This rule has not been without exceptions, differences in individual susceptibility in these animals, as in the calves, not unfrequently overcoming the influence of numbers of bacilli. For the details of the evidence upon which this statement rests, the table of culture injections into rabbits may be consulted.

In one important respect the influence of dose is different in the rabbit and the bovine race. In the latter, injected with a virus of the human Group I., the disease may cease to spread and the animal recover, if the dose be small enough. In the former it appears that there is no dose so small that it will not produce a progressive tuberculosis.

Differences of Pathogenicity between Cultures of Tubercle Bacilli and Emulsions of Tuberculous Tissue.

It will be quite clear from the instances given above that the number of tubercle bacilli injected have a marked influence on the course of the resulting disease when these bacilli happen to be virulent for the class of animal to which the recipient belongs. But there are other factors included in the term dose besides the numbers of tubercle bacilli and their comparative virulence.

In our experience, in order to cause severe tuberculosis with virulent cultures, it is necessary to inject far larger numbers of bacilli than when emulsions of tuberculous tissue are used. In the chapter on the stability of virulence of tubercle bacilli in artificial culture it is estimated that that bacilli injected along with the tuberculous tissue in which they have developed are at least as effective as 100 times as many from an artificial culture of three weeks' growth.

REPORT ON CONGENITAL TUBERCULOSIS AT BLYTHWOOD FARM.

Dr. L. COBBETT.

Many guinea-pigs and rabbits born of tubercular mothers have been killed and examined here, and no case of tuberculosis has been seen by me among them.

Six calves have been born of mothers artificially infected with viruses of the human Group I, and of these three were born tuberculous.

The mothers of those which were healthy at birth had only a very small amount of tuberculosis. Two received very small doses. No. 295 was injected with a quantity of tissue emulsion estimated to contain one million tubercle bacilli into each of two quarters of the udder. She gave birth to a calf 179 days later, and died soon after. The tuberculosis was minimal and limited to the infected quarters of the mamma and to the supra-mammary lymphatic gland. The calf was free from tubercle but died two days after birth. No. 259 received a subcutaneous injection of culture estimated to contain approximately two million bacilli. She calved nearly fourteen months after injection when well and fat, and was killed four days later, and found to have minimal lesions only.

The third normal calf was born of Heifer 233 two months after a subcutaneous injection of a tissue emulsion estimated to contain 53 million tubercle bacilli. The mother was not killed until four months later; besides a small local lesion, she had one or two minute tubercles in the lungs and spleen, and scattered tubercles in one quarter of the mammary gland which was indurated.

The remaining three heifers which calved during an experimental infection, gave birth to tuberculous calves, and each suffered from disease of the uterus. In two there was extensive advanced tuberculosis of lungs of the Perlsucht type. No. 235 received a subcutaneous injection of emulsion containing 4½ million tubercle bacilli: she gave birth to a calf five months later, and was killed three weeks after parturition. The calf reacted well with tuberculin. It was killed twenty-six days after birth. It was found to have caseous patches in the portal lymphatic glands, and a suspicious focus in the lungs. An emulsion of a portion of lung and also one of a portion of the liver which appeared healthy gave rise to tuberculosis after injection into guinea-pigs.

No. 245 received a subcutaneous injection estimated to contain 157 million tubercle bacilli. She gave birth to a calf six months later and was killed sixteen days after parturition. There was extensive disease of the lungs, and but little tuberculosis elsewhere, but a few caseating foci were found in the cotyledons of the uterus. The calf was killed when five days old. A few tubercles were found in its portal lymphatic glands.

The third heifer, No. 251, is even more interesting, because it developed tuberculosis of the uterus and mammary gland, and gave birth to a tuberculous calf, when there was no tuberculosis elsewhere, except at the seat of injection, and a few little retrogressive foci in the nearest prescapular, and in the thoracic glands. The mother received a subcutaneous injection of a tissue emulsion estimated to contain 2,800 million tubercle bacilli; she remained apparently well, and gave birth to a calf four months afterwards. Nine days after parturition she was killed. There was extensive tuberculosis of the cotyledons of the uterus, and a caseous mass in the left fore-quarter of the mammary gland. The calf was killed six days after birth. In its portal lymphatic gland numerous early caseating foci were found; and there were also a few early tubercles in the lungs, liver, spleen, and kidneys, and in the thoracic and lumbar glands.*

The proportion of calves born of tuberculous mothers and affected with congenital tuberculosis is undoubtedly far larger than that which is believed to occur under natural conditions. The numbers are small—three out of six—yet too large to be altogether exceptional. How, then, can this exceptionally large incidence be explained? An experimental tuberculosis is no doubt often a far more acute and rapidly fatal form of the disease than that which usually occurs under natural conditions. Yet this does not explain the high incidence of congenital tuberculosis here, for the mothers in no cases were suffering from the acute form of the disease, and in one, the disease, except in the uterus, was very slight. It will be observed that the proportion of uterine disease was very high, and that congenital tuberculosis did not occur without it. I am inclined to attribute the frequency of uterine disease to the conjunction of two causes: (1) The susceptibility during pregnancy of that organ, as well as of others which undergo a great increase of functional activity, and (2) the distribution of bacilli in large numbers by the blood stream which follows on subcutaneous injection. The last condition probably does not occur under natural conditions, and is, I believe, the dominating reason why congenital tuberculosis has been comparatively so common in the course of our experimental investigations.

LOUIS COBBETT.

* A summary of the particulars of these animals will be found on page 253.

ROYAL COMMISSION ON VIVISECTION.

THIRD REPORT

OF

THE COMMISSIONERS.

Presented to both Houses of Parliament by Command of His Majesty.

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ROYAL COMMISSION.

WHITEHALL, 17TH SEPTEMBER 1906.

The KING has been pleased to issue a Commission under His Majesty's Royal Sign Manual to the following effect :—

EDWARD, *R. & I.*

EDWARD THE SEVENTH, by the Grace of God, of the United Kingdom of Great Britain and Ireland and of the British Dominions beyond the Seas King, Defender of the Faith, to

Our right trusty and well-beloved Cousin and Councillor WILLIAM COURT, VISCOUNT SELBY ;

Our right trusty and well-beloved Councillor AMELIUS MARK LOCKWOOD, Commander of Our Royal Victorian Order, Honorary Colonel of the 4th Battalion of the Essex Regiment ; and

Our trusty and well-beloved :—

SIR WILLIAM SELBY CHURCH, Baronet, Knight Commander of Our Most Honourable Order of the Bath, Doctor of Medicine ;

SIR WILLIAM JOB COLLINS, Knight, Doctor of Medicine ;

SIR JOHN MCFADYEAN, Knight ;

MACKENZIE DALZELL CHALMERS, Esquire, Companion of Our Most Honourable Order of the Bath, Companion of Our Most Exalted Order of the Star of India, one of the Under-Secretaries of State to Our Principal Secretary of State for the Home Department ;

ABEL JOHN RAM, Esquire, one of Our Counsel learned in the Law ;

WALTER HOLBROOK GASKELL, Esquire, Doctor of Medicine ;

JAMES TOMKINSON, Esquire ; and

GEORGE WILSON, Esquire, Doctor of Medicine ;

GREETING !

Whereas We have deemed it expedient that a Commission should forthwith issue to inquire into and report upon the practice of subjecting live animals to experiments, whether by vivisection or otherwise ; and also to inquire into the law relating to that practice, and its administration ; and to report whether any, and if so what, changes are desirable ;

Now know ye that We, reposing great trust and confidence in your knowledge and abililty, have authorized and appointed, and do by these Presents authorize and appoint you, the said William Court, Viscount Selby (Chairman) ; Amelius Mark Lockwood ; Sir William Selby Church ; Sir William Job Collins ; Sir John McFadyean ; Mackenzie Dalzell Chalmers ; Abel John Ram ; Walter Holbrook Gaskell ; James Tomkinson ; and George Wilson to be Our Commissioners for the purposes of the said inquiry ;

And for the better effecting the purposes of this Our Commission, We do by these Presents give and grant unto you, or any five or more of you, full power to call before you such persons as you shall judge likely to afford you any information upon the subject of this Our Commission; and also to call for, have access to and examine all such books, documents, registers and records as may afford you the fullest information on the subject, and to inquire of and concerning the premises by all other lawful ways and means whatsoever.

And We do by these Presents authorize and empower you, or any five or more of you, to visit and personally inspect such places as you may deem it expedient so to inspect for the more effectual carrying out of the purposes aforesaid.

And We do by these Presents will and ordain that this, Our Commission, shall continue in full force and virtue, and that you, Our said Commissioners, or any five or more of you, may from time to time proceed in the execution thereof, and of every matter and thing therein contained, although the same be not continued from time to time by adjournment.

And We do further ordain that you, or any five or more of you, have liberty to report your proceedings under this Our Commission from time to time, if you shall judge it expedient so to do.

And Our further will and pleasure is that you do, with as little delay as possible, report to Us under your hands and seals, or under the hands and seals of any five or more of you, your opinion upon the matters herein submitted for your consideration.

And for the purpose of aiding you in your inquiries We hereby appoint Our trusty and well-beloved Charles Clive Bigham, Esquire, Companion of Our Most Distinguished Order of Saint Michael and Saint George, Captain in Our Army, to be Secretary to this Our Commission.

Given at Our Court at *St. James's*, the 17th day of September 1906, in the sixth year of Our Reign.

By His Majesty's Command.

H. J. GLADSTONE.

Note.—His Majesty was subsequently pleased to create Mr. Chalmers a Knight Commander of the Most Honourable Order of the Bath.

---

350. 2,500—Wt. T.L. 306.—9/07. Wy. & S.

ROYAL COMMISSION ON VIVISECTION.

THIRD REPORT.

TO THE KING'S MOST EXCELLENT MAJESTY.

MAY IT PLEASE YOUR MAJESTY,

Availing ourselves of Your Majesty's permission to report our proceedings from time to time, we beg to submit to Your Majesty, as an Appendix to this our Third Report, Minutes of the Evidence taken during the months of April, May, June and July, 1907, as we think the present publication of these documents is desirable.

ALL WHICH WE HUMBLY SUBMIT FOR YOUR MAJESTY'S MOST GRACIOUS CONSIDERATION.

SELBY (*Chairman*).
M. LOCKWOOD.
W. S. CHURCH.
W. J. COLLINS.
J. McFADYEAN.
M. CHALMERS.
A. J. RAM.
WALTER H. GASKELL.
JAMES TOMKINSON.
GEORGE WILSON.

CLIVE BIGHAM (*Secretary.*)
29 July 1907.

ROYAL COMMISSION ON VIVISECTION.

APPENDIX

TO

THIRD REPORT

OF

THE COMMISSIONERS.

MINUTES OF EVIDENCE,

April to July, 1907.

Presented to both Houses of Parliament by Command of His Majesty.

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ROYAL COMMISSION ON VIVISECTION.

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350*a*. 1,500.—Wt. T.L. 306. 9/07.—Wy. & S.

ROYAL COMMISSION ON VIVISECTION.

LIST OF WITNESSES.

In the order in which they appeared before the Royal Commission.

Date.	Name of Witness.	Profession, Occupation, or Residence.	Representing.	Number of first Question.	Page
1907. 16th Day - April 24th	Sir T. LAUDER BRUNTON, M.D., F.R.C.P., F.R.S.	Consulting Physician to St. Bartholomew's Hospital.	A Committee of Medical and Scientific Societies.	6,777	1
17th Day - May 1st.	Miss L. LIND-AF-HAGEBY	Author - - - - -	- - - - - -	7,186	18
18th Day - May 8th.	Mr. HENRY MORRIS, F.R.C.S.	President of the Royal College of Surgeons of England.	Royal College of Surgeons of England.	7,651	41
19th Day. May 15th.	Major L. ROGERS, M.D., F.R.C.P., I.M.S.	Professor of Pathology at the Medical College, Calcutta.	- - - - - -	8,029	59
	Rev. J. PAGE HOPPS -	Unitarian Minister.	The Social Purity Alliance.	8,312	69
20th Day - May 29th.	Rev. L. S. LEWIS, M.A.	Clerk in Holy Orders - - -	Church Anti-Vivisection League.	8,629	79
	Miss A. L. WOODWARD	Hon. Assistant Secretary, Church Anti-Vivisection League and World League of Opponents of Vivisection.	„ „ „	8,786	87
	Mr. G. H. BURFORD, M.D.	35, Queen Anne Street, Cavendish Square.	World League of Opponents of Vivisection.	8,899	92
21st Day - June 5th.	*Miss L. Lind-af-Hageby* (re-called).	- - - - - -	- - - - - -	9,105	100
22nd Day - June 12th.	Mr. (*now Sir*) H. R. SWANZY, M.D.	President of the Royal College of Surgeons in Ireland.	Royal College of Surgeons in Ireland.	9,752	121
	Mr. E. A. SCHÄFER, LL.D., SC.D., F.R.S.	Professor of Physiology at Edinburgh University.	Royal Society of Edinburgh and Edinburgh University.	9,980	130
23rd Day - June 19th.	The Hon. STEPHEN COLERIDGE.	Hon. Secretary of the National Anti-Vivisection League.	National Anti-Vivisection League.	10,251	143
24th Day - June 26th.	*The Hon. Stephen Coleridge* (re-called).	- - - - - - -	- - - -	10,551	164
25th Day - July 3rd.	*The Hon. Stephen Coleridge* (re-called).	- - - - - -	- - - - - -	10,991A	185

Date.	Name of Witness.	Profession, Occupation, or Residence.	Representing.	Number of first Question.	Page.
1907.					
26th Day - July 10th	Mr. C. J. MARTIN, M.B., D.Sc., F.R.S.	Director of the Lister Institute of Preventive Medicine.	A Committee of Delegates of Medical and Scientific Societies.	11,630	208
27th Day - July 17th.	*Mr. C. J. Martin* (recalled).	- - - - - -	- - - - -	12,141	236
	Mr. D. W. BUXTON, M.D., B.S., M.R.C.P.	Consulting Anæsthetist to the National Hospital for Paralysis and Epilepsy; Anæsthetist and Lecturer on Anæsthetics in University College Hospital.	Society of Anæsthetists.	12,383	243
28th Day - July 24th.	The Right Hon. Sir J. FLETCHER MOULTON, F.R.S.	Lord Justice of Appeal.	- - - - -	12,691	251

MINUTES OF EVIDENCE

TAKEN BEFORE THE

ROYAL COMMISSION ON VIVISECTION.

SIXTEENTH DAY.

Wednesday 24th April 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Sir T. LAUDER BRUNTON, M.D., F.R.C.P., F.R.S., called in; and Examined.

Sir T. L. Brunton, M.D., F.R.C.P., F.R.S.
24 April 1907.

6777. (*Chairman.*) You are a Doctor of Medicine, and a Fellow of the Royal College of Physicians, a Fellow of the Royal Society, and you have other degrees, honorary and otherwise?—That is so.

6778. I believe you are consulting physician to St. Bartholomew's Hospital?—I am.

6779. And have you taken, from an early period in your studies in medicine, a great interest in this question of experiments on animals?—I have.

6780. And you have had practical experience yourself?—I have.

6781. For a good many years past?—For a good many years.

6782. Did you confine yourself to any particular class of object originally? We have had some witnesses who have confined themselves principally to the operation of drugs, others to particular organs of the body, and so forth. Were your experiments directed to any particular subject originally?—The particular subject at which I was working was to ascertain the action of drugs, especially with a view to utilising them in the treatment of disease.

6783. I think it would be convenient, as some members of the Commission have to leave at a rather early hour, and may not be able to get back soon, if I were to ask you now with reference to an experiment of yours, taking it now a little out of order, that was referred to by Mr. Graham. I think you have had sent to you a note of what he said?—Yes, I have got it before me.*

6784. He quoted from memory, and without reading from the book, some report that he had seen of these experiments, and he gave the general effect of it as being that cats were baked to death in order to ascertain, as I understood, what the effect of a high temperature was on them, or what temperature they could live in. I do not quite understand, from what he said, what the precise object was. Perhaps you will explain the experiment in your own way?—Yes; and perhaps I might be allowed to go back a little further even than that experiment, because, by doing so, I should give a sort of continuous view of what I was doing.

6785. If you please?—In 1870, Professor Stricker found that rabbits, when completely narcotised with chloral, lost their power of maintaining their temperature and died. If those same rabbits, instead of being laid out upon a table, where they could cool readily, were simply enveloped in cotton wool, they lay senseless for many hours and ultimately recovered.

6786. You mean enveloped in cotton wool during the experiment?—Practically what he wanted to do was to examine the tissues microscopically, their ears, and so on, and he found it was convenient to render them completely senseless and immobile by a large dose of chloral, that rendered them completely senseless so that they could neither feel nor move. They remained in this condition for many hours, but unless they were kept warm they ultimately died. If, however, he simply wrapped them up in a sheet of cotton wool, at the end of several hours they began gradually to recover. He directed my attention to this, and I found that if, instead of merely wrapping them up in cotton wool, I kept them warm artificially with an ordinary hot water bag, or by a tin hot water can, the animals would bear a bigger dose of chloral, and still recover, than if they were simply wrapped up in cotton wool. And I am glad to say these experiments of mine have borne practical fruit; because, not very long after I published them, the late Dr. Young, of Florence, had a case of an overdose of chloral, when the patient, an old lady, would certainly have died but for the fact that he remembered the results of my experiments, about which I had been conversing with him. He wrapped her up in cotton wool and put hot water bags round her, and she came round. That was the first thing that drew my attention to the effect of temperature upon the action of drugs. The next thing I wanted to find out was why certain drugs, given in cases of disease accom-

* *See Questions* 5844–5849, 6079–6081, 6131–6140.
Dr. Graham, in returning his own corrected evidence on 15th March, had also appended the following note:—"Though there is nothing to correct in the above account, it may save time and controversy if I add that since I gave evidence I have looked up the detailed account of the experiments which preceded the above and which were recorded in the Reports of St. Bartholomew's Hospital for 1871, *Vol. VII., p.* 216, *by Sir (then Dr.) Lauder Brunton alone. He gives details of his method. The animals were narcotised only with chloral—once with laudanum—and so were kept still. The heating apparatus was a tin with double bottom and sides into which hot water was poured, and a hot water bag was put on the belly. The heart was pierced by a needle and the thermometer was in the rectum. These methods may or may not have been followed in the later series I quoted.* J. W. G.

Sir T. L. Brunton, M.D., F.R.C.P., F.R.S.

24 April 1907.

panied by high fever, do not act in the usual way. It was pointed out by Professor Thomas that pneumonia, when treated by digitalis, does not yield to digitalis in the way you would expect from the action of the drug upon a healthy man. The digitalis seems, in the case of a person having pneumonia, not to act upon the heart in the same way that it would upon an ordinary person. It occurred to me that this might be due to the effect of temperature upon the heart, and so I proceeded to make a number of experiments upon animals with high temperatures. If you keep a healthy animal or a healthy man in a warm place, as a rule their temperature does not rise very high; it rises a little, but not very high, as everyone knows who has gone into a Turkish bath; but if you narcotise an animal completely, and then keep it warm, it has lost its power of regulating the temperature, and the temperature goes on rising and rising until the animal dies, although the animal remains perfectly unconscious for the whole time. I therefore narcotised animals (and especially I used rabbits in that series of experiments) by a large dose either of chloral or opium. I then placed them in a bath, of which I have had a copy made. The object of the experiment was to imitate the conditions of a patient suffering from high fever and lying in bed. To have had any pain inflicted would have tended to vitiate the experiment. The description was fully given in the paper that I wrote in the St. Bartholomew's Reports, Volume VII, for 1871, at page 218.

6787. Will you please read the description of the bath?—"These experiments were made by narcotising the animal with opium or chloral, and laying it in a tin vessel about 20 inches long, by six inches broad and three inches deep in the inside, and well padded with cotton wool. The vessel was double, and by pouring hot water into it the temperature of the rabbit was gradually raised. The belly of the animal was also covered with cotton wool, and sometimes with an india rubber bag containing hot water. To make respiration easier a cannula was introduced into the trachea, and in some experiments the inspired air was passed over warm water so as to warm it and saturate it with moisture, and lessen the loss of heat from the lungs."

6788. I thought it necessary to ask for that description, because Mr. Graham said that the animal, a cat, I think he said, was put into a stove and baked to death?—Yes, and Miss Frances Power Cobbe also described these experiments as "roasting to death."

6789. (*Sir William Church.*) Mr. Graham's words are: "They were put into a kind of stove and were heated until they died"?—Yes. Then I may pass over the means, perhaps, of recording the beats of the heart; and then I go on to say: "As the animals were completely narcotised and remained perfectly motionless, the thermometer remained undisplaced in the rectum from the beginning to the end of the experiments." That shows how complete the narcosis was, that the thermometer introduced into the rectum remained perfectly undisturbed the whole time. The animal did not make the slightest movement.

6790. (*Chairman.*) Are we to understand that the object of this experiment was to test to what temperature you might safely raise the heat of an animal while it was under narcotics?—No; the object of the experiment was to find out what the condition of the heart was in what is known as hyperpyrexia. In many cases of disease the temperature of the patient rises high above the normal, and then these patients are in a state of extreme danger, and unless you can reduce the temperature in some way we know that these patients will die. The object of these experiments was to find out what the condition of the heart and circulation was in pyrexia, and especially in hyperpyrexia, so that one might be able to use appropriate remedies to reduce the temperature and slow the circulation.

6791. Was the animal operated on after it was in this box or while it was in the box?—There was almost no operation done on it. The only operation that was done was that it was first of all completely narcotised by a subcutaneous injection of chloral or opium; the animal became completely senseless, and then a cannula was introduced into the trachea at the same time to allow us to warm the air entering the lungs. The only other operation done upon it was to put an ordinary sewing needle between the ribs and attach it by means of a thread to a lever so as to record the movements of the heart, in order to see how quickly the heart was beating in accordance with every rise of temperature in the animal. The object was to ascertain the action of the high temperature of the hyperpyrexia of the high fever upon the organs of the body, and especially the heart, with a view to finding out how we were to prevent danger of death to our patients when suffering from high fever.

6792. And did you attain the object?—To a certain extent, yes.

6793. Do you consider that the experiment was a useful one?—Certainly.

6794. Not only justifiable to try it, but it was useful in its results?—Assuredly, I think.

6795. (*Sir Mackenzie Chalmers.*) The experiment was absolutely painless throughout?—Absolutely.

6796. (*Chairman.*) I think, in fact, if I recollect aright, Mr. Graham said in his evidence that obviously it was impossible to perform it under anæsthetics?—Yes, he said so. This is not the series of experiments referred to by Mr. Graham, but this is the apparatus used in the series, to which he refers in "The Practitioner."

6797. Before you finish with that, was the rabbit killed before it came out of the anæsthesia, or was it allowed to recover?—They were all killed, either killed or died from the effects of the chloral.

6798. (*Mr. Tomkinson.*) I thought the object was to see whether they would not recover by the preservation of their heat?—No, excuse me; these are two entirely different series. The first series of experiments I referred to was upon the effect of heat in preserving life; this second series was upon the effect of heat in causing alteration in the heart.

6799. (*Sir William Church.*) As a matter of fact, these animals were narcotised by a poisonous dose of chloral or morphia?—Always.

6800. So that they naturally died?—Yes, these animals would have died anyhow whatever we had done to them.

6801. We have been told here repeatedly that morphia is not an anæsthetic; that depends, of course, upon the quantity. We have been told also that chloral is not an anæsthetic; that also depends upon the quantity. These animals receive poisonous dises in order to completely narcotise them?—Yes; and as to the statements that chloral and opium or morphin are not narcotics, and do not remove pain, there is no other word for it, it is simply a lie; you may as well say that chloroform does not remove pain. If you give any animal a sufficiently large dose of chloral or opium, you so completely abolish sensibility that there is nothing you can do that will awaken its sensibility; you might burn it alive, you might throw it on the coals, and it would remain unmoved. I have never tried it, but I know you can conduct any experiment which would in the ordinary nature of things be painful, but there is no pain attached to it. The animal is as senseless as a piece of board.

6802. (*Colonel Lockwood.*) But it all depends on that statement "if a sufficiently large dose is given"?—Quite.

6803. (*Chairman.*) You are speaking of chloral and morphia?—I am speaking of chloral and morphia. I may say that nearly all my experiments were done after I had been in Professor Ludwig's laboratory in Leipsic, in which the invariable rule was that before any experiment was done the animal should be thoroughly narcotised. Dogs were frequently used, and the way they narcotised them was to inject into the vein that runs on the hough joint, and which is there prominent, at least a drachm of tincture of opium. The usual effect of this was that in less than half a minute the animal was completely senseless, and never came round.

6804. You were going on to the actual experiment which Mr. Graham was referring to in his evidence. What you have told us was introductory, leading up to the other?—Yes; he refers to the "Practitioner," Volume 33, page 273; and the reason why I have given this long explanation of this series of experiments is not only that Miss Frances Power Cobbe described this series as "roasting rabbits alive," but that the second series, the series with digitalis referred to by Mr. Graham, were a continuation more or less of this first series, and were made in exactly the same way. At page 275 of the "Practitioner," Volume 25, the papers quoted by Mr. Graham—Dr. Cash and I say, after referring to the first series of experiments, which I have

just been describing, "In experiments of the same kind which we have made upon cats"; and the reason why no full description of the method of the experiment was given in the "Practitioner" was that the whole method had been so completely described before that anyone who wished to know how the experiments were conducted had only to refer to the former paper.

6805. When you say, "In experiments of the same kind," what was the preceding sentence; because I suppose it referred to the previous experiments?—Shall I read the whole?

6806. What I want to get at is this: I want to see whether a person reading those words, "of the same kind," would have any means on that page of tracing back and seeing what the previous experiments were; because it is assumed that these animals were slowly baked to death without any anæsthetics, as Mr. Graham says?—Then, perhaps, I had better read the sentence before: "We have therefore attempted, as a first step in our investigation, to ascertain the effect of rise of temperature alone on the pulse in the cat, which animal we employed in the present research. In a former series of experiments made by one of us——" Then comes, in a footnote, a reference to this series, to which I have already referred, "on the effect of heat upon the pulse in the rabbit, it was found that the pulse-rate increased with considerable regularity as the temperature of the body rose, as is shown by the following experiments, in one of which the vagi were uncut and in others were cut. At the commencement of the experiment there were slight irregularities before, which have not been copied here. After the temperature had risen above a point which is marked by thick type in the table, the pulse rate began to fall." Then follows a table of the temperatures and the corresponding rates of the pulse; and immediately after the table comes the expression I have just used: "In experiments of the same kind which we have made upon cats."

6807. So that any investigator reading that and seeking to ascertain the whole truth about it, would have the means of seeing precisely what the nature of the experiment was?—Precisely.

6808. And the conditions under which it was performed?—Precisely—the reference is fully given; and the reason why it was thought unnecessary to give it again was that anyone who wished to ascertain precisely how the experiments were done could do so by referring to the original paper—the first paper. But this paper on digitalis was not written for physiologists or pharmacologists, but for general practitioners, and had a practical object in view; it was to tell them what alterations they might expect to find in the action of digitalis if they gave it to a person suffering from high fever; and so after giving, in such a way as the general practitioner would understand, a *résumé* of our results, we then went on to the practical conclusions, which are practically that "The administration of digitalis, or of drugs which act like it, to patients in a febrile condition is therefore likely to have much less effect upon the pulse than at the normal temperature; and if the temperature be very high they may have no effect at all whilst this persists." And then comes rather an important point: "When the temperature began to fall the pulse naturally became slower, and this slowness is increased if digitalis has been given at the height of the fever. It is therefore evident that digitalis and its congeners, if they are given at all when the temperature is high, should be given with great care; for, otherwise, the medical man may be induced, by the apparent inaction of the remedy, to push its administration too far during the fever, with the consequence of producing too great depression of the pulse during defervescence." Now this is a very great practical danger, because, in a case of high fever, you are tempted to use every means in your power, and to push every means to reduce the temperature, and to slow the pulse, which is going tremendously quickly; and you are apt to forget that in a fever like pneumonia, which ends after a definite period, the temperature will fall and there will come a condition of defervescence, and very likely of collapse, and that if you are careless in pushing your digitalis too far during the fever you may help to produce a greater condition of collapse after the fever is over and kill the very patient you want to save.

6809. Was this experiment that you made with a cat conducive to, or was it essential to, the discovery of which you have just told us?—Yes, surely. I do not see how you could have done it without.

6810. You say it was necessary; it was the way in which it was discovered?—It was the way in which it was discovered, and I do not see how you could have discovered it in any other way. The only other way in which you could do it would be to experiment upon a number of patients. The difficulty in experimenting upon patients is that every medical man who is called in to a patient feels himself bound to do his best for that individual patient. He is not called in to advance the science of medicine by what he tries upon his patient; he has got to consider that individual patient and do the best for him. Whereas, if you are working upon a lot of animals, you do what is best to attain knowledge without considering whether the animal is going to reap any particular benefit from what you give it or not.

6811. I think every member of the Commission would agree with you there, that when a doctor is called in his first duty is to his patient and not to science?—Surely.

6812. That is your description as given from your book and your comment of actual experiment which is referred to here?—Yes, and here I have had a model made.

6813. I should like to have this quite clear, without referring back again, that the heating was carried on in the way which you have described in that box with cotton wool and under a fatal dose of chloral?—Yes, or of morphin or of opium.

6814. Ultimately fatal?—Yes.

6815. Then I gather that it is not true to say what Mr. Graham says in answer to this question: "You do not wish us to understand that these poor creatures were slowly baked to death, because that could not be under anæsthesia, and it would be most awful torture"? To which he replied: "They were slowly done to death by heat." That, taken in conjunction with the question, is not very accurate?—It is not so.

6816. (*Colonel Lockwood.*) Do you say chloral and opium?—No, chloral or opium. Once or twice I have used the two together, but as a rule it is unnecessary to use the two; but once specially I remember using the two together.

6817. (*Sir William Church.*) But as a matter of fact in your second series of experiments on these cats the majority of them did not recover, I think?—No, those all died too; they all got this fatal dose. But then there is yet another series which, perhaps, I had best refer to, they have not been referred to in the evidence, but still Mr. Graham has evidently got a notion of a stove of some kind.

6818. I think, if I might say so, Mr. Graham mixed up the sets; because he led me to think that some of the animals recovered?—He has probably mixed up yet a third set, and these were a series of experiments upon the action of aconite as an antipyretic. In many cases of fever small doses of aconite seem to reduce the fever very much; and so Dr. Cash and I tried to see how far the aconite would do this in animals, and more especially we used pigeons. In pigeons the temperature is higher than in ordinary animals; the normal temperature of a pigeon being about 104, whereas in man it is only 98.2 Fahrenheit. With these pigeons we did use a kind of a box, and as that will very likely be referred to later on—I mean that they may mix it up again—I have thought well to take the box question up as well. This box was simply a metal box with a glass top, and surrounding the metal box was a second case in which you could put either hot or cold water, so as to raise the temperature of the box to such a degree as we liked. We padded the inside with brown paper, so as to prevent any contact of the animal with the metal itself, because we did not want any local chill or any local burning to vitiate the experiment. All we wanted to do was to find out the condition of fever such as would occur in a patient in bed. We raised the temperature of the animals by raising the temperature of the box, in some cases to 106 Fahrenheit; but this can hardly be described as baking alive, because I have found by inquiry at the Jermyn Street Bath that, whereas our highest temperature was 106 Fahrenheit the lowest temperature of the Jermyn Street Bath is 120 Fahrenheit, their highest temperature running up to 230.

6819. (*Chairman.*) Do you mean the temperature of the atmosphere at which the animal was was 106?—Yes. May I bring the figure of the box round to show it you? (*Exhibiting the figure of the box to the Commissioners.*) We raised the temperature of the box in some cases up to 106°.

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6820. (*Mr. Tomkinson.*) That was the temperature of the air which the animal breathed in the box?—Yes.

6821. 106° only?—Yes, whereas the lowest temperature of the Jermyn Street Baths is 120°.

6822. The first room is 120° or 125° and runs up to 220°?—Yes.

6823. I have sat in 200° myself?—So that although a box was used here which might be described as a stove, an animal was put into it, the pigeons sometimes at 107° did show symptoms of discomfort and feeling too hot; but that could hardly be described as roasting them alive.

6824. (*Chairman.*) Mr. Graham refers to the temperature of the cat, he says, as being raised to 111° in one case and 113° in another. Was it in that experiment?—No; that was in the series of experiments of which I spoke before.

6825. The rabbit?—No; that was the one with cats on digitalis, but carried on with the same instrument as the experiment on rabbits.

6826. Carried on in the tin bath?—Yes; the tin bath I have already shown, and of which I have read the description (6787). If you narcotise a cat completely with either chloral or with opium and leave it just with the cotton wool round it, and it is warm, its temperature gradually rises, because the narcosis disturbs the normal power of regulating the temperature, so that the temperature of the cat will rise to 111, 112, and 113, and then that is hyperpyrexia, and they die, just as those patients who have acute rheumatism often die with high temperature in a hospital ward.

6827. In the case of the cat, the intention was that it should not recover?—That it should not recover.

6828. And you were testing it under this complete anæsthesia to see at what point that hyperpyrexia would occur?—Yes, that was, however, rather incidental. What we wanted to find out was the condition of the heart throughout.

6829. I should rather say that you were testing it with the full understanding that it would occur at one point?—Yes.

6830. (*Colonel Lockwood.*) I only wish to ask you a few questions as to the present law. I note that you say in your *précis* that the law, instead of being made more stringent, ought to be relaxed. Do you look upon the present laws on the subject of animal vivisection as a stop to science?—To a certain extent I do.

6831. And in consequence, do you think that our knowledge of science is behind that of foreign nations where those laws do not exist?—It would be behind if the foreign nations had the same laws as we have; but we keep pace with them by learning what they are doing. We depend for a great deal of our information upon what we get from abroad.

6832. Then our superior intellect enables us to outstrip them?—In some cases; certainly not in all.

6833. Therefore I presume you would be very much opposed to any further restrictions upon experiments upon living animals?—Surely. Perhaps I might be allowed to give one illustration of how the Act has interfered with work which might have been beneficial. About 1871, with Sir Joseph Fayrer, in making experiments upon snake venom, we found out that there were certain substances which would destroy snake venom absolutely; but we had not succeeded in obtaining immunity or saving life after a big dose had been injected. We could not administer the antidote in such a way as to prevent the action of the poison when once introduced. Then came the anti-vivisection laws, and we could not continue our experiments at all at home. The consequence of that was that they lapsed for a number of years, and they were only taken up again about two years ago by Major Rogers. He was able to take them up again because Professor Waller had invented a method of giving chloroform continuously for thirty hours or more, so that he was able to give the poison and then keep the animal for many hours under the anæsthetic, and then he could try the effect of the antidote. I had invented a small lancet which I thought might be useful, but it had not practically been tried until Major Rogers got this chance of doing it, when he found that it answered quite well. He has since been using it in India, and has, I think, now got either seventeen or twenty cases of snake-bite in which the persons would almost assuredly have died and who have been saved by the use of the instrument. I have one here; it is made in two forms (*exhibiting the same*). It consists of a small lancet, in the handle of which is some permanganate of potash. In using that, the punctured wound made by the snake's fang is simply converted into a slit, and permanganate of potash is rubbed in after being wetted with saliva.

6834. You claim that your little lancet has been discovered by the assistance of experiments on living animals?—It was made in consequence of a number of experiments that I made with Sir Joseph Fayrer.

6835. (*Chairman.*) Colonel Lockwood's question was whether the shape of the lancet was discovered by experiment. I do not know whether you mean that?—The shape of the lancet is of no importance.

6836. (*Colonel Lockwood.*) I did not mean that.

6837. (*Chairman.*) The cure is by permanganate of potash?—Yes.

6838. (*Colonel Lockwood.*) All obtained by experiments on living animals?—Yes, experiments which were made quite thirty years ago.

6839. Have you ever used curare only, in experiments on living animals?—Yes.

6840. Latterly?—No, I cannot very well say whether I have used it only.

6841. When performing an operation under licence from the Home Office?—No, any experiments I made with curare would be previously to the existing law.

6842. (*Chairman.*) Previously to 1876?—Yes.

6843. (*Colonel Lockwood.*) And never since?—I cannot say that I even did it before. It is just possible that I may have done it, but I cannot recollect it.

6844. Have you witnessed a very great number of experiments, and conducted yourself very many?—Yes.

6845. Do you, or do you not think that further inspection of laboratories where these experiments are carried on is advisable?—I do not think it would be any good at all; I do not think it is advisable.

6846. You believe that the experiments in these laboratories are conducted absolutely painlessly?—Yes.

6847. And without unnecessary suffering?—I do.

6848. (*Sir William Collins.*) In the interesting *précis* which you have sent us I see you state that in your opinion the majority of those who desire the abolition of experiments on animals" are actuated by praiseworthy motives"?—Yes.

6849. But you think that their opposition is founded on ignorance?—In many cases it is, I am sure.

6850. You trust that the evidence taken by this Commission may help to dispel that ignorance?—Yes.

6851. I see you call attention to the importance of bearing in mind a liberal interpretation of the word "experiment" in dealing with the whole question of the effect of experiments upon living animals for scientific purposes?—Yes.

6852. You speak of experimentation as the condition under which all living beings exist?—Yes.

6853. And you say that medical men also know only too well that every effort they make to alleviate pain or cure disease is an experiment made on their patients?—Yes.

6854. Then you refer to the natural experiments, as it were, of epidemic diseases, as in the case of the epidemic of measles in the Fiji Islands?—Yes.

6855. You state in regard to that case that a disease which like measles is comparatively harmless in a population where it is endemic, as in this country, may become a devastating pestilence when introduced into a country like the Fiji Islands, where it was formerly unknown?—Yes.

6856. Do you mean that it was because of the action on a virgin soil, as it were, that the fatality was so great in that case?—I think that was so; I believe that you actually do get people used to the conditions under which they are, both hereditarily and personally. Newcomers, when going to Venice, for example, suffer from diarrhœa and disorders of the digestion, which people who are living in Venice do not suffer from, although they are under exactly the same conditions, drinking the same water, smelling the same smells, and altogether under the same conditions.

6857. With regard to that particular epidemic of measles in the Fiji Islands in 1874, do you happen to remember the remarks upon it of Hirsch, in his "Historical and Geographical Pathology"?—I do not.

6858. I see that under the head of "Severity of type due to neglect of the sick," he suggests that the mortality in that case was due to the fact that the afflicted were exposed unattended and untreated. That consideration would have to be borne in mind in assessing the cause of fatality there, would it not?—It would; but, of course, in the Fiji Islands, leaving them unattended would not have the same bad effects as it would here. The climate of Fiji has to be taken into account as well.

6959. I see Hirsch suggests that every complication was invited and rendered more intense by this neglect and exposure?—I should think it is very doubtful indeed. I do not think Hirsch ever went to the Fiji Islands. I have never been there either; but I have known people who have been there, and from what I learn about the climate there, it seems to me that Hirsch's remark is perhaps not to be taken too literally.

6860. Do you know also that he suggested that those that were treated did not die at an unusual mortality in that particular epidemic?—No, I did not know that.

6861. With regard to snake poison, I see in the little box you have handed into the Commission there is a paper which states "The best remedy that can be carried out for this purpose" (that is to say, against snake-bite) "is permanganate of potash, the efficacy of which as an antidote to the poison, not only of the cobra, but of other venomous snakes, when properly applied, has been conclusively demonstrated"?—Yes.

6862. Do you agree with that?—It might be advisable for this Commission to get Major Rogers to tell them exactly what he has found. He has, I think, seventeen or twenty cases, or perhaps more, which have been quite successful.

6863. When did Major Rogers begin the experiments to which you allude. What is he?—He is in the Indian Medical service, but he is home just now on leave.

6864. Will you tell the Commission when he began the experiments; you said a short time ago; you did not mention the year?—I think it was about three years ago. It was 1904.

6865. (*Chairman.*) There have been experiments, I understand, upon animals to test the nature of permanganate of potash?—Yes, there were experiments made by Sir Joseph Fayrer and myself about 30 years ago. Those were stopped in the middle because of the vivisection laws, and they were taken up again, in 1904, I find it is, by Captain Rogers, as he then was, and he was able to continue them because he had got an apparatus for keeping an animal under an anæsthetic for thirty hours, so that he could give the snake poison and give the antidote and see whether the animal lived.

6866. (*Sir William Collins.*) Are you acquainted with the experiment of Calmette and others with regard to anti-snake venom sera?—Yes, perfectly.

6867. Do you agree that a serum has been found which is valuable against snake bite?—Certainly. But the difficulty is that the serum that is prepared, for example, for a krait, will not act for a cobra, and *vice versa*, whereas this will act for any poisonous snake, for krait, cobra, or water snake.

6868. Then permanganate of potash in your opinion is a universal remedy?—Yes, and more than that, I can testify from my own experience that it is an antidote to ordinary septic poisoning.

6869. And you regard that as conclusively demonstrated?—Yes.

6870. Have the various treatments by serum and your treatment been largely utilised in India?—Not yet. The difficulty of the serum treatment, of course, is that the serum and the snake are not likely to be in the same place. You do not always get the snake and the serum together as in Calmette's case, where he got bitten while lecturing, and applied the serum forthwith, which he made himself. As a rule you cannot use the serum, and even this lancet has not been largely used yet. It is only just beginning to find its way; but I think that already Major Rogers has had 17 or 20 successful cases.

6871. You can hardly say that the mortality from snake bite in India has as yet been reduced by any treatment based upon experiments on animals, can you?—One can say definitely that it has been reduced; it has been reduced; it has been reduced at least by 17 or 20 cases. But we cannot say in what proportion it has been reduced. I do not know how many people have tried this lancet, but in 17 or 20 cases where it was tried it was successful, and in so far the mortality has been reduced.

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6872. I see in the year 1905 according to the official figures, there were 21,797 deaths from snake bite in India?—Yes.

6873. A mortality of 94 per million living?—Yes.

6874. Whereas in 1876 there were 15,819 deaths, or a rate of 85.4 per million living?—Yes.

6875. So that apparently the numbers at present are unfortunately increasing?—There is all the more need for a liberal distribution of these little lancets.

6876. Has the Indian Government adopted them?—I do not think it has. If it would simply adopt these little lancets and sell them, as it sells packets of quinine for a few pice at every postage station, the mortality in India from snake bite would be very materially reduced in a short time.

6877. Or distribute them gratuitously?—If they would sell them at cost price, they might be produced on a large scale at about a half-penny each, I believe. Of course the Indian peon is an awfully poor man. The price of a lancet represents so much rice to him.

6878. But if you wish to let your antidote be as universal as the bane, it would be desirable to make it gratuitous?—Yes, it would be.

6879. Can you refer me to a record of the 17 or 20 cases in which the success has been conclusively demonstrated?—I am not sure that I can, because I chiefly quote from a letter to myself from Major Rogers; but he has, I know, published some of them.

6880. Can you refer me to the publication?—No, but I will get it for you willingly.

6881. Is Major Rogers a Government official?—Yes, he is Professor of Pathology in the Calcutta Medical College.

6882. Would the report on these 17 or 20 cases in which the efficacy of the remedy has been conclusively demonstrated, appear in an official report?—I do not know whether it is an official report or whether it is in a medical journal, but it will be in one or the other no doubt.

6883. Have you the letter by you on which you base your evidence?—No, not here, but I have got it by me. If you would like the letter I can send it on.

6884. (*Chairman.*) Major Rogers himself is in England, you say?—Yes, I can send you his address. He is, I think, working every day at the School of Tropical Medicine, and he gave the lecture on kalaazar before the Royal College of Physicians two or three weeks ago.

6885. And if the letter is not a private one perhaps you would let us have a copy of it?—Certainly.

6886. (*Sir William Collins.*) As to the administration of digitalis in pneumonia, I asked the President of the Royal College of Physicians, Sir Douglas Powell, when he was here, whether he would give us his opinion as to any rules which have been obtained as the result of experiments on living animals for the administration of digitalis in pneumonia, and he replied that he was not aware of any experiments which had established the usefulness of digitalis specially in pneumonia?—That is quite true. The usefulness of our experiments was chiefly cautionary. That a man might be tempted to use digitalis in pneumonia and thereby do harm.

6887. Is it the result, in your opinion, that digitalis ought not to be used in pneumonia?—My own belief is that sometimes it may be useful, but as a general thing it is advisable not to use it. Our experiments showed that it is possible to use digitalis in pneumonia and thereby do harm.

6888. Then in regard to amyl nitrite, a question which has been raised by other witnesses, I see that in your *précis* you state that by finding in a case of angina pectoris that the blood pressure rose too high, and having learned through experiments on animals that nitrite of amyl would lower the pressure, you applied

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the drug to the treatment of the disease with perfect success?—Yes.

6889. What was the date of the first publication of yours on the subject?—It was in July, 1867.

6890. In that publication did you refer to a previous work of Sir Benjamin Ward Richardson upon the subject?—Yes.

6891. Did you say "Little attention, however, was paid to it for some years until it was again taken up by Dr. B. W. Richardson"?—Yes.

6892. Have you seen Dr. Richardson's statement in regard to it in his book called "Vita Medica"?—No, I do not think I know it. I only know his published papers in the British Association reports.

6893. It has been doubted whether the discovery of nitrite or amyl was based upon experiments upon animals or trying it on the human subject. Dr. Richardson stated, "Research with nitrite of amyl proceeded in the usual way. I made myself the first victim, and tried the effects of the substance by taking it both in the form of vapour and fluid. I had also some friends who were willing to follow in my wake." Did he not produce it at the meeting of the British Association, I think in 1863, when its effects were noted by some of those present, who tried it by inhalation?—It may have been so; but his chief account of nitrite of amyl is given in the Report of the British Association for 1864, where he refers to an earlier paper which he had read at the Newcastle meeting in 1863; but on looking up the volume for 1863 I cannot find any reference whatever to Dr. Richardson's paper, either in the Index of the Report or in the Index of Communications. I have brought it here so that there should be no mistake. This is the volume for 1863 (*handing in the same.*) So that it cannot have been of any very great importance, I think. Then in the Reports for 1864 there is a very full account, but his reports deal much more, in 1864, with the experiments upon animals than the experiments upon himself. Here are the experiments upon animals (*handing in the volume*). He gives here those upon the skin and so on. Then at page 126 he gives a *résumé* of its action. Then in the next year he took it up again, in 1865, at Birmingham, and he again gave a *résumé* of its action even shorter than what he gave in 1864. Perhaps I might read the *résumé* that he gave himself. He says, "Before proceeding further, permit me to invite the attention of the section to a very brief summary of the facts brought forward at our last meeting in 1864, respecting the nitrite of amyl. I showed then that nitrite of amyl was: 1. The most powerful excitant of vascular action known. 2. That in animals whose bodies admit of its spontaneous evaporation, the nitrite suspends animation, and that in animals of higher organisation it induces a condition resembling cataleptic insensibility. 3. That it is not an anæsthetic. 4. That the effects produced by it are developed on the motive powers of the organism, which it first vehemently attacks, and then paralyses. 5. That it arrests the process of oxidation." So that, you see, all he did there was to show that it was a "powerful excitant of the vascular action," without going into details. He did suspect, and correctly, that the excitement of the heart was probably due to the effect of nitrite of amyl in dilating the capillaries and lessening the resistance; but he did not pursue that any further. He went on, in his report of 1865 and later, to discuss the question of the action of other compounds of amyl, and left the nitrite, which was the most important, alone; so that the question of how it acted remained unsolved. It was then taken up by Rutherford and Gamgee, who made experiments upon animals with an ordinary manometer, and found that the blood pressure was greatly reduced by nitrite of amyl. At that time I was working a good deal in the laboratory with Gamgee, and knew all about the experiments. I had made none on nitrite of amyl myself, but I was very susceptible to nitrite of amyl, and Gamgee very often tested it upon me, taking traces of my pulse; and then I saw the extraordinary depression in the tension of the pulse which was produced by nitrite of amyl. At that time I was house physician in a hospital in Edinburgh, and there was one poor man in the wards suffering dreadfully from angina pectoris; he used to have an attack every night, and for two hours the unfortunate man would sit on the edge of his bed and could not move forward, backward, or to one side, with his face pale and the sweat pouring off it, in perfect agony. I used to sit by him and try to do what I could to ease his pain. I gave him chloral, I gave him ether, alcohol—everything I could think of. It was all no use. I took tracings of his pulse to see what was happening, and I found that his pulse during the attack became very tense. I tried everything that I could think of, and one day he said, "Well, you have been very good, but it is no use; let me go." I said, "Well, I will try one thing more, and if it does not do you any good you shall go out to-morrow." I went straight up to Gamgee and asked him for some nitrite of amyl, because I thought if nitrite of amyl lowers the tension so much, and this poor man's tension rises during the attack, if I gave amyl it ought to ease him. I brought it back and gave it, and it was perfectly successful. But for the experiments I should never have known the action of nitrite of amyl—nobody would have done so. Richardson's experiments upon himself, you see, simply showed that it flushed the face, that it excited the action of the heart, and produced palpitation; and produced a feeling of giddiness and wanting to sit down, and that was all.

6894. Does not Richardson say, "My description of the effects of the nitrite was that it relaxed the living muscles"?—I suppose that was what he meant to say; I do not think it is in the reports.

6895. Is anything you have quoted incompatible with Dr. Richardson's description of his own researches of 1867, namely, that "the research with nitrite of amyl proceeded in the usual way. I made myself the first victim and tried the effects of the substance by taking it both in the form of vapour and fluid"?—No; he probably tried it, but these experiments would not have helped—his observations did not help at all with regard to angina pectoris.

6896. But you referred to his experiments in your paper of 1867, did you not?—Yes, surely, because it was he who drew attention to the action of the drug. He drew attention to it, and Gamgee and Rutherford followed it up and worked it out, and then I applied it.

6897. (*Chairman.*) How do you trace the benefit of that discovery to experiment on animals?—Because, but for the experiments on animals made by Gamgee and Rutherford, we should not have known that it reduced the pulse tension.

6898. (*Sir William Collins.*) In a letter to the "British Medical Journal" on the 16th of March of this year I see you state that "any benefits which may result to patients from either plan (the inhalation of oxygen or the injection of strychnin) must be regarded as the fruits of experiments on animals"?—Yes, because in regard to strychnin, the first experiments that were really of much use in strychnin were made by Prokop Rokitansky in Professor Stricker's laboratory many years ago. Usually, when you cut the spinal cord across, just at the junction of the head and spine, all respiratory movements cease; but if you inject strychnin beforehand they do not quite cease, and if you inject strychnin afterwards you get certain respiratory movements, imperfect no doubt, but still an attempt at respiratory movement, showing, apparently, there is a certain amount of respiratory centre which extends a little way down into the spine. In consequence of this, one has been accustomed to give it in failure of respiration. Then in the case of the heart, if you tie a ligature around a frog's heart, the junction of the venous sinus and the auricle, it stops; but if you inject a little strychnin into it it will begin again; and if you inject strychnin beforehand, before tying the ligature round, it does not always stop. This appears to me to show—although I daresay Dr. Gaskell may have a different explanation to give of the fact—that the strychnin acts as a stimulant to the heart; and, practically, when we have come to try the result upon patients, we find that strychnin has got a stimulating action in keeping up the respiration and keeping up the heart in cases were both seemed to be about to fail.

6899. I was going to ask you whether the claims you make on behalf of oxygen and strychnin are generally accepted by the profession?—I believe they are.

6900. I notice that on the 23rd of March, this year, in the "British Medical Journal," Sir James Barr seems to speak rather disrespectfully of your discoveries. He says, "Now for Sir Lauder Brunton's other discovery of the use of oxygen and strychnin in pneumonia. I am pleased to think that the craze for cylinders of oxygen is dying out, notwithstanding this spasmodic attempt to revive it." And again he

says, "I rather hesitate to conjecture how many persons have been poisoned by it since that fateful day when Sir Lauder Brunton 'scribbled the other half of his paper in a railway train.'" Perhaps you would like to make a statement in regard to that?—I have heard from a good many doctors since, and they do not agree with Sir James Barr, and they do agree with me. A man of large clinical experience, for example, in Scotland, who has got one of the largest consulting practices in Scotland, as well as a hospital appointment, says that he quite agrees with me and does not agree with Sir James Barr.

6901. I need hardly say, as an old pupil of yours, that I have great respect for your opinion.

6902. (*Mr. Tomkinson.*) I notice that you used the word "narcotised" always. You mean that to be equal to anæsthetised?—It is something more than anæsthetised.

6903. The use of opium, chloral and morphia produces a state of insensibility?—Yes.

6904. Is that at all akin to that insensibility which is produced in a hopelessly drunken person that we see sometimes?—Yes, but to a much greater extent.

6905. But one does occasionally see, unfortunately, people apparently absolutely insensible from drink. Would you say that those people were insensible to pain?—Yes.

6906. Do you mean that although it is almost impossible to rouse them, if an operation were performed on them they would not cry out or show some sense of pain?—Certainly they would not. That observation is as old as the time of Solomon, because Solomon makes the drunken man say, "They have beaten me, and I felt it not. I will go and have some more—I will seek it yet again."

6906A. Then in regard to that case which has been called the baking of dogs, I think it is well it should be absolutely understood that in the case of the first experiment it was merely an artificial external means of keeping up the temperature of the animal while it was under this dose?—Yes, that is so.

6907. And to prevent the loss of temperature, the loss of heat, which is induced by that dose, unless it is kept in by artificial means?—Yes.

6908. And I understand those artificial means were merely external in the shape of a warm foot-warmer and cotton wool?—Yes.

6909. And that it would not be applied to an animal in a greater heat than possibly a very warm bed into which a patient would be put?—Yes.

6910. And could in no case cause pain or burning, or anything to the skin?—No, in no case would it have caused pain, or burning, had the animal not been narcotised.

6911. A process such as that would hardly need any protection from the law?—Those experiments were done before the law was passed.

6912. It would not render the operator or exhibitor of it liable to prosecution for cruelty to animals?—No; but in some of those cases, in addition to this outward case, after the animal was narcotised, a cannula was introduced and warm air was passed in, and then that, of course, became an operation.

6913. Was not that the second series?—The second series.

6914. But the whole statement about this heating never exceeded a temperature of 111 degrees, which the animal was called upon to inhale?—We never reached as much as that. It was the animal that reached that temperature, but the external temperature was never so much.

6915. *Chairman.*) I think it was 106° you gave?—Yes, as the external temperature.

6916. (*Mr. Tomkinson.*) Do you know what the normal temperature of cats and rabbits is?—The temperature of the rabbit is an exceedingly variable thing; it has very little power of preserving a steady temperature; I speak under correction by Dr. Gaskell, but my impression is that it runs practically from about 100° to 104°.

6817. Rather higher than in man?—Yes, and in pigeons it is about 104°.

6918. You do not often use curare?—No, I have very rarely used it. I do not think I have used it at all since the passing of the Act, because I always had the feeling that curare alone might not completely destroy sensibility. I may, perhaps, mention that I was very anxious to find that out, and when I was in Leipsig in 1869, I proposed to Professor Ludwig to give me curare, and then, perhaps, apply a hot iron or something to the back of my hand, and keep up artificial respiration till I came to life again; and then I would tell him whether it would hurt or not; but he said that the risk was too great, that the means of keeping up artificial respiration were not sufficiently good, and he thought it was not worth while to do it—and he did not do it.

6919. With regard to inspection, you are not favourably disposed to increased inspection?—No, I do not think it would do any good.

6920. Have you seen a letter, in "The Tribune," I think it was, from Miss Lind-af-Hageby, headed "Vivisection and Inspection"?—No.

6921. She writes that while in the years 1903, 1904 and 1905, the number of experiments performed was 89,581, the united labours of the two inspectors led them to witness 93 experiments only, leaving the cost to the country, taking their salaries, 2,550 guineas, whilst 89,488 experiments were left uninspected. That shows a very small fraction of inspection of the experiments that take place. Now, in regard to inspection by the public, we will say, would it not be well, do you think, in order to set the public mind at rest, from your point of view, that there should be some right of entry by authorised people occasionally to witness experiments?—I do not know that it would.

6922. What would be the objection?—The objection would be that they would describe things in a way that was coloured by their own imagination. In the same way as they misstate what I have done with regard to warming animals, so they would be apt to describe any experiments in a falsified way.

6923. I did not in the least mean a newspaper reporter for the sake of copy, but, say, medical men, or perhaps Members of Parliament, on presentation of their card?—No, I think that anyone who wanted to inspect should be authorised. There would be no objection to having them if they applied to a central authority, the Home Office, and the Home Office thought they were right and proper persons to see them—then, by all means, let them go. But it should not be merely on presentation of a card. The Home Office should be certain that the person was a right and proper person to go and see the thing, and then, of course, there could be no objection whatever.

6924. (*Chairman.*) I will go back now to the general line of your evidence. I will leave out those matters which you have already dealt with in answering the members of the Commission who have been examining you. It has been suggested to us by several witnesses that the results of experimenting on animals and relying on the results of experiments has dulled the edge, as it were, of clinical observation, and that doctors do not now give the same attention to clinical observation that they did before. Have you formed any opinion as to whether that is so—do you think it has made any alteration whatever in the zeal with which a doctor pursues his clinical observations when he is attending a patient?—I do not know that that is so. Experimentation on living animals has given doctors so many more means by which they can readily ascertain things assuredly, that perhaps in some respects they may be said not to have the same zeal as before. To take an example unconnected with experiments upon animals, formerly a doctor had to depend entirely for his knowledge of fever upon inspection of the tongue, the feeling of the pulse, and the feeling of the skin. Now, instead of taking so much trouble with the pulse and skin he not infrequently just puts a clinical thermometer into the mouth; he reads off the temperature precisely, and he knows exactly what the fever is. He does that, of course, in less time and with less trouble, and he prefers to do it in that way. Perhaps if there were two doctors, one of whom was accustomed to trust to the thermometer and the other had never known of the thermometer, and they were put down by the bedside of a patient, if the doctor who used the thermometer had broken his thermometer he would not be so good as the man who had never used one.

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6925. But taking the whole knowledge and instruction of doctors together and assuming what you say about clinical observation, do you think that their power of dealing with disease has increased or the reverse?—It has increased incomparably; and I can speak to that because I came in just at the time when the change was beginning. When I was a student there were no remedies for producing sleep except opium, henbane, and tartar emetic. When a man had no fever on him we gave him opium or some henbane; and if he had fever we gave him opium and tartar emetic. I remember perfectly well when I was house physician one of the other house physicians got ill with typhus, and he was very sleepless, and we watched with great interest the introduction of a new remedy, bromid of potassium, to see whether it would give him any sleep. Since that time we have had any number of drugs to produce sleep. We have got chloral, veronal, sulphonal, and trional, and a whole lot of others, and we can now produce sleep in a way that we could not dream of doing before.

6926. Was bromid of potassium discovered by experiments on living animals?—I do not think so—it is a rather difficult thing to trace how it came into use. My own belief is that it came in rather by chance observation upon men.

6927. Were these other soporifics to which you have referred discovered by experiments on animals?—Entirely.

6928. Have they the same kind of operation as bromid of potassium to the same extent, or have they advantages over it?—They have considerable advantages. Bromid of potassium is more of a general nervine sedative rather than a sleep producer. These others are distinctly producers of sleep or soporifics, and they have all been made upon definite principles. They have all been tried upon animals, and have been introduced in consequence of experiments upon animals.

6929. And in those experiments on animals is the object first to discover the effect—whether they cause sleep, and then to discover also what is a safe dose?—Yes, both those things.

6930. And are experiments of both those kinds necessary before you venture to try them on mankind?—Yes, absolutely; because the life of an animal is so much less valuable than the life of man, and we cannot afford to test these things upon man.

6931. That is only an instance that you have been giving us in the matter of producing sleep; but I was asking you a general question, and I think you answered it in the affirmative, that you thought there had been a great improvement of knowledge?—An enormous improvement.

6932. Do you see any reason to suppose that that increase of knowledge would have come from mere clinical observation without experiments upon animals?—No. One may say that it certainly would not have come, because clinical observations have been carried on for very many centuries, and have led to nothing. Taking again the case of sleep, clinical observation has been carried on for millions and millions and millions of years, and all that time opium and henbane and tartar emetic were the only results; whereas in 50 years we have got any number of new drugs.

6933. Leaving out the question of surgery?—I am speaking only of the question of hypnotics or soporifics.

6934. But my question was rather general—would your answer apply to general medical knowledge?—Yes.

6935. And surgical knowledge?—And surgical knowledge as well.

6936. You refer in your *précis* to the value of various discoveries by experiment, with which I need not trouble you, because we have heard of them from several witnesses—Harvey's discovery of the circulation of the blood followed by others afterwards. With regard to the diagnosis of cardiac disease is that a thing in which experiments on animals have improved knowledge?—Our recognition of heart disease is based upon the change in the cardiac sounds. The explanation of those changes was first afforded by a Committee of the British Association who made experiments on animals and ascertained definitely that certain alterations in the cardiac sounds were associated with definite changes in the valves of the heart, and it is to those experiments that we owe our power of recognising the presence of cardiac disease.

6937. What was the date of that Committee?—1836, I think.

6938. Then in the treatment of heart disease, has advance been made in consequence of experiments on animals?—One may say that the whole treatment of heart disease is really dependent upon the results of experiments upon living animals.

6939. Both as to drugs and as to regimen generally?—Yes, in regard to drugs especially one may say. When I was a student digitalis was regarded as a drug which would weaken the heart, and was to be avoided in all cases of cardiac weakness, whereas now we know that digitalis is an extremely powerful tonic to the heart, and we use it in cases of cardiac weakness instead of avoiding it.

6940. I do not know whether you have had an opportunity of reading the evidence which Dr. Cushny gave before us?—Yes.

6941. Do you agree with his evidence about digitalis?—Yes.

6942. Then I need not take you into it at length, because he went very elaborately into it?—Quite so.

6943. Is the study of blood pressure in the blood vessels a matter of great importance?—It is, I think, a matter of the utmost importance in regard to the prolongation of life, because it is just when a man has gained most experience and has got the widest power that he very often begins to fail, and very often dies off suddenly, or to suffer from some condition of the heart which prevents him from taking any active part in life. The condition which arises with advancing years is one of increased tension within the vessels. We can now recognise that tension by means of instruments which have been worked out in consequence of experiments upon animals; but this would be of little use to us were it not that by means of drugs, which have also been worked out by experiments on animals, we are able to reduce this tension and to keep it down so that we are able to preserve lives for many years after they would otherwise come to an end.

6944. Why do you say that you discovered the state of tension by virtue of experiments on animals?—Because it is only by experiments upon animals that we have learned anything about the tension inside the vessels. The first experiments upon this point were made by the Rev. Stephen Hales, under the auspices of the Royal Society. He put a tube into the carotid of an animal and saw how far the blood mounted in the tube.

6945. We need not trouble you with regard to that; we have had it gone through?—But shall I explain the practical value of it.

6946. If you please?—When you take the blood pressure of a man (I wish I had brought the instrument down; it is very easily done; you can take it in half a minute now), if you find that the blood pressure is low, you can give the man a tonic which will act on his heart and vessels and raise the temperature up to the normal, thus increasing his power; but if you find that the blood pressure is above a certain tension, then you know that that man is liable either to have a vessel give way in his brain, and so to become paralysed, or to have the tension become too high for his heart to withstand, and then the heart begins to fail and stop, or he gets angina pectoris. In such a condition you can put him upon a diet and upon medicines which will lower his tension and keep it at the normal, thus preserving his life or preventing either the paralysis or the cardiac pain which would otherwise come on.

6947. I think you have already explained what you have to say about the effect of nitrite of amyl on angina pectoris, in answer to Sir William Collins?—Yes.

6948. I need not ask you any further questions about that. As regards dropsy, have there been considerable discoveries made in consequence of experiments on animals?—Yes, because through experiments on animals we now know that we can influence dropsy in more ways than one; that we can influence it through the blood vessels by preventing too much fluid being passed out, or we can influence it through the kidneys by causing them to secrete more freely, and we can influence it also by drugs which act upon the com-

position of the blood and prevent exudation from the vessels.

6949. These are things that you have learned to do since the beginning of the era of experiment?—Surely. These have all come within the last fifty years.

6950. These are remedies which had not been discovered so far by clinical observation?—That is so.

6951. I do not know whether you have said all that you wish to say about respiratory stimulants and strychnin?—I have said all I wish to say about that.

6952. I think you wish to tell us something as to abdominal surgery?—I regard abdominal surgery, really, as starting with some experiments upon animals. The first experiments that were made from which one can trace more or less continuously the present use of surgery in abdominal disease were the experiments of Ludwig and Thiry, who were investigating the action of intestinal juice. They cut out one piece of intestine and sewed the ends of the cut intestine together, making a continuous tube, leaving one part out.

6953. These were experiments on animals?—Yes. These experiments were continued for a long time, more or less, only in the laboratory, and then they were taken up by the surgeons, more especially by one of the Viennese surgeons, Billroth, who did a great deal in that way, and I think that Billroth's taking them up was really due to the influence of his colleague Professor Stricker, Professor of Pathology.

6954. And have these experiments made possible that and other operations in the intestines which were not possible before?—Yes, of course.

6955. (*Sir William Church.*) You would not say that directly; you would not say that those operations alone have made that possible; there have been others?—I meant that those were the starting point; and from those, I think, you can find a regular succession.

6956. (*Chairman.*) Of experiments of the same kind in that direction?—Yes.

6957. (*Sir William Church.*) That must be taken in connection with Listerism, for instance?—Experiments made before Listerism are like operations before Listerism; they would very likely be unsuccessful, and it is only since the introduction of antiseptic precautions that these experiments have been done very much in the laboratory or elsewhere.

6958. (*Chairman.*) I do not think it is necessary to ask you upon every one of these particular instances that you give, but you attach great importance, I gather, to the result of experiments with regard to myxedema and cretinism?—I do.

6959. Would you tell us what you have to say with regard to that subject?—I thought that had been taken up by some of the other witnesses. I do not know that I can give you such a connected history of myxedema and cretinism as some other man would, Sir Victor Horsley, for example, but practically it was found that the removal of the thyroid gland produced a condition in which cellular tissue became thickened and the animal became enfeebled; and in cases where that was done in man it produced a condition of imperfect action of the mind resembling those cretins who used to be found so commonly in some of the Swiss valleys; so that the function of the thyroid gland became connected with this condition of the thickened connective tissue and stunted growth and feeble development of the mind. It was then found that by giving the thyroid gland in this condition, especially to children, the feebleness of mind disappeared, the child began to grow, and at once the dwarfed idiot began to develop into a perfectly normal man or woman.

6960. Has this remedy of the thyroid gland been tried in the Swiss valleys, do you know?—I do not know. I have no doubt it has; but I should think there must have been something else that has led to the disappearance of the cretins; because 50 years ago in the Zermatt Valley you found the whole place crowded with them, with those enormous goitres; and before the introduction of the use of the thyroid these had to a great extent disappeared, so that there must have been some other factor, though I do not know what it was, which led to the disappearance of cretinism in those valleys. But this condition of myxedema now is very commonly recognised at once, whereas it was not recognised before, and we find that in almost every case now we are able to remove it, for a time at least, by the administration of thyroid gland.

6961. (*Mr. Ram.*) How is it used—hypodermically?—No; at first it was given hypodermically, but then we found it could be given perfectly well in tabloids, or the extract of the gland in solution could be taken internally and produced all its effects.

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6962. (*Chairman.*) I forget from what animals the thyroid is taken?—I think generally they take it from the sheep. 24 April 1907

6963. (*Sir Mackenzie Chalmers.*) From sheep killed in the ordinary way?—Yes.

6964. Immediately after death?—Yes, immediately after death—I believe so; but I am not quite sure whether other animals may not be used instead.

6965. (*Chairman.*) Some witnesses have made a great point—I do not know that it has been supported by any very precise evidence—of the danger of introducing the materials which form part of another animal into the human body, some upon the ground that there is something abhorrent to the feelings in it, and others that it gives to the person who takes that remedy some part of the susceptibilities of the animal which to some extent affect the mind and body of the person according to the animal from which it was taken. Do you, as a scientific man, see any foundation for that?—No. Of course the whole idea of eating meat at all is to many people abhorrent, but otherwise I do not see anything in it.

6966. You do not see any difference between eating a slice of a leg of mutton and some of the thyroid gland, as far as that argument goes?—No.

6967. Does it make any difference as to whether you give it through the mouth or injected into the body?—Yes, in some cases at least. For example, the blood of an animal cooked which you take into the human stomach is digested there and does no harm; but if it is injected into the carotid or into the jugular vein it would do harm. That is the reason why the old plan of infusing the blood of animals into man was given up. But there is no other reason for supposing that there is any special action.

6968. Of course it is very much the same as the objection taken to vaccination from a healthy calf?—Yes.

6969. From an unhealthy one, I suppose there would be an objection?—Yes. And then, of course, one does not like to say, that in a very widespread superstition there may be no element of truth; because we find that cannibals believe that if they take the heart of a strong man and eat it, they get his strength going into them; and the Hottentots and other uncivilised nations are very fond of eating the heart of a lion, so as to gain the strength and courage of the lion; so there may be something in it, but I do not think there is a great deal.

6970. I see you say that one of the most important inventions in regard to the relief of pain, may be considered to be the introduction of hypodermic medication, which is the result of experiments made upon animals?—Yes.

6971. I do not quite follow that. It would seem to one that there is nothing in this that could not be done by clinical observation?—It is quite true.

6972. That any hypodermic injection might serve the purpose, instead of swallowing?—The way it came about was this: Sir Robert Christison was a student of Orfila's in Paris, and Orfila made a very complete series of observations on the action of all kinds of drugs upon animals, which he recorded in his Toxicology. Sir Robert Christison was a pupil of Orfila's, and used to talk a good deal about Orfila and his experiments; he talked about them to old Dr. Alexander Wood, and Alexander Wood somehow or another thought that instead of using the intravenous injection of which Orfila was so fond, if he injected into the subcutaneous tissue he would get the same result; and he tried it, and it answered; and that is how it came about. I do not know whether Dr. Alexander Wood published definitely this, but what I am giving you now is the result of what I used to hear when I was a student, when I knew Christison, and slightly knew Wood.

6973. (*Sir William Church.*) Of course the improvement in chemistry had something to do with it?—Yes, undoubtedly.

6974. But it would not have been possible for Orfila to have made use of many drugs which are now used hypodermically, because their active principles had not been extracted in the shape that they now are?—No,

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but at first when Wood introduced the syringe it was employed almost entirely for morphin.

6975. Now, of course it is used for many other drugs besides morphia, and do you think with advantage, for instance, quinine in cases of malaria?—Yes.

6976. And also mercury in cases of syphilis?—I do. In the cases of malaria especially one knows that there are certain cases that do not yield to quinine given by the mouth, and do yield to quinine given by the hypodermic syringe. In regard to syphilis I have very little experience, because I do not see many of those cases, and I have no experience whatever in the case of mercury hypodermically given.

6977. In certain maniacal cases it is sometimes useful when you would have great difficulty in getting the patient to take the drug by the mouth?—Yes. In such cases it is of the utmost service, because the injection of a very small dose, even the hundredth of a grain of hyosin will probably quieten them down.

6978. Also apomorphia in certain cases of poisoning?—Yes, in such cases you can get active vomiting in a very short time, quicker than you possibly could do it otherwise.

6979. (*Sir Mackenzie Chalmers.*) By subcutaneous injection?—By subcutaneous injection; and then, of course, in the case of a bit of gristle just sticking in the throat and gradually choking the patient, if you can inject hypodermically apomorphin and get him to vomit, the gristle will be ejected.

6980. (*Chairman.*) Then with regard to experiments for the purpose of teaching, what is your opinion about that?—I believe that in order to thoroughly teach youths physiology and pharmacology in the way they ought to know it, you must have experiments. I have had both kinds of training. When I was a student no experiments whatever were shown, and I learnt my physiology from text books and from diagrams. I then went across to Germany and learnt it there by demonstration; and the difference between the two is just as great as learning how to put on a splint by reading an account of it in a book and doing it yourself. You get a practical notion of your physiology from demonstrations that you cannot possibly do from lectures.

6981. It cannot be done by anatomising?—No, it cannot be done, because anatomy deals only with structures, and physiology with the living functions.

6982. You approve of that course being followed?—Assuredly. If there was any pain involved to the animals, I should say that we had better not do that; but as it is a question where there is absolutely no pain involved to animals and a great deal of advantage to be gained by students and thereby a great deal of pain, I believe, saved to patients afterwards, I think it is the bounden duty of the teacher to give demonstrations.

6983. You think that anæsthetics ought always to be used?—Surely.

6984. (*Sir Mackenzie Chalmers.*) You would not allow the animals to recover, as I understand; you would limit it to operations where the animal was not allowed to recover?—I am not sure of that.

6985. (*Chairman.*) I believe you yourself were a witness before the 1876 Commission?—Yes.

6986. So far you are the only witness before that Commission who has appeared before us?—The others are dropping off.

6987. (*Mr. Ram.*) You say in your *précis* that when you were carrying on your investigations you were cut short by the difficulties occasioned by the Vivisection Act?—Yes.

6988. What was it in the Vivisection Act that squeezed your experiments out of existence?—The difficulty of getting a licence for a number of experiments without anæsthetics. You see, in order to investigate the action of snake poison, the injection of snake venom was requisite. This was looked upon as an experiment, and an injection of or application of the permanganate of potash by an incision was also looked upon as an experiment without anæsthetics. Of course the amount of pain inflicted was very small indeed; but then it was regarded as an experiment without anæsthetics; and the difficulty of getting leave for a sufficient number of such experiments was great, so great that we had really to give it up.

6989. But you were able to take to it again in 1904?—Colonel Rogers then did the experiment, but he was able to do it in this way: that in the interval Professor Waller had invented an apparatus by which chloroform anæsthesia could be maintained without interruption mechanically for 30 hours or more; so that Colonel Rogers was then able to do the whole thing under anaesthetics, and of course there was no difficulty in receiving a licence to do a number of experiments under anæsthetics.

6990. I am obliged to you; that answers that point. You say also in your précis that it seems to you that the law against experimentation ought to be very much relaxed?—Yes.

6991. Are you able to indicate in what direction you would relax it?—I should allow greater freedom, I think, in regard to experiments in which no actual pain is inflicted, for example, such as the inoculation experiments. It is simply inoculation of a microbe, but there is no actual pain; the experiment itself is really devoid of pain. These are now becoming among the most important of all the experiments; and in regard to them I should allow much greater freedom. I should not regard many of them as experiments at all. Perhaps I may just mention the view of the late Professor Huxley. He said to me one day—he was then a member of the Fisheries Commission: "If I take a live minnow and put a hook right through it, and then spin it round alive until I catch another live fish, and I pull the live fish, after many struggles and a length of time, out of the water, I am perfectly entitled to do that because I do it for sport; but if I scratch the nose of a salmon for the purpose of ascertaining the nature of the salmon disease, I am making an experiment, and if I have not got a licence I am liable to a fine of £50."

6992. Then as regards your relaxation that you were speaking of, in that direction would your wishes be met if a number of experiments of this non-painful nature were allowed; and would you still retain the law as at present, that that can only be done by a licensed person on licensed premises?—No, that is just where the difficulty arises, where you have a combination of a licensed man and a licensed place. If you license a man you might let him experiment in an unlicensed place. For example, other witnesses can tell you much more about it, but I believe the examination of grouse disease by a licensed man is rendered exceedingly difficult, or almost impossible sometimes, by insisting also upon a licensed place.

6993. We have had that case brought before us?—So I should say practically that if you licensed the man or the place, an experiment done in a licensed place under licensed supervision even by an unlicensed man might be allowed, or done by a licensed man giving a proper report in an unlicensed place.

6994. Would you limit that to experiments strictly non-painful?—No.

6995. The experiment might be painful and you would ask that it should be done by a licensed man, either in a licensed or an unlicensed place, or by an unlicensed man, provided the place was licensed?—Yes, that is the position I take.

6996. Are you aware of the state of things in America to-day?—I know that in many places there is a great agitation being got up, very much the same as in this country.

6997. In favour of what?—In favour of anti-vivisection.

6998. Do you know that there is a large body there of scientific men who are asking not to have no regulation on vivisection, but to have exactly such an Act as we have here?—No.

6999. I think I have only one other matter to trouble you with. In your *précis* you say that it is through experiments on animals also that the nature of the most dreadful of all respiratory diseases, namely, phthisis, has been discovered, and that we have already begun to lessen, and you trust ere long will stamp out the disease. Can you elaborate that a little? What is being done with regard to ascertaining the nature of phthisis?—That has now been ascertained to a very great extent indeed. We now know that phthisis depends upon an invasion of the lung by a certain bacillus, the tubercle bacillus; that in many cases, however, the infection is not a pure infection but a mixed infection. That there are various other microbes associated with the tubercle bacillus, and

which aid its invasion of the body. But if we could simply prevail upon all people to recognise the infective nature of phthisis, to prevent spitting in all public places, to take proper precautions against the conveyance of the tubercle bacillus from one individual to another, in 30 years phthisis would be just as rare in this country as leprosy is now.

7000. Is that true also of all tubercular disease?—I believe that all tubercular disease could be stamped out entirely in this country in 30 years. The chief objection to it is the expense; because in order to stamp out tuberculous disease, you would probably require to slaughter a large number of cattle; and that is the crux of the matter in regard to animals, and in regard also to the tubercle, more especially in the abdomen of children.

7001-3. Is it your view that tubercle in the abdomen of children is contracted from cattle, from milk?—I think so.

7004. (*Sir Mackenzie Chalmers.*) From milk or meat?—I believe it is much more from milk; because the heat that is used in thoroughly cooking, either by boiling or by roasting meat, is sufficient to destroy the vitality of almost all bacilli; and milk is a vehicle in which bacilli can be conveyed, because it is almost always used without being sterilised.

7005. (*Mr. Ram.*) I think I understood you to say that the use of bromid of potassium as a sedative came in, you think, from clinical observation?—I think so; but I am not quite clear about it.

7006. That was after the lapse of centuries of clinical observation?—Bromid of potassium was really only a more or less new product.

7007. Has the use of useful drugs which are now commonly used increased very largely since experimentation came in?—I do not quite know that I follow that question.

7008. What I mean is this: Of course, all drugs in the days before experimentation on animals came in by clinical use?—Yes.

7009. And it took a long term of years to get a certain knowledge of a certain number of drugs?—Yes.

7010. Has the use of drugs which have turned out to be useful been very much more rapid; has there been a large number of drugs added to the repertoire of useful drugs since experimentation on animals?—Yes, the increase in the number of useful drugs added to those which were formerly employed has been very great since the introduction of experiments on animals.

7011. Has experiment upon animals also had this effect, that it has given an accurate knowledge of previously known drugs of which the use may have been mistaken before that?—Yes.

7012. (*Sir William Church.*) With your experience should you say that there was any difficulty in keeping animals under the influence of anæsthetics for lengthened periods?—None whatever.

7013. Not in the case of dogs?—No.

7014. Do you think that physiological experiments, not directly connected with medicine, have been of great value to medicine?—Yes, an enormous advantage. Many of the experiments which have had apparently little or no connection with medicine at first, have turned out really, afterwards, to be of the utmost possible value.

7015. You remember, perhaps, the way in which chloral was discovered?—Yes, I do.

7016. Do you think it is not probable we should have arrived at the use of chloral if it had not been for Liebreich's experiments?—No, we certainly should not.

7017. That was one quite unconnected with practical medicine—his original experiment. I will not trouble you with it, because we have it from Professor Cushny; I only wanted to get confirmatory evidence; but his original experiments had nothing to do with practical medicine?—They were originally intended more from a scientific point of view. It was from the scientific point of view that he started.

7018. And the action of sulphonal in the same way, I think, was not found out by experiments directed towards practical medicine?—No, it was incidental; it came out in a by-issue in some other experiments.

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7019. Was not also the same the case with regard to antipyrin and antifebrin?—No, those, I think, were really the definite results of experiments made on animals upon definite lines. The starting point of the connection between chemical constitution and physiological action was really, I think, in modern times made by Dr. B. W. Richardson. Somewhere about 1844, Dr. James Blake, under Dr. Sharpey's direction, made experiments on the relations between physiological action and chemical composition; but then there was a great lapse of about 20 years, and then Dr. B. W. Richardson took up the chemical series of the alcohols, showing that there was a relationship between the chemical constitution and the physiological action of the different alcohols. Following upon this came the experiments of Crum Brown, and Fraser, who found that if you alter the chemical constitution of strychnin by the introduction of methyl, its physiological action becomes altered, and instead of becoming a convulsive poison it becomes a paralysing poison, like curare. Then after that followed a number of experiments with the benzine compounds, and it was found that many of them had the power of lowering temperature. A series was then produced of synthetic chemical compounds of the benzine series. These were tested, and various of them were found to be practically very useful antipyretics.

7020. To interrupt you for a moment, was not the idea of the original experiments of antipyrin that it was antiseptic rather than antipyretic; that it counteracted septic fever in animals and fever produced artificially in animals?—It is quite possible that you may be correct, because the antipyretic and antiseptic actions are very closely combined in the benzine series; so that—I cannot quite recollect it now, I have not looked up the subject—it is quite possible that they were working on antiseptic lines, and found that the antipyretic power was the one most definitely pronounced. But those two lines run quite parallel; so that now we have got a great body of artificial antipyretics and antiseptics produced which are all made by the trained chemists who prepare these substances on definite chemical lines, and then they are tested physiologically upon animals. If they are found to be satisfactory they are then used upon patients.

7021. Could you tell us how cocain came to be introduced into medicine?—So far as I can recollect now, Von Anrep was the first man to introduce it.

7022. Is it not the case that he was experimenting for physiological purposes rather than for a practical end?—Yes.

7023. Up to that time cocain was not known as an anæsthetic, but as a stimulant enabling fatigue to be borne?—That is so.

7024. (*Colonel Lockwood.*) Do you say that that was the case?—Yes.

7025. That he was experimenting for other purposes?—Yes, physiologically. The first experiments upon cocain which might lead to a knowledge of its anæsthetic action really were made by the late Dr. A. Hughes-Bennett and Professor MacKendrick, because they demonstrated that it destroyed the power of sensory conduction in the cord; but for some reason or other these experiments were not followed up, and it was Von Anrep who actually gave the first actual impetus to the introduction of cocain into the practice of medicine.

7026. (*Sir William Church.*) Then he was followed by Koller, who made use of it for ophthalmic purposes?—But if I remember rightly it was used only for ophthalmic purposes in man after it had been previously tested on the eyes of animals.

7027. And now local anæsthesia for which cocain, or eucain, or one of the similar bodies is used is a very important addition to medicine?—Yes, it is, and in addition to that purely local use, it is now being used by introduction into the spinal cord, which is a form of use which is rather based, one would say, upon the very old experiments of MacKendrick and Bennett.

7028. With regard to some experiments which have been mentioned in this room by witnesses on both sides, experiments upon digestion and upon the passage of food down the intestines, it has been stated here that all that was of value was learned by Dr. Beaumont from the examination of the stomach of Alexis St. Martin. How far do you think that is true?—That is not correct, of course. Dr. Beaumont's experiments were very valuable indeed, because it was

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a case of experiment upon man. The side of poor Alexis St. Martin was blown away by a charge of duck-shot, so that they could see inside, and Dr. Beaumont made very good use of his opportunities; but that only went a very short way. It showed that the gastric juice had the power of digestion; it showed that when Alexis St. Martin drank too much spirits his stomach got very much out of order, and that was about the end of it. Dr. Beaumont also found that if he stirred up the stomach very much with a glass rod, or something of that sort, Alexis St. Martin might feel a little sick; but the whole of the rest of the practical introduction of such things as pepsin, pancreatin, and all the adjuncts to digestion have resulted from the experiments upon animals, made afterwards by Blondlot, by Bernard, Bedder, and Schmidt, and others, and the most recent have been made by Pawlow.

7029. In your opinion is gastric fistula in an animal or an intestinal fistula in an animal a condition in which it suffers great pain?—Certainly not.

7030. You say that confidently?—I say that confidently.

7031. Why do you speak so confidently?—Because years before the Act I made a gastric fistula in an animal, in a dog, which never showed the slightest sign of pain, and when I wanted to examine inside the stomach it showed great delight—just like a dog that has been sitting about the house and wants to run out for a walk. When it saw that I was going to look into its stomach it frisked about in the same way as if I was going to take it out for a walk.

7032. (*Colonel Lockwood.*) How did it know what you were going to do?—Because I did it day after day. I made the fistula and the fistula healed up. Then I had a cannula in the stomach and corked it up so that I could look in.

7033. I confess that I am rather aghast. If I understand you rightly—I do not want to do you injustice—the dog showed great delight when you were about to perform an operation on its stomach?—No, after the operation had been done and the thing was healed up, when I wanted to examine it it seemed delighted.

7034. Because it afforded it relief, I suppose?—No, it was because it liked to be made much of and petted and shown round.

7035. (*Sir Mackenzie Chalmers.*) It liked to be taken notice of?—Yes.

7036. And there was no pain?—There was no pain. Practically, one knows there is no pain afterwards. For example, just two days ago a celebrated surgeon did an operation upon a patient whom I had seen because the patient was then suffering from cancer of the œsophagus and could not eat. The patient was almost dead when he came to the surgeon, who then injected into his veins a quantity of saline solution. He was brought round sufficiently far to do the operation, and then, having got it done, he poured into his stomach a lot of nutriment. The patient is now very comfortable and has no pain after the operation. I have no doubt, if you are at all doubtful about it, that I can get a certificate from the patient.

7037. I will take it from you, of course.

7038. (*Sir William Church.*) As a matter of fact, in men, is the opening of the stomach a very painful operation?—No.

7039. In fact the actual opening of the stomach is usually done without any anæsthetic at all being administered. The attachment of the stomach to the wall is done under anæsthetics?—Yes.

7040. And after the stomach has been opened the patient seldom or never experiences pain in the passage of the tube into it for food?—That is so.

7041. You answered some questions about myxedema, but it is stated now by anti-vivisectionists that iodin used to be given before any experiments were made on the thyroid, and that it was a cure for the condition of myxedema or cretinism, or, at least, that it has acted beneficially. I should like to know your opinion upon that?—It is quite possible that it might act beneficially, but it certainly was not a cure.

7042. And now the same effects that were obtained by feeding with thyroid gland can be got by a substance which is called thyroidin, which is, of course, obtained from the gland. Do you know anything about it; or would you rather not say anything about it?—I do not quite see how that bears upon the question one way or another; because the use of thyroidin is as much derived from experiments upon animals as the use of the thyroid gland itself.

7043. It is really not a true distinction; it is a mere play upon words?—Simply a play upon words.

7044. Have you turned your attention at all to the value of experiments on animals in connection with preventive medicine?—No, that is such a very large subject.

7045. I meant, rather as a means of diagnosis, have you turned your attention to that. We have been told here by an authority that the surest and the best method of diagnosing the presence of certain diseases that may be imported at our ports is by inoculation experiments in plague, cholera, and such like?—I cannot give you any definite information about that. It has not come in my way.

7046. Do the answers that you gave to Mr. Ram cover all that you would like to say with regard to the restrictions that the Act imposes upon experimentation upon animals, or would you like to add anything more? I gather from your answers to him that you are in favour of allowing persons who hold a licence to experiment where they like?—Yes.

7047. And, on the other hand, that persons who did not hold a licence might experiment in licensed laboratories?—Yes.

7048. But I suppose you are in favour of the present law, that no one should do experiments upon animals excepting the holder of a licence or in a licensed place?—Surely.

7049. Do you think there are no restrictions further that you would like to see removed? Are you satisfied with the present licences and certificates?—Perhaps, in regard to that, I am rather at a disadvantage, inasmuch as I have not been able to work under licence or certificate for some time, so that those in more active work than I am could probably give a better answer. So far as I am concerned, I think the licence and certificates are all very well. I do not know that I should advise very much alteration except in the way I have mentioned.

7050. You do not think that the having to get special certificates such as are now required interferes seriously with the work in scientific laboratories?—To some extent it does, as I found in my own case when, some two years ago, or rather more, I gave a course of lectures on the therapeutics of the circulation at the London University. I did not allow sufficiently long a time for obtaining a licence and a certificate to show experiments under anæsthetics. The consequence was that when I began to give my lectures I found that I had got over a considerable extent before I got any licence or certificate, and the result of that was that I was unable to go on in the regular sequence. That disturbed me very considerably. The delay in that case was of very considerable importance, and disturbed very considerably the lectures I should hav given. If I had been able to get a certificate and licence at once, say within a few days, it would not have mattered, but, as it was, I was kept waiting a considerable time—I forget exactly how long; I think between a fortnight and three weeks—and it was very inconvenient.

7051. But still you would not press for such a point as this, for instance: that a person holding a licence should be freed from the necessity of getting certificates for experimentation on certain classes of animals and for keeping animals alive after experimentation?—I think if you have got a licence as a recognised good worker it might be at once endorsed: "This licence carries with it the liberty to work upon such and such things." That would greatly free a man, for this reason, that every now and again, in every research, a question crops up, and you wish to get that question answered; if you have to wait to get a special licence before you can tackle the question the opportunity for answering it has sometimes gone; so that if, instead of requiring special licences on every occasion, the licence simply carried with it, "This licence allows the operator to do so and so," at the very beginning, that would greatly lessen the hampering of research.

7052. (*Colonel Lockwood.*) How long do you suggest that licence should last?—I think a year.

7053. (*Chairman.*) Then you dispense altogether with the intervention of any medical body or authority who

has to approve the certificate as being for a proper reason?—It would not be necessary then.

7054. It would not be necessary, because you would have dispensed with it by the licence?—I take it that this certificate would not be granted without previous inquiry into the candidate's qualifications.

7055. Yes, but you make it simply a question of qualification, and having got a man who is qualified to hold a licence you would have him qualified to perform operations which now require a certificate—that is to bring the animal to life again without killing it?—Not necessarily. You might have a simple licence, and then if the experimenter thought that he foresaw the possibility of requiring something more, he might at once indicate beforehand the probability of his having to do certain experiments, which could be then submitted to the proper body, and with their sanction a certificate could be given.

7056. That is qualifying a good deal what you said before?—That is really what I mean. I did not think of a licence being granted simply to everyone, with the idea of his doing everything he pleased. I did not think that a licence carrying the power to do everything should be given to every applicant, but that when the applicant had a plan before him, involving the probability of various extensions, he should put down, as he is required to do now, I think, what he wants the licence for, and that in the event of his probably needing extensions, these could be at once granted beforehand to him in the licence, instead of his waiting until the need arose, and then applying for a special certificate.

7057. You would add to the present Act, without repealing the present provisions, a clause granting a special licence in a particular case, with special privileges as regards operations that might arise out of a particular leading operation?—Yes.

7058. (*Dr. Gaskell.*) You meant rather, did you not, that this special licence should only be given to well-tried men?—Surely, to trained men to be approved by somebody, as at present.

7059. (*Sir William Church.*) I only want to ask you one other question, and that is this: Do you think there is the slightest ground for saying that the witnessing, or the performing of experiments on living animals in any way affects the character of the spectator or the operator?—No, there are no grounds whatever for it. I may say that there are no more grounds than for believing that looking at operations on patients blunts the susceptibilities of man. My own belief is that, whereas surgeons who have been accustomed to see and to do surgical operations, have less of the hysterical element about them, and do not faint or become helpless at the sight of the suffering and pain of others; they have become on the contrary much more tender-hearted and much more willing to put themselves to a great deal of inconvenience, and to sacrifice themselves in order to save pain to others; and I believe that there is no more injury done to the moral faculties of the students who see experiments performed simply in order that they shall learn how to give relief to their patients than there is when they see the surgical operations done to give relief to their patients.

7060. (*Sir Mackenzie Chalmers.*) To follow that up, I suppose that when a man faints at seeing an operation, it is really because he is sorry for himself, and not because he is sorry for the patient?—Yes.

7061. And when he gets used to it, it ceases to affect his own nerves, but it does not affect his feeling for the patient?—That is so; that is very truly put.

7062. You are a Fellow of the Royal Society as well as a Fellow of the Royal College of Physicians?—I am.

7063. How long have you been a Fellow of the Royal College of Physicians?—About 30 years, and I have been a Fellow of the Royal Society, I think, for 33 years.

7064. In this inquiry we are anxious to get evidence from both sides. Can you give us the name of any Fellow of the College who could give us evidence against the view that you have been taking to-day?—No, I do not know of any; of course, there might be such.

7065. But you do not know any one whom you could suggest to us?—No.

7066. Now I want to go more into general questions. There is a great deal of feeling, naturally, about dogs. If experiments on dogs were prohibited would medical science suffer?—Yes.

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7067. You think they are essential?—It is essential that dogs should be used.

7068. Have you read Professor Starling's evidence on that point?—I have read it, but I have not looked at it recently.

7069. However, speaking from your own experience, do you think that it would be possible to dispense with them?—No, I do not.

7070. Would you give us your reasons?—Because other animals are too small. For many of the experiments guinea-pigs, rabbits, and cats are too small, and other animals of the same size, such as sheep, goats, and pigs are too expensive.

7071. Is there any importance in the fact that the dog is carnivorous?—I think there is. I think that in experiments, for example, on the composition of chyle or digestive experiments they are very important.

7072. We have had some evidence given by two or three witnesses who are against experiments on living animals, to the effect that an animal differs so materially from a human being that only false deductions can be drawn from experiments on animals; do you agree with that view?—That is quite untrue.

7073. There are differences, of course?—Yes.

7074. But allowing for those differences, can an experimenter get trustworthy results?—Yes. If I may give an example, if you give nitrate of amyl to a man it quickens his pulse very greatly; if you give it to a rabbit it very often does not alter the pulse rate at all; the reason of that is that the pulse rate in a rabbit is very quick to start with. If you give nitrite of amyl to a dog it has the same action as upon man—it quickens the pulse rate; but if before you give it to a dog you cut the vagi in the neck so that the pulse rate becames as quick as it is in the rabbit it does not quicken it any more than it does in the rabbit.

7075. The engine is running at its maximum speed?—That is just it, but when you get those two experiments together you can at once see what is the action of nitrite of amyl.

7076. (*Dr. Gaskell.*) You would not draw any limit as to how low you might go down in the animal kingdom and still get advantage?—No, I do not know where you could draw the limit, because we are now going lower and lower in the animal scale and still get an advantage.

7077. (*Sir Mackenzie Chalmers.*) You yourself have not used curare since 1875, I understand?—To tell the truth I do not recollect when I last used curare.

7078. Would any detriment to science result if curare was prohibited, or if further restrictions were put upon its use?—I do not know. For my own part I should not be inclined to allow the use of curare as an anæsthetic.

7079. The Act prohibits that?—I think that is quite right, but if an animal is anæsthetised I do not see why you should not use curare. It will not increase any pain—it will do the animal no harm, and it might possibly be of service in keeping the animal quiet. There are certain cases which one can conceive where the employment of curare might be very useful. For example, if you wish to find out the effect of any stimulation of a sensory nerve upon the circulation, apart from any movement induced by a motor nerve, you might find the employment of curare advantageous, so that I think I should certainly not prohibit its use.

7080. (*Dr. Gaskell.*) Would it not also be of use in the case of peripheral stimulation of nerves to find out the question of circulation of blood in the muscles?—Yes.

7081. That could not be done without curare?—No.

7082. (*Sir Mackenzie Chalmers.*) Taking your evidence generally, am I correct in saying that you think that the continuation of experiments on living animals is indissolubly bound up with advance in medical science?—Yes.

7083. I take it broadly that the distinction between observation and experiment is that in the case of an experiment you deal with conditions artificially produced, and in the case of observation you have to wait until Nature or some external cause produces those conditions?—Yes.

7084. And you may have to wait an indefinite time?—Yes.

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7085. And you cannot isolate the factors?—That is the chief difficulty.

7086. For instance, in the testing of drugs by observation, you only get an empirical result?—Yes.

7087. And by experiment you get a scientific result?—Yes.

7088. You get at the cause, and you can isolate the various conditions one by one?—Yes, that is so.

7089. Now, coming to particular cases, two witnesses have referred to your own experiments, which I may call baking and freezing experiments, and you have explained the baking experiments. May I take it that the temperature to which the animal was exposed was in no case higher than that to which an ordinary human being goes in a Turkish bath?—Certainly, in no case.

7090. So that if the animal had not been anæsthetised no more pain or discomfort would have been experienced by that animal than if it had been put into a Turkish bath?—Surely.

7091. (*Dr. Gaskell.*) So far as temperature goes?—Yes.

7092. (*Sir Mackenzie Chalmers.*) I am speaking only of temperature. You did not give us any evidence about what one of the witnesses before us called the freezing experiments. I suppose you have tried the effect of reducing the temperature?—Yes.

7093. Do you happen to know to what extent the temperature was reduced?—In one of the experiments on the action of aconite, the pigeon was shivering as if it was very cold, but its temperature was 103.1.

7094. What was the temperature of the air to which it was exposed?—The temperature of the air was 100 Fahrenheit.

7095. That is practically ordinary blood temperature?—Yes; but then it was low for a pigeon.

7096. Did you in any of these experiments bring down the temperature below freezing?—I do not think so, so far as I can recollect.

7097. May I put it in this way: As regards low temperature, so far as you remember, did you expose any animal to any temperature to which it would not be subject in its wild state on a winter's night?—Certainly not, and then, of course, every precaution was taken that there should be no local irritation, either from excess of heat or from excess of cold applied, for example from a portion of the cold metal touching the skin. The boxes were always padded, either with cotton wool or with paper, so as to prevent any local irritation.

7098. Have you followed lately the experiments on snake poisoning with Calmette and Fraser's sera?—Yes.

7099. Have any improvements been made lately?—I do not think so. I do not think that Calmette has made any further improvements.

7100. They were tested some years ago by Dr. Douglas Cuningham, I think?—I think they were, but I had forgotten it.

7102. You have not followed out that particular branch lately?—No.

7102. How many years is it since you first made experiments on animals?—I first experimented in 1865.

7103. And you have practically held a licence up to quite recent years, have you not?—No, my work has been very much interrupted. Practically I have not done a great deal of work myself since about 1877, because at that time I found that my practice was getting in the way, and the only plan of doing experiments was to get an assistant whom I paid, who should carry out the experiments which I devised.

7104. But still you have been connected with people who have been experimenting on living animals for many years?—Yes.

7105. Have you ever known of any case of inhumanity or want of consideration for causeless pain; I am speaking of England, of course?—No; in England certainly not, and I do not remember any case abroad, because, as I said before, most of my experiments were done in Professor Ludwig's laboratory in Leipsic, and he was always most careful of the feelings of the animals that were experimented upon.

7106. In those that he saw?—He would not allow anything that approached to cruelty in his laboratory. So careful was he, that whenever a frog was killed by cutting off the head, he would not allow the student to throw away the head; he said, you must destroy the brain, because you do not know whether that brain may not have the power of sensation still.

7107. Have you seen a very convenient presentation of the case against vivisection drawn up by Surgeon-General Sir James Thornton?—No, I do not think I have seen it.

7108. He has collected his statements in alphabetical order?—No, I have not seen it.

7109. And on some of these statements you yourself have given evidence. May I put to you some of his statements and ask you for your comments as to how far you agree with them or not?—Surely.

7110. Speaking of abdominal surgery, he says: "Abdominal surgery has been seriously retarded by experiments on living animals. The following statement was made by Sir Frederick Treves in his 'Address on Some Rudiments of Intestinal Surgery,' delivered before the Midland Medical Society at Birmingham, and reported in the 'British Medical Journal,' November 5th, 1898:—'Many years ago I carried out on the Continent sundry operations upon the intestines of dogs, but such are the differences between the human and canine bowel, that when I came to operate upon man I found I was much hampered by my new experience, that I had everything to unlearn, and that my experiments had done little but unfit me to deal with the human intestine.'" How far do you agree with that statement?—Sir Frederick Treves has, of course, more knowledge of his own condition than anyone else; but I believe that he has since modified his statements so as to show that the impression that it appears to have conveyed to Surgeon-General Thornton is incorrect.

7111. Apart from Sir Frederick Treves' opinion, what is your opinion?—My opinion is definitely that but for experiments on animals abdominal surgery would not now exist.

7112. The next thing he discusses is anthrax. I do not know whether you have paid special attention to that or not?—No, not special attention.

7113. I will only take those heads which I think seem to refer to yourself. Another head is the Calabar bean. He says there: "The most conflicting testimony is given by different physiologists as to the action of Calabar bean upon men and animals, and it appears that no positive conclusion can be reached as to its action upon the vagi nerves. Dr. Harley declares that it does not affect the arteries when applied locally. Dr. Fraser contradicts this, and says he has demonstrated that the local application of the drug produces dilation of the arteries." That is brought forward to show that experiments on Calabar bean have been futile. What is your comment upon that?—Practically, Calabar bean is not used for its action upon the vessels, but it is used for its action upon the eye, and that was discovered first of all by Fraser in his experiments upon animals.

7114. This passage therefore, you think, is a little irrelevant to the question whether experiments on living animals with Calabar bean have been useful or not?—I think so. With regard to the vagi, that is a question of dose. With a small dose you will get a stimulation of the vagi; but if you give an enormous dose you get paralysis.

7115. You get different results, I suppose, even if you take common salt?—Yes, with a little common salt and water you get an appetite; with a lot of salt and water you get the exact opposite.

7116. Then he goes on to chloral hydrate. He says: "Experimenters contradict each other about the physiological action of this drug in the most bewildering manner. Chloral hydrate has not sustained in the case of human beings the conclusion derived from its use upon animals; indeed, it is a question whether it has not proved an evil rather than a benefit in treating disease. Of its injurious effects on the human mind there is now no doubt, though it was at first recommended expressly on the ground that it had no such evil mental results. The error naturally arose from experiments being made with it on animals with no mental constitution analogous to that of man." What is your comment upon that?—There, again, it is a

question of dose and a question of amount. What was made out in animals was the question of the soporific action, and, to a certain extent, an analgesic action upon animals. Both of these are applicable to human beings. The effect upon the mental power of human beings is that if you give chloral hydrate in proper doses it will produce quiet sleep and comfortable sleep, followed by no bad results, for a long time together; but when people get into the habit of drugging themselves with chloral hydrate they may bring on mental aberration, and I have even seen a patient who was confined in a lunatic asylum because no one knew she was in the habit of drugging herself with chloral hydrate until she was confined, when the whole thing came out, and then she became cured. But there has been no fallacy introduced at all by experiments upon animals. The experiments upon animals were restricted to finding out certain points about chloral hydrate which were definitely ascertained by the experiments on these living animals. You cannot give an animal chloral hydrate for months and months together to find out the effects upon its mental condition.

7117. The next subject that Sir James Thornton goes on to deal with is chloroform. I think you have made experiments on chloroform yourself in Hyderabad and elsewhere?—Yes, very many.

7118-9. After pointing out that chloroform was not discovered by reason of experiments on animals, Sir James Thornton proceeds to say: "No experiments were ever made in connection with the discovery of the anæsthetic properties of chloroform." Do you agree with that statement?—So far, it is right in that the anæsthetic action of chloroform was discovered by experimentation upon man, unless the story be true that it was accidentally discovered in the voluntary experiment of a dog upon itself. The story is—I do not know whether it is true or not—that a chemist had got hold of some of this new stuff, which he put in a little jar in a corner of his shop; and one day a lady came in with a little lapdog. The little lapdog went smelling all round, and smelt at this jar, when it fell over insensible; and that was what started Professor Simpson on his experiment. But the definite experiments in regard to its anæsthetic action were really made by Professor Simpson upon himself and on Dr. Matthews Duncan and one or two others.

7120. But, as regards the safest way of keeping it and the best way of administering it, has that been investigated by experiments upon animals?—It has been investigated by experiments on animals, but a good deal of it has been made out upon man. The number of experiments upon man, as compared with the number of experiments upon animals, is simply enormous.

7121. (*Colonel Lockwood.*) Then Sir James Thornton is right in that?—Yes; I think he is so far right. I should not contradict that.

7122. (*Sir Mackenzie Chalmers.*) Then he says generally, under the heading of "Cruelty of Vivisection": "It is unnecessary to enlarge upon this point, as the cruelty of experiments on living animals is self-evident, and is admitted by most of the experimenters themselves." What is your comment upon that?—It is not true; it is not admitted. The cruelty of experiments upon living animals is not great, and it is not admitted. There is very little pain inflicted in experiments upon living animals, and I should say that there is no cruelty at all—that any pain that may be inflicted is either inadvertently inflicted, and there is very little, if any, of it; or it is because they think that by, possibly, putting an animal to a certain amount of inconvenience (I would hardly say pain), they may save pain to their patients.

7123. The next point he deals with is diabetes. Is that a question you deal with—I will read you what he says upon it—or would you rather not express an opinion?—I am quite willing to answer any question about it.

7124. "It has been claimed," he says, "that the experiments of Claude Bernard, Schiff, Pavy, and others have thrown much light upon this disease, but, however interesting from a scientific point of view, these investigations have not enabled physicians to cure the disease. As Dr. Althans observes, glycosuria which is experimentally induced is not diabetes such as we observe it clinically, but only a passing physiological storm, which is certain to disappear in a short time, and which, moreover, reproduces only one of the symptoms of diabetes. The experiments referred to are therefore of no value as regards the cure of this disease." What is your comment upon that?—It is simply this—that the experiments of Claude Bernard and others have taught us a great deal about the nature of diabetes; that they have not taught us all yet, and because we have not learnt everything it is simply useless to decry the utility of the things we have learnt. It seems to me that in any physiological or vital problem we deal with our materials as a man does with a bridge. A man goes on building the piers of a bridge on both sides for a length of time, a couple of years perhaps; there is no sort of good. Nobody can pass across the stream or the valley until at length they are so far completed that the keystone is put in, and then over the whole thing they go. But unless the piers are built first you will never get the keystone in. With diabetes we have been getting the piers built up, and before long I think we shall get the keystone in; but I am quite willing to grant that we do not know anything like the whole pathology of diabetes yet.

7125. As the result of experiments, can you alleviate diabetes in any way?—Yes, very greatly indeed; because before Bernard's experiments we did not in the least know that the amount of sugar that was passing was due to the conversion of starch into sugar, whereas now we know that, and we limit the diet of the diabetic to proteid foods, cutting off the starches and the sugar as far as possible, and giving them only proteid and fatty foods. In this way we can lessen his sufferings and can prolong his life.

7126. The patient remains in a chronic state, instead of the disease becoming acute; is that it?—Not always. There are many cases where there is sugar in the urine and where, if the sugar were allowed to remain, the patient might perhaps pass on to a condition of true diabetes. These cases we now term glycosuria only, and by simply putting them upon a proper dietary for a length of time we stop the sugar. We can then gradually give them back the farinaceous food, and even the sugar, without any sugar appearing in the urine; in fact, we cure them permanently, although if the disease were allowed to go on it would probably have turned into chronic diabetes.

7127. His next statement relates to digestion, and Sir James says: "The most valuable contributions in this direction are those of Dr. William Roberts, of Manchester, and the experiments necessary for them were made in laboratory vessels, not on animals at all. In fact, all experiments upon digestion can be made outside the living body with greater accuracy and ease than in it." What is your comment on that?—My comment on that is that we can experiment with the juices (pepsin, pancreatin, and so on) outside the body, but until we had learned by experiments upon living animals the relationship of these with different organs, we could not have carried on these experiments, which can now be carried on in the laboratory—that all the foundation has been laid by experiments on living animals, and now other experiments can be made without any further use of animals.

7128. He then goes on to deal with diphtheria and antitoxins. I do not know whether you have any special opinion on that. We have had evidence from Sir Douglas Powell about it?—My own experience is that the diphtheria antitoxin is an extraordinarily valuable thing, that it practically robs diphtheria of much of its terror. Before, diphtheria was a frightful disease. You got a sudden call to see a child affected by it, but you could do little or nothing for it, and the child died; whereas, with the antitoxin you simply find that the membrane sometimes will melt away like butter.

7129. At any rate it is a powerful remedy, not a cure?—It is a powerful remedy, and a most useful one.

7130. Referring to the antitoxin Sir James Thornton says: "It may cause paralysis, kidney disease, heart mischief, and other disorders, and in some instances, when used on healthy persons as a prophylactic, it has caused sudden death like a dose of deadly poison." What is your comment upon that?—I should think in relation to the alteration in the circulation, to which he refers, as well as the paralysis are much more likely due to the diphtheria disease than to the antitoxin—to the toxin, and not the antitoxin. So far as regards these cases of sudden death, I do not know anything about them; but what one does know is that

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many people are very liable to faint from very comparatively slight operations.

7131. Like vaccination?—Such as vaccination. I was told that on board the troop boats taking young, healthy officers out to South Africa, the number of men who fainted on simply being inoculated for enteric fever was extraordinary, and sometimes in certain peculiarly nervous people, instead of getting simple syncope they get fatal syncope.

7132. You would attribute the deaths then rather to what I may call sudden syncope than to the toxic action of the serum?—Yes.

7133. But you do not know the cases to which he refers?—No, but it is quite evident that it was not due to the serum, because the toxin does not act instantaneously like that.

7134. You think that that observation of his is unfounded, or made under a misapprehension?—People may have died from the disease, but it is not from the serum.

7135. You disagree with his statement that "when used on healthy persons as a prophylactic, it has caused sudden death, like a dose of deadly poison"?—Yes, it may be so, but I should say that the operation of inoculation has caused it, but not the serum.

7136. Then the next heading he takes is drugs generally, and Sir James says: "It has been asserted that 'nearly all the facts definitely known regarding the action of remedies have been gained by experiments on animals.' A direct contradiction is the best answer to this preposterous claim, experiments with drugs and animals having not only been futile, but also misleading. Dr. Rutherford was misled by his experiments on animals so far as to declare, contrary to universal experience, that calomel diminished the secretion of bile. The most contradictory opinions have been put forward as to the action of drugs on animals, showing that the experiments were quite useless, and only calculated to cause error and confusion"?—Well, if a man says that the moon is made of green cheese it is very hard to convince him that it is not, if a man makes a statement like that.

7137. You would say that that is too general a statement for you to deal with?—Yes, it is too general a statement to deal with. That is a statement made in pure absolute ignorance.

7138. Then I will pass over various cases which I think more or less surgical, and his next heading is myxedema. He says: "It has been stated that a cure for this disease was discovered through vivisection; but this is not the case. Dr. Murray, of Newcastle, discovered, though not by vivisection, that an injection of the extract of the thyroid gland of a sheep into myxedemic patients would improve their health and avert death, but the treatment must be persisted in regularly, or relapses occur." Do you agree with that or did Dr. Murray experiment on living animals in fact?—It is said that a partial truth is worse than a lie. Sir James Thornton is perfectly correct about Dr. Murray, but he should go a step or two further back, and ask how Dr. Murray came to use this treatment. If he does that he will find that the whole treatment that Dr. Murray adopted was founded upon experiments on animals.

7139. He then speaks of nitrite of amyl. I think you have also mentioned that?—Yes.

7140. He says: "The claim that nitrite of amyl was discovered and brought into use through vivisection is quite unfounded. This substance was discovered in 1844 by the French chemist Balard, and was introduced to the medical profession in 1865 by the late Sir B. W. Richardson for the relief of angina pectoris, the action and properties of the drug having been by that time well ascertained. Some years later certain experiments on animals were performed by Mr. Gamgee with this drug, but nothing was discovered that was not known before." What is your comment upon that passage?—First of all he has got his dates wrong. It was not introduced as a remedy for angina pectoris by Sir B. W. Richardson in 1865. It was recommended by him in 1864 for syncope and for tetanus, but he never did come to discover its use in angina pectoris. The experiments upon animals performed by Gamgee were performed in 1866, and showed the action of the drug in lowering the blood pressure, a fact which had not been ascertained before; and it was this fact which led to the use of the drug in angina pectoris.

7141. As you have explained to us in an answer in a previous part of your evidence in the case of a St. Bartholomew's patient?—Yes.

7142. There is another remedy, is there not—nitroglycerin?—Yes. I made the physiological experiments with that in 1876, and I published them in the St. Bartholomew's Hospital Reports. I am unfortunately very liable to headache, and when I experimented with it it gave me such awful headaches that I hesitated to test it upon my patients, because I was afraid of giving them headaches such as I had got, but in the meantime a few months after I had done my experiments Dr. Murrell used it.

7143. Your experiments were on animals?—Yes. He introduced it, and it has proved perfectly satisfactory. Mr. Tate and I showed that the action was just the same as that of nitrite of amyl.

7144. But you give it in a different way, do you not, in which it is more convenient to give it?—Yes. It acts rather more slowly, and its action is more prolonged.

7145. How is it given?—It is usually given in little tablets mixed with chocolate.

7146. I need not trouble you perhaps with the details, as you have answered generally?—Here it is (*handing in a book*).

7147. Then Sir James goes on, I think, to deal with Pasteur's antirabic inoculations, and he contests the value of those inoculations. Have you any practical experience of the Pasteur treatment?—Yes. I was a member of the Commission sent across to investigate the utility of Pasteur's treatment; I went over perfectly convinced that Pasteur was wrong.

7148. What was the date of that Commission?—About 1884 or 1885, but I came away perfectly convinced that Pasteur was right, and there is no doubt whatever, I think, in the mind of any unprejudiced person that Pasteur's treatment has produced a complete revolution in the treatment of hydrophobia. Hydrophobia was a most frightful disease. Just before I went over I was a member of a Committee of the British Medical Association to investigate hydrophobia, and I went throughout all the country examining cases; and the cases I saw were simply heartbreaking. As the result of it I got inoculated, and had to burn it out.

7149. Your own finger got inoculated?—Yes, unfortunately, with rabid poison when making a post-mortem examination.

7150. What treatment did you pursue yourself?—Caustic potash; there was nothing else. I took a stick of caustic potash and rubbed it in, and scraped it out and rubbed it in again.

7151. Did you go through Pasteur's treatment?—No; it was before Pasteur's treatment; that was the only treatment available at the time, but it does not improve the fingers.

7152. You satisfied yourself on that Commission that Pasteur's treatment saves life?—Absolutely.

7153. Have you followed recently the results of the Pasteur treatment in the new laboratory at Kasauli in India?—No.

7154. A laboratory has been established there to enable Pasteur's treatment to be applied in India, where there is a great deal of rabies?—Yes.

7155. You cannot give us any information upon that?—No.

7156. Who could?—That laboratory is under Hankin, is it not?

7157. I do not know whom it is under. I did not know where we could get any information?—You could easily get it from the India Office. Sir Benjamin Franklin could possibly provide you wth all you want.

7158. Then Sir James Thornton speaks very strongly in favour of Buisson's treatment by baths; was that in any way investigated by your Commission?—No, we did not deal with that.

7159. Have you any opinion about it?—It is a very old treatment, but, like other old treatments, it was quite unsatisfactory.

7160. You think experience has proved it to be unsatisfactory?—Yes. It was tried, of course, without any definite results.

7161. Has it been tried with reference to animals as well as man?—I do not think so, but I cannot be sure.

But you could get information about it, I daresay, from Dr. C. J. Martin, of the Lister Institute.

7162. Then the next heading is Poison Tests, and Sir James says: "Poisons act in such different ways on men and animals that deductions from experiments on the latter are fallacious and misleading. Rabbits can eat belladonna with impunity; dogs, aloes; apes, strychnin; and goats, hemlock; while birds in general are little affected by morphia. Even in human beings the effects of certain poisons are greatly modified by race. All this tends to show how vain a thing it is to expect to find out remedies for our own diseases by experiments upon animals, which are not constituted as we are, and which frequently find their food in things which would be fatal to mankind." How far do you agree with that general statement?—A number of the instances are quite correct. The general conclusion is quite erroneous. For example, rabbits eat belladonna with impunity, just in the same way (as I mentioned before) as, if you cut the vagi in a rabbit, you find no change in the pulse rate. If you cut the vagi in a dog you see its pulse rate going up at once. If you give a dog belladonna, at once you will get the pulse rate go up too; the reason being that in a rabbit the vagi act ordinarily hardly at all, whereas in the dog they are constantly acting. When you paralyse the vagi in a rabbit with belladonna there is no difference. If you paralyse the vagi in a dog you get a great difference at once, just as you get a difference at once in a dog's pulse rate when you cut its vagi. When he says that birds in general are little affected by morphia, they are very much affected by morphia, but in a different way. For example, pigeons are not much affected apparently by morphia in the way of its causing drowsiness, but it acts upon them as an antipyretic—it lowers their temperature as much as if you put them in a very cold place—so that they are affected, though in a somewhat different way.

7163. But the point of Sir James Thornton's criticism is that these substances act so differently on animals and man that you can draw no true inference from one to the other. What is your comment upon that?—The same may be said of the action of the drugs upon different men. A dose of aloes, which he says may be swallowed by dogs with impunity, may be swallowed by certain men with perfect impunity; and yet we know that in the great majority of human beings aloes will act as a purgative, notwithstanding the failure to do so in those individuals. The same with opium. Sir Robert Christison used to tell us of a particular family who were not affected by opium. Sir William Hamilton, the Scottish metaphysician, would swallow an ounce of laudanum with perfect impunity—it had no action at all upon him; but it was not that he was an opium-eater or accustomed to it.

7164. It was a pure idiosyncrasy?—Yes; and it was inherited by his eldest son. But still the majority of people are affected by laudanum; so that what holds good in all he says in regard to animals applies in the same degree to man: that we can only tell from observation on man the effect of drugs upon the majority; that we will find exceptions just as we find them in animals.

7165. And by experiments is it possible to make the necessary allowance?—Yes.

7166. (*Chairman.*) I do not quite see how that is in the instance you gave. If pigeons generally are made cold by taking morphia, and it has not that effect on humanity, does not that show that you cannot experiment on pigeons with regard to the effect of morphia as an anæsthetic or soporific?—No, because you see I have dealt with that very question in this paper on the effect of heat and cold, upon the action of aconite on pigeons.

7167. I am assuming that what you say about it is quite correct as a fact. But after knowing what the effect of it was on pigeons, you would not experiment on pigeons with morphia with a view to obtaining information for the treatment of man, would you?—Yes, certainly; and for this reason, that there are differences in men. There are certain people who are called gouty who tend to pass out their nitrogenous waste in the form of urea or uric acid. Other people tend to pass out their nitrogenous waste in the form of urea, and what Dr. Cash and I were very anxious to find out was whether we could trace any relationship between the action of morphin on pigeons, which, like other birds, passed their nitrogenous waste out in the form of urea or uric acid and gouty people; because there are certain classes of people on whom morphia or opium does not act as a satisfactory soporific; instead of causing sleep it causes excitement. We thought that very likely by experimenting on animals we should find out whether this bears any relationship to the mode of excretion of nitrogenous waste. We find, for example, that morphia will not act upon pigeons and other animals that excrete uric acid, and we thought perhaps that we might find by going to work at the bedside, that men who excrete uric acid would not be satisfactory subjects for morphin. We were not able to continue the experiments, but that was the line of them; and if they are continued on those lines I think we shall get very valuable information indeed in regard to the action of drugs on account of the very variations in their action upon different animals, which will enable us to isolate the different factors which determine the kind of action of a drug upon man, by finding out one factor or another on the different animals upon which we experiment.

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7168. (*Sir Mackenzie Chalmers.*) The next heading is, perhaps, too wide a one to discuss at this time; it is "Preventive Inoculation," and that raises too wide a question to go into?—It is not one to which I have paid particular attention.

7169. Then Sir James Thornton discusses Snake Venom, and states that Sir Joseph Fayrer in 1904 pointed out the effect of permanganate of potash?—Yes, but it was before then; it was long before that. He mentioned it first of all in his book, "Thanatophidia of India."

7170. I presume it is, at any rate, from experiments on animals that you can test the proper mode of administering a drug like permanganate of potash?—Yes.

7171. You can only test the best way of dealing with snake bite by inoculating an animal efficiently, and then noting the different conditions under which the drug operates successfully or unsuccessfully?—Yes, that is so.

7172. There is an enormous loss of animal life as well as of human life in India through snake bite, as you know?—I believe they are beginning to use those little lancets for animals as well.

7173. That is an improvement that has been made in recent years as the mode of administering the antidote?—Yes.

7174. Then he takes the heading "Rheumatism," and he says: "Lactic acid is found in the bodies of rheumatic patients, and its injection into animals produces rheumatic symptoms; but this information has not resulted in the discovery of any means of preventing or curing the disease." That I imagine is true?—I think that is true.

7175. Have you anything to say for those experiments? Are they likely to be useful or useless?—I do not know that they are any use as yet, although one does not know what the result may be. As I said before, in all those things we have to accumulate a good deal of information before we get any practical application.

7176. I have only one more point to put to you. Under the heading "Temperature" Sir James Thornton says: "No alteration in the treatment of fever has resulted from the experiments of Claude Bernard on raising the temperature of animals till death occurred. These experiments therefore were totally useless." What is your comment upon that?—That is not so. I think perhaps the best comment I can make, if you will allow me, is to give you a case that I once saw. When I was a student, a man with pneumonia came into the hospital in Edinburgh, and was dying. Professor Bennett, who was a clinical physician, was very much disgusted because this was going to spoil his average. He said that he had had over a hundred cases of acute pneumonia, and he had never once lost a case. However, this man was really dying; he was moribund, so Professor Bennett gave a grunt and passed on. A Swiss named Skolberg, who was visiting the clinique, asked what did Professor Bennett think about the man. "He is going to die." "May I treat him?" "Yes, you can do what you like with him." So Skolberg pulled off all the bedclothes; he got a sheet, dipped it in a tub of water, and wrapped it round the man, pulled it off again in five minutes, and then repeated the dose. I went away, but about half an hour or three-quarters of an hour afterwards I returned, expecting to find the bed empty. To my

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astonishment, I found the man lying there, apparently perfectly well and sleeping comfortably. That man was dying of hyperpyrexia; but that was just before the introduction of the clinical thermometer, and we did not know what he was dying of. It was a bit of pure empiricism. But the experiments on animal temperature now allow any student to know what is happening and to know the time when he is to apply the cold douche and so to save the lives, not of one, but of many. What was then a mere accident is now the routine practice.

7177. You have explained the reason by experiment?—Yes.

7178. And therefore you can apply the treatment intelligently rather than empirically?—Yes. Of course, the effect of the treatment is 1,800 years old. Musa, the physician of Augustus, saved the Emperor's life by cold baths, but, not knowing how to apply them, he killed the Emperor's nephew, the next patient he had to treat.

7179. (*Dr. Gaskell.*) I just want to ask you one question with reference to a question put to you by Sir William Church. We have been told that there is a difficulty about anæsthetising, dogs especially, because the anæsthetic is so liable to cause death, and that the margin between death and complete anæsthesia is so small that it is impossible to keep the animal under complete anæsthesia. Do you consider that dogs are more liable than other animals to have stoppage of the heart through vagus action in consequence of the administration of chloroform?—No, I do not think that they are.

7180. You do not think that dangerous symptoms come in in the case of dogs early in the anæsthesia?—No, I do not think so. I think that there is very little danger with dogs, if you are watching what you are doing. All the time we were in Hyderabad the only fatalities we had were when somebody was not attending to his work. All the others went quite smoothly; we had no difficulty whatever.

7181. Is it not frequently the case that when you give chloroform to dogs you have what you speak of as vagus pulse?—Yes; but I do not think that that makes any difference. It does not kill the dog in the least. And you get it in man too.

7182. You do not think, then, that there is any more difficulty in keeping a dog under chloroform or that it is any more liable to death than in the case of cats or other animals?—Certainly not; and less than in the case of rabbits. I should put the dog upon very nearly the same plane as a man; I should say that the difficulties with a dog and man are just about the same. But with a rabbit, if you do not do it rightly, you do get death. There are two entirely different ways of chloroforming rabbits. One is to do it very gently, with a bell jar, and allowing the chloroform to increase very gradually until the animal becomes insensible. The other way is simply to cram the chloroform on to a cloth and stick it over the rabbit's nose and keep it there until it goes under. Either way is quite safe; but a combination of the two is unsafe. If you begin gently, and then cram it on, and then do it gently again, it does not do.

7183. You have had experience of chloroform given to a vast number of dogs?—Yes.

7184. And you have entirely convinced yourself that they have been under anæsthesia?—Yes, entirely.

7185. And complete anæsthesia?—Complete anæsthesia; as complete as any patient could possibly be under on the operating table.

SEVENTEENTH DAY.

Wednesday, 1st May 1907.

PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Col. the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MACFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Miss L. LIND-AF-HAGEBY, called in; and Examined.

Miss L. Lind-af-Hageby.
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7186. (*Chairman.*) You are a native of Sweden, I believe?—Yes.

7187. And you resided there and were educated there, until you came over to England for the purposes of attending lectures?—Yes; and I have been living in England a great deal. I was at school in England, but I have been living here since 1902—practically residing here.

7188. Before you left Sweden, I understand that you took a great interest in this question of vivisection?—I did, a very great interest since 1900.

7189. And you took an active part in the agitation against vivisection?—I have taken an active part in the agitation for some seven years.

7190. Since 1900, you mean?—Yes.

7191. Did you receive any medical education in Sweden?—No, I did not. The medical education I received I received here; but it was never our intention to take up the study of medicine—it was simply to study physiology.

7192. For the purpose of assisting your action on the question of vivisection?—Yes.

7193. You were a confirmed anti-vivisectionist when you came over to England?—We were.

7194. You and Miss Schartau were joint authors of a book called "Shambles of Science"?—Yes. Although we were confirmed anti-vivisectionists in a way, we were quite open to conviction, and were anxious to find out what vivisection really was and to get more knowledge. We found that we had not sufficient knowledge to fight against it as we were doing, and it was out of a desire to get more knowledge and get more thoroughly acquainted with it that we decided to take up the study of physiology.

7195. You had had no previous experience of physiology or of attending lectures?—No, but it was a visit to the Pasteur Institute in Paris that made us interested in the whole question; it was the things we saw there which made us really care for the question.

7196. The principal thing we wish to have your evidence upon is the cases that you refer to in your book, in which it is alleged or suggested that there were breaches of the law and cruelty; and perhaps it would be more convenient to take them just in the order in which you refer to them in the book?—May I be allowed to read a statement which I have prepared? It is very much in connection with the book and very much in connection with my whole point of view.

7197. I am afraid we should have things twice over

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in that case. It would result in our hearing a good deal twice over, because it would be necessary to ask you as to the precise facts you there speak to on each of those experiments. I propose now to go through them, and if, when I have done, that there is anything you wish to add, you will have an opportunity?—In my statement I have prepared certain information in connection with those experiments which have been already brought before the Commission; but before that I have some evidence which to me it is absolutely necessary to give in connection with "Shambles of Science," and in connection with statements that have been made already before the Commission; and I therefore shall be very grateful if I may be allowed to read it, as to me it is very closely connected.

7198. We find that when witnesses read statements they very often have not got present to their minds what the object of this Inquiry is so much as what their own views are, and we find that those statements very often go into matters which are not really relevant to our Inquiry?—I am afraid that my evidence will not be of the same use if I am not allowed to read my statement, as other witnesses have been allowed; for instance, Professor Starling, Mrs. Cook, and others have been allowed to make statements; and I have prepared my statement very carefully. All my statement is composed of corroborative facts which to me are absolutely essential if I am to present my defence of "Shambles of Science" from the point of view of the accusations which have been made; and I am afraid if I am not allowed to present it in this way, I must beg leave to withdraw as a witness. I do not feel justified, in the interests of the cause that I represent, in defending my book in a way that would not do justice to the cause.

7199. You must remember that it is not your book that is on trial here; it is the experiments that you speak to. You say that it is a statement of facts that you wish to read?—I wish specially to speak to those facts in immediate connection with my book; but also I understand, if the Commission wish to hear my observations, that I have the same right to speak on the general question as Mrs. Cook has done, and Dr. Arabella Kenealy, and other witnesses who have come from the anti-vivisection side.

7200. Yes; I am not disputing that you are at liberty to speak on every possible relevant question?—I do not intend to do that. I have limited myself to physiology. I should infinitely prefer to be allowed to read a statement, as other witnesses have done.

7201. You have sent us a *précis* of your evidence?—Yes. May I be allowed to say that I have endeavoured to give in my *précis* the very same points as I have in my statement.

7202. I was going to follow your *précis*, in which you begin by stating that you wish to give evidence especially in connection with your book "Shambles of Science" and passages contained therein, which the Commission have already dealt with, and the experiments on the so-called "brown dog" and other facts bearing upon the basis and progress of the anti-vivisection cause?—Yes, but if you read the next point in my *précis*, there are certain things I wish to bring out before I come to that.

7203. I was going to follow on with that?—I have a short statement on the ethical side of the question, and I thought I should be allowed to read that first.

7204. Very well; will you give us that?—The "Shambles of Science" was inspired by moral indignation, and written as a protest against the cruelties of vivisection, the callousness of vivisectors, and the demoralising effect of vivisectional demonstrations before students. Before I proceed to lay before the Commission facts which corroborate the accusations made in the book, I wish to plead the priority and the ultimate persistence of the moral objection and revolt against experiments upon living animals. There can be no doubt that the vivisection of human beings would at present be of the utmost scientific value to the same section of scientists who claim the right to vivisect animals, but the majority of vivisectors shrink from expressing such desires, though there are exceptions. We have advanced so far in our moral conceptions that a general clamour for human beings for vivisectional purposes would meet with disapproval and resentment. The question of utility or uselessness is here rightly deemed to be immaterial in comparison with the moral issue, and in the same way a further moral development will prohibit the vivisection of animals. The history of the evolution of morals affords repeated proofs of such moral victories. The use of instruments of torture for the purpose of extracting especially coveted information has been abandoned in most civilised countries. Religious as well as scientific torture of human beings is condemned as immoral. There can be no doubt that, were the arguments of the vivisector to be considered a safe moral guidance in other walks of life, the artist and the sculptor might crucify in order to further realism in their art, and the writer of drama might order the infliction of pain and suffering on the helpless for a similar purpose. Few would deny that humanity is as much in need of art as of science, and yet divorced from morality they both become scourges instead of blessings. From the purely physical point of view it might be advantageous to kill off a few thousands of our incurable lunatics, imbecile children, incorrigible paupers, worst criminals. But we refrain from acting in accordance with such a view because we are restrained by a moral consciousness which was non-existent in our savage state. Moral evolution has forced us to include the sub-human races in the circle of our compassion and our sympathy. Kindness to animals, gradually passing into a conscious acknowledgment of their rights and our duties towards them, is the last and finest product of our developing altruism. From the moral point of view the elimination of the unfit means the elimination of the cruel and the selfish. In the light of progressive morals anti-vivisection is not an isolated fad or the expression of uncurbed sentiment—it is part of a great whole, of a humanitarianism which includes every sentient creature in its protective embrace, and which condemns every form of cruelty to man or beast. The abolition of vivisection, then, becomes an unavoidable moral necessity of the future; for the growth of sympathy and the demand for justice cannot permanently be stopped by any appeals to our cowardly fears or to purely egotistic considerations.

I now come to my evidence in immediate connection with the "Shambles of Science," and everything I am going to say now is closely bound up with the book. I have brought some copies in case anybody would like them.

7205. Now you are going to speak with reference to the experiments which you saw and your description of them?—Yes. I am going to try to give my own experience of vivisectional demonstrations and vivisectional anæsthesia, as well as certain evidence in corroboration of that experience.

7206. Do you mean other experiments which are not mentioned in your book?—No, I shall not speak to any experiments except those I have seen—not generally, of course. I have some corroboration, of course. I am afraid I do not quite understand the question.

7207. I thought from what you said that you were proposing to give evidence of breaches of the law in experiments in operating rooms and laboratories on other occasions than those referred to in your book?—No.

7208. This surely would be more convenient to take by question and answer. We are now coming to the first of them?—Yes, I am now coming, if I am allowed, to what experiments are "absolutely necessary," if I may read a very short statement. I have not taken it from the point of view of going through the whole book chapter by chapter; I should never think the Commission would have time to listen to it if I were to give lectures on each chapter; and I have simply taken certain facts.

7209. What we want to get at is the facts. When you speak of "painless experiments" you give what took place on a certain occasion in a lecture room with reference to a frog?—Yes.

7210. What really took place is what we want to know?—Yes.

7211. Because you know very well that some of the physiologists differ from you?—Yes, I do.

7212. And some who were present on the occasion differ from you and give different accounts?—Yes.

7213. Then in order to get at the facts we must pursue the ordinary method of doing it—that is, to ascertain what your statement of the precise facts is to which the other witnesses speak?—If the Commission would perhaps find it possible to follow the *précis*, the next point is that the "Shambles of Science" throws light upon the manner in which the provision of the Act of 1876, that only experiments which are "absolutely necessary for the due instruction of the persons to whom such lectures are given" is interpreted. That

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is a point which runs through the whole book rather than one single chapter.

7214. You are speaking now of experiments for instruction and demonstration?—Yes, and I am very anxious to bring out a point here, that point being that the Act of 1876 demands that only experiments which are absolutely necessary shall be performed in illustration of lectures; the principle underlying this restriction evidently being that too much vivisectional demonstration before students would have a demoralising effect—at least it must be some notion like that which underlies that Section of the Act. I wish to bring out now that our experience as recorded in the "Shambles of Science" shows that a great many experiments are performed before students which cannot be justified as being "absolutely necessary for the due instruction of the persons" (those are the words of the Act) "to whom such lectures are given, with a view to their acquiring physiological knowledge or knowledge which will be useful to them for saving or prolonging life or alleviating suffering." As an instance, I have mentioned in my *précis* the injection of diseased spinal cord matter taken from a lunatic into a dog. I have mentioned that as an instance of an experiment that cannot very well be said to be absolutely necessary.

7215. That is on page 75 of the book?—That is the page.

7216. You want to prove from what you saw then that it was unnecessary?—Yes. I refer to that experiment especially as one that cannot be said to be one that was absolutely necessary; and my contention is that all the experiments are more or less unnecessary. May I proceed on this point?

7217. If you will tell us what you find in this chapter which supports your argument?—In this chapter my argument is supported by the fact that this experiment has no therapeutic value whatever; further, that it was not absolutely necessary to demonstrate it from the physiological point of view. I do not know whether I am to mention the names of the physiologists who performed these operations.

7218. Certainly?—I do not wish to mention their names with any desire to libel them. The Commission wish me to mention their names?

7219. Certainly?—I want then to bring out that Professor Halliburton, who conducted this demonstration, in his "Handbook of Physiology" refers to the test for cholin; and he says the cholin may be tested in two ways—by a purely chemical test and by a physiological test; but—these are not his words, but it is my opinion—as there are the two tests it was not absolutely necessary to choose the test of injecting it into a live animal; he might just as well have chosen the purely chemical test. Further, as I said, the experiment is of no therapeutic value whatever, and Professor Halliburton himself does not seem to assign great value to this experiment, for he says in his Handbook, on page 177: "It is possible that such tests may be of diagnostic value in the distinction between organic and so-called functional diseases of the nervous system."

7220. Was Professor Halliburton one of those engaged in this operation?—Professor Halliburton was the lecturer upon that occasion; and my contention here would be that when the lecturer himself does not know or is very doubtful as to whether there is any diagnostic value, even diagnostic value, in such an experiment, that particular experiment should not have been one which is tried before students. Further, I wish to state that the choice of what is absolutely necessary seems to depend upon the individual taste of the physiologists who are lecturing and demonstrating. Thus Dr. Bayliss, during the Bayliss *v.* Coleridge trial in 1903, referred to his particular experiment upon which the action was based as one that was "absolutely necessary"; whilst Professor Gotch, of Oxford, during the trial said that that particular experiment was not included amongst those which he showed to his students. That shows, in my opinion, that the choice of what is "absolutely necessary" depends on the individual taste of each vivisector, and that one might ask why there is not a certain curriculum, a certain number of experiments which are agreed upon as being "absolutely necessary" and performed before students, but that there is unlimited freedom in the choice of that which is "absolutely necessary."

7221. Are there any other of the experiments which you refer to in this book which bear upon that point of demonstrations?—My point—from my point of view—of course I do not expect physiologists to share it—is that none of those experiments can really be said to have been absolutely necessary for the due instruction of students.

7222. You select this one as what you consider a strong instance?—I select it as a strong instance; that is all. May I now go on to my second point in connection with the book?

7223. Yes?—The descriptions given in the book in connection with the subject of vivisectional anæsthesia and the lack of humaneness on the part of lecturers and demonstrators who, whilst they are anxious to impart knowledge to their pupils of the theories of experimental physiology, neglect altogether to inculcate any ideas of mercy or carefulness in the treatment of the animals, are corroborated by the tone and instructions which characterise most modern textbooks and handbooks on physiology, theoretical and practical. Vivisectors have lately laid claim to humaneness in the execution of their pursuits, and have disdainfully repudiated connection with the old vivisectors, who were cruel, or foreign vivisectors, who may be cruel now. Thus there was a letter in the "Times" of March 12th, 1902, by Sir Victor Horsley, in which he referred to foreign vivisectors in these words. He was speaking of a representative of the anti-vivisection agitation, saying that he "confuses what may be done in Germany in the direction of operating on animals without anæsthetics with what is actually done in the United Kingdom under anæsthetics, and terms it all vivisection." There are many other instances of similar statements, which I do not think I need read to the Commission. But the veracity of their claims is disproved by their own teachings. If everything were done to teach students the moral obligation of being merciful to the victims of scientific research these textbooks and handbooks would show some trace of it. They would contain special and serious warnings against the infliction of pain, and special and detailed instructions as to the care of animals from the humane point of view, both during the operative and the observational periods of experiment. The following well-known books contain no such warning or instructions. (I have brought all these books with me to-day.) "Directions for Class Work in Practical Physiology," by E. A. Schäfer, 1901; "Exercises in Practical Physiology," by Aug. D. Waller, Part II., 1905; "Essentials of Experimental Physiology, for the Use of Students," by T. G. Brodie, 1899; "Practical Physiology," by Beddard, Edkins, Hill, Macleod, and Pembrey, 1905; "Halliburton's Handbook of Physiology," 1905; "Elements of Human Physiology," by E. H. Starling, 1905; "Outlines of Practical Physiology," by William Stirling, 1902; "An Introduction to Physiology," by L. A. Hodgkinson Lack, 1905; "Essentials of Human Physiology," by D. Noël Paton, 1905; "Essentials of Physiology for Veterinary Students," by D. Noël Paton, 1905; "A Manual of Physiology," by G. N. Stewart, first edition 1895, fifth edition 1905; "Textbook of Physiology," edited by E. A. Schäfer (for advanced students), 1898. The following books are by four American professors of Physiology, but have English or Anglo-American publishers, and are widely known and studied in England:—"Textbook of Physiology," by W. S. Hall, 1900; "Handbook of Physiology for Students and Practitioners of Medicine," by Austin Flint, 1905; "A Textbook of Physiology for Medical Students and Physicians," by Will. S. Howell, 1905; "Laboratory Manual of Physiology," by Fred C. Busch, 1906. I have not said that directions as to the administration of anæsthetics are absent from all these books, but I wish to point out that, although some are less callous than others, on the whole they lack all signs of any dominating wish to attempt the humane teaching and practice of experimental physiology. I now come to my next point, and that is that incomplete anæsthesia is frequently advised in these textbooks for students. One of the most important lessons to be drawn from a study of these textbooks is the utter inadequacy and unreliability of vivisectional anæsthesia as a means of abolishing pain. "The sleep of anæsthesia is a pathological condition due to the action of a poison," says Professor Halliburton in his "Handbook of Physiology," and experience of vivisection as it is taught and as it is carried out shows that experiments frequently require partial or perfect freedom from that condition.

7224. From what condition?—From the condition of anæsthesia. Instead of warning students against cruelty, some textbooks deliberately advise light anæsthesia, and warn their readers against employing anæsthetics in a manner which would bring about deep anæsthesia.

7225. What was Professor Halliburton speaking in reference to in that passage?—Professor Halliburton was there merely speaking as a scientist, and not with any special reference to demonstrations. I simply quoted his words as being an authority, and saying that the sleep of anæsthesia is a pathological condition due to the action of a poison; and I brought it in here to show the likelihood of physiological experiments being frustrated and injured by anæsthesia.

7226. (*Colonel Lockwood.*) Have you got Professor Halliburton's book?—I have it here.

7227. (*Chairman.*) Would you just read the passage again?—"The sleep of anæsthesia is a pathological condition due to the action of a poison."

7228. (*Sir Mackenzie Chalmers.*) If you can give us the page we can see for ourselves?—I am afraid I have forgotten the page.

7229. (*Chairman.*) You only read part?—The rest was my own.

7230. What followed a little surprised me?—It was badly read by me. I have said that these physiological books for students, as I am going to show, frequently require perfect or partial freedom from that condition—the condition which Professor Halliburton refers to as a pathological one.

7231. We should like to see the passages, if necessary?—I have got all the passages marked.

7232. I do not want them read, but you will give us the references so that they may be examined, because so much depends upon the context?—In support of this I beg to draw the attention of the Commission to Professor Brodie's book "Essentials of Experimental Physiology for the use of Students." Professor Brodie was lecturer on physiology at the London School of Medicine at the time of my attendance there.

7233. (*Sir William Church.*) The London School of Medicine for Women?—The London School of Medicine for Women. In this book Professor Brodie expresses his preference for light anæsthesia and curare in the following terms, at page 176, dealing with stimulation of the sciatic nerve: "Often, too, struggling movements are produced, especially if the animal be only lightly under the anæsthetic, when with each convulsion the blood pressure rises considerably. The only completely satisfactory method of obtaining a pure pressor effect is to previously curarise the animal"; and at page 202, dealing with the superior laryngeal, he says: "We may stimulate this nerve in two ways. We may imitate the normal method by introducing into the larynx a curved probe whose end has been wrapped in cotton wool. In this instance, and, indeed, for all these nerve stimulations, the rabbit must not be too deeply under the anæsthetic."

7234. (*Chairman.*) Do you understand that to mean that he is advocating that the rabbit should still be sensible to pain?—I do not mean to suggest that Professor Brodie is advocating that, but I mean to suggest that vivisectional anæsthesia is exceedingly unreliable, as the deeper stages of surgical anæsthesia are not compatible with physiological research. On page 206, dealing with stimulation of the central end of the vagus, he says, "This result is much better obtained if the anæsthesia be not too deep." Professor Stewart's "Manual of Physiology" shows the usual fear of too deep anæsthesia, though the last edition is better in this respect than the first. In the practical exercises attached to the various chapters which give full directions for vivisectional work, students are advised only to give ether in addition to morphia or chloral "when necessary"; and on page 156 of the first edition, dealing with stimulation of the depressor nerve in the rabbit, we read: "Anæsthetise a rabbit with ½ gramme chloral hydrate, and, if necessary, with ether. In blood pressure experiments only small doses of chloral hydrate or chloroform can be given, as they affect the vasomotor centre." Dr. Waller gives a similar caution in his "Exercises in Practical Physiology" (Part II., pages 46 and 47).

7235. There, again, do you mean to suggest that in the doses he recommends he is recommending a dose which would not produce insensibility to pain?—I am not at the present moment dealing with doses.

7236. Well, quantities?—I am not at the present moment dealing with doses; in fact, I do not know that the doses are given in these cases.

7237. I am not speaking of this specific dose. But he says there should be a light anæsthesia?—Yes, I mean to suggest that all these (and there are one or two or more that I want to bring out on this point) imply the induction of an anæsthesia which from the humane point of view is unreliable because it is too light, and I am going to endeavour to prove by these facts that that is generally practised in physiological laboratories.

7238. That is the real point we know, whether it is generally practised; and this is really a little outside our inquiry, as to what doctors write in books and recommend. It is what is done that we are concerned with, because they may not be recommending it but saying that certain effects will not be produced under heavy anæsthesia?—I do not think it is outside your inquiry when in the "Shambles of Science" I have described what I have seen done. These are the theories which would be responsible for the doings I have seen. It is corroboration.

7239. We might have to go into the education of every doctor who performed an operation, at that rate. We are not so much concerned as to what doctors have written on the question, and what the effect of light or heavy anæsthesia would be upon animals, as upon what is done?—But if I were to come before the Commission and simply say I have seen this, and I have seen that, I have every reason to believe that the Commission naturally would not look upon me as an authority on the subject, but would probably take the opinions of physiologists, who hold entirely different opinions, as more valuable; but if, at the same time as I bring my own experience, I corroborate that experience by theories as they are put down in every current and modern text book, I consider that I strengthen my case considerably.

7240. (*Colonel Lockwood.*) You are arguing that the anæsthesia as recommended by the authorities you quote is so light as not to render the animal insensible to pain?—I mean that it is very possible that the animal may not be insensible to pain.

7241. (*Sir William Collins.*) Are you quoting from the handbooks of physiological laboratories?—I am quoting from handbooks all of which I have brought with me.

7242. (*Sir John MacFadyean.*) Would you admit that there is any point in the argument if the authors of these books tell us that when they said the anæsthesia was not to be too deep they never for a moment intended it to be so light that the animal was to be conscious?—I quite admit that the defenders of vivisection would say that, but my point is that vivisectional anæsthesia is unreliable, because the scientific requirements do not tally with the humane requirements, and because these scientists would always rather follow the scientific requirements than the humane ones.

7243. May I point out that the train of your argument really seems to involve a belief on your part that they intended to suggest that the anæsthesia should be so slight that the animals might be conscious; because it arose in connection with a question of cruelty?—My whole evidence is in connection with cruelty.

7244. So that you do wish us to believe that these authors deliberately intended to advise that in certain cases the anæsthesia should be so slight that the animal would probably be conscious?—I have never said that or suggested it. I have said that they advise light anæsthesia, and I have not suggested anything as to what their views are on the animal feeling pain or not; in fact, I think in most cases they hold no views on that subject when they write these books. But I mean to suggest from the anti-vivisectionist point of view that it is very likely that the animal is insufficiently and improperly anæsthetised, and therefore we anti-vivisectionists have a very strong reason to complain.

7245. (*Colonel Lockwood.*) Why do you say it is "very likely"?—Because I know, and I think everybody knows something, of the difficulties of anæsthetising generally, what a science it is, what an art it is; and from my experience of vivisectional anæsthesia I know how that is practised, too. And if the dominating idea is to have a light anæsthesia, the dominating idea cannot be to have profound anæsthesia; and we know that anæsthesia is always fluctuating between deep and

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light, and that therefore the tendency, the weight of balance would be towards the light, towards the return of consciousness, rather than towards the total and permanent abolition of consciousness.

7246. (*Chairman.*) It all depends upon whether those are really instructions which would be read by any medical student as instructions for the purpose of allowing animals to be too lightly anæsthetised, so that they still should feel pain—whether that is the intention?—May I read again Professor Brodie's words: "This result is much better obtained if the anæsthesia be not too deep." This book is written for students.

7247. "Is not too deep"; but yet it may have to be deep enough to prevent pain being felt by the animal?—Professor Brodie would no doubt say that; but the whole fight over this question is on the different points of view. There are different opinions, and I present one side of the case.

7248. But you are citing a book and suggesting that it has a certain meaning, and then you say that therefore it is advocating to a certain extent cruelty—it is encouraging cruelty?—No; I say that it may lead to cruelty.

7249. (*Dr. Gaskell.*) Do not you know that in this kind of experiment of which you have quoted there is a difference in the result according to the amount of anæsthetic given?—That is just what I am suggesting.

7250. That it is not a difference of pain; it is a difference in the pressor or depressor effect, and also in the action of the nerves on the vasomotor centres?—Yes?

7251. And if there was any pain, the experiment could not be carried out at all?—I do not think that is so. I shall bring further evidence to show that experiments can be carried out under pain. Dr. Waller gives a similar caution in his "Exercises in Practical Physiology" (Part II., pages 46 and 47) in these words: "Stimulation of the Central End of the Lingual nerve. The lingual nerve is ligatured near its entrance to the mouth, and so stimulated. Unless the animal is too deeply anæsthetised, salivation is accelerated"—in other words, that is another caution that too deep anæsthesia would interfere with the experiment. In Professor Halliburton's Handbook there is another caution in these words: "It is essential, when the experiment of stimulating the cortex of the brain is being performed, that the animal should be anæsthetised, otherwise voluntary or reflex actions will occur which mask those produced by stimulation. If, however, the animal is too deeply under the influence of a narcotic, the brain is inexcitable." This statement of Professor Halliburton is supported by many similar expressions in the literature of vivisection. Thus Dr. David Ferrier, in his book "The Functions of the Brain" (2nd edition, 1886) says, in speaking of electrical stimulation of the brain, at page 224: "That which will cause intense and indefinite action in an animal non-narcotised will excite only moderate and definite action in an animal sufficiently narcotised to abolish all sense of pain, and no effect at all on an animal deeply anæsthetised." And on page 232: "Deep narcosis, however, abolishes the excitability both of the grey matter and the subjacent medullary fibres, so that no reaction whatever occurs in the application of currents otherwise effective." On page 166 in Professor Busch's "Laboratory Manual of Physiology," dealing with the subject of excretion, students get the following advice: "Narcotise a rabbit, lightly, with morphin. Anæsthetise with ether just sufficiently to keep the animal quiet." There is no attempt to introduce any moral consideration into these words—"just sufficiently to keep the animal quiet."

7252. (*Chairman.*) Is that your observation?—That is my observation—but instructions are then given for the utilisation of this animal for a number of other experiments. The Commission do not probably want to hear about them, because this is an American book; but it has an English publisher and is very well known in England. I have only a few more remarks about it.

7253. I think you might confine your evidence to English books. You have read a number of passages, and the real question is, What is the fair meaning of those passages? I do not think you add anything by going to American books. We may take it that a great number of books do contain expressions such as you have quoted. The question is really what is the fair meaning of them?—My next point is that there are cases where even no anæsthesia is spoken of as a condition that is quite likely to occur to be necessary. It is certainly not only light anæsthesia that is advocated in these textbooks; sometimes they calmly describe the course of experimental results in unanæsthetised animals, or point out that better results are obtained if no anæsthetics are employed. The following instances illustrate this. From "A Manual of Physiology," by G. N. Stewart (5th edition, pages 137-138)——

7254. Is that a modern book?—All the books are modern. The 5th edition was published in 1906.

7255. By "modern" I really meant after the Act of 1876?—Yes, all my books, I should say, are within the last 10 years. He is dealing with the depressor nerve in the rabbit, and he says: "Excitation of its central end causes a marked fall of blood pressure, accompanied by, but not essentially due to, a distinct slowing of the heart. If the animal is not anæsthetised there may be signs of pain, and for this reason the depressor has sometimes been spoken of, somewhat loosely, as the sensory nerve of the heart."

7256. What do you suggest is the suggestion made to students there?—The suggestion made to students is that experiments have been performed on animals that are not anæsthetised. "If the animal is not anæsthetised there may be signs of pain" when the depressor is stimulated; and my suggestion here is that experiments on unanæsthetised animals productive of pain do not seem to be an impossibility to teaching vivisectors—nor do they withhold information.

7257. But do you really infer from that that Mr. Stewart is practically admitting that since the Act of 1876 licensed persons have performed these operations without anæsthetics?—I do not mean to infer anything more than what is said.

7258. But does it say that?—I do not think that writers on physiology have the Act of 1876 in front of their eyes when they are writing these textbooks for students. I think they are purely considering the scientific aspect of the matter.

7259. But when you are asking us to draw such strong inferences against a writer, is it not quite consistent that he is describing the result of observations that he has made when he has seen such operations performed in France or Germany?—I cannot possibly say what Professor Stewart meant to infer, but I shall come later on to similar statements by Professor Starling about the effect of pain, and I only quote them as being material to my side of the case; I cannot draw any inferences.

7260. I only suggest that if they only go so far as that, they do not seem to me to assist you at all?—I am afraid I must leave it to the Commission to draw inferences. I am not here to do that.

7261. (*Sir John MacFadyean.*) But you read this evidence to us with a special object, which is, to get us to draw certain conclusions?—Certainly.

7262. Do you wish us to conclude from it that the writers have actually performed, or that any other licensed person has performed painful experiments without anæsthetics?—I wish the Commission simply to draw the conclusion, first, that experiments are desirable under light anæsthesia, which I have already shown; and, secondly, that there are cases where no anæsthesia is scientifically preferable; and without committing myself to saying what was done here or there, or anywhere where I do not know, or suggesting anything personal or definite in connection with breaches of the Act. I only bring this scientific wish and scientific desire and scientific necessity before the Commission, and I must leave it to the Commission to draw their own inferences. Professor Flint says, in his "Handbook of Physiology for Students and Practitioners of Medicine," page 373: "If the abdomen of a dog, perfectly quiet and not under the influence of an anæsthetic, is opened, and a portion of the liver excised, rinsed in cold water and rapidly cut up into boiling water, the extract will show no reaction with Fehling's test for sugar." From the same book, page 374: "A notable experiment on the influence of the nervous system on the liver is the one in which artificial diabetes is produced by stimulation of the floor of the fourth ventricle (Bernard). The operation is not difficult. The instrument used is a delicate 'stilet' with a flat cutting extremity and a small projecting point about $\frac{1}{25}$ of an inch (one millimetre) long. In performing the operation on a rabbit, the head of the animal is firmly held in the left hand, and the skull is penetrated in the median line, just behind the superior occipital.

protuberance. This can easily be done by a few lateral movements of the instrument. Once within the cranium, the instrument is passed obliquely downward and forward, so as to cross an imaginary line drawn between the two auditory canals until its point reaches the basilar process of the occipital bone. The point then penetrates the medulla oblongata between the roots of the auditory nerves and the pneumogastrics, and its projection serves to protect the nervous centre from more serious injury from the cutting edge. The instrument is then carefully withdrawn and the operation is completed. This experiment is almost painless, and it is not desirable to administer an anæsthetic, as this in itself would disturb the glycogenic process."

7263. (*Dr. Gaskell.*) Would you mind mentioning to the Commission that Professor Flint is an American?—Professor Flint is an American, but this experiment is one that is frequently referred to in all textbooks on physiology, and I have here an English book describing the same experiment—it is "Recent Advances in Physiology and Bio-Chemistry," edited by Leonard Hill (page 340)*—which likewise describes those experiments, also without mentioning any anæsthetic.

7264. (*Chairman.*) What Professor Flint, I understand, said was that it was better to perform it without any anæsthetic?—Professor Flint distinctly says that anæsthetics must be avoided in experiments on the glycogenic function.

7265. Then it is very material to know that he was an American, because his book was written in America (and I suppose originally for Americans) where there is no vivisection law I understand in most of the States; therefore it is a very different case from that of England?—I wish to bring out, too, that an English book, "Recent Advances in Physiology and Bio-Chemistry," edited by Leonard Hill, describes the same experiment, also without mentioning the use of anæsthetics.

7266. But Professor Flint does mention the use of anæsthetics. Does the English author use the same phrase about anæsthetics?—No; he says nothing about it at all.

7267. But that is a most material difference?—I quite appreciate the difference, and I quite appreciate, too, why the English author says nothing about it.

7268. You think that by not mentioning the use of anæsthetics in a country where the law is that anæsthetics must be used, the author is implying that they ought not to be used?—I should think that as a scientist, as a physiologist, Dr. Hill knows quite well, what all physiologists know, that these particular experiments have to be performed without anæsthetics. I cannot form any opinion as to what he means by not saying anything about them.

7269. Are you citing this for the purpose of bringing home to our minds the action taken by those who advise the students in England?—Yes.

7270. I want to know whether you are reading that passage to show us that they refrain from advocating the use of anæsthetics, and therefore encourage illegal operations by such a passage as you have just read to us in an English author's book?

7271. (*Sir Mackenzie Chalmers.*) You know that that experiment would be illegal in England?—Yes. I am not at present dealing at all with how the law is carried out or administered.

7272. (*Chairman.*) It is the interpretation of what the author means that we are dealing with; we have to interpret his meaning, and one wants a great deal more than that. Do you mean to suggest that he is inculcating the illegal performance of operations in his book?

7273. (*Sir William Collins.*) Does Dr. Hill say that glycogenic experiments can be successfully performed in England without anæsthetics?—No, he does not say that, but he would know as a physiologist that it would be impossible from a purely physiological point of view. I now come to "Practical Physiology," by Beddard, Edkins, Hill, etc.

7274. (*Sir Mackenzie Chalmers.*) What Dr. Hill is that?—The same Dr. Leonard Hill. On page 156 we read, "The gastric secretion is obtained by making an incision in the stomach, as in Figure 138, and then reflecting and suturing the mucous membrane of the stomach so as to make a separate secreting sac which is still in muscular and nervous continuity with the rest of the stomach. The mouth of the sac is sewn to the opening in the abdominal wall. The vagus is exposed and divided." I must say in fairness, that there is a note in Mr. Beddard's book saying "This experiment cannot be demonstrated." That note, I should think, evidently refers to the English law; but these are the words: "Three days later the peripheral end of the vagus is excited in the unanæsthetised animal, and the juice collected. Anæsthesia or operative procedures easily inhibit the gastric secretion."

7275. (*Chairman.*) Is Dr. Leonard Hill an Englishman or an American?—An Englishman. This is an English book, "Practical Physiology." This experiment is one typical of the methods of the Russian physiologist, Professor J. P. Pawlow, Director of the Physiological Department of the Institute for Experimental Medicine, St. Petersburg.

7276. (*Colonel Lockwood.*) But it does not refer to experiments done in England?—Yes, I am coming to that. Professor Pawlow has been much quoted lately by physiologists as a vivisector who is able to work under "perfect physiological conditions"—that is, no pain and no anæsthesia. Professor Starling has described those conditions as "an ideal" which has been attained by Pawlow; and in his admiration for these methods he used the following words in his evidence (Question 4119): "You get an animal and you alter its anatomy in some way or other so as to make some process accessible to simple observation, so that you can see what goes on without interfering in any way with the animal's comfort or with the animal's normal condition." In the Harveian oration for 1902, Dr. Ferrier refers to these methods as "novel and luminous." And there are many expressions of admiration for his methods in the literature of physiology. But a direct reference to Professor Pawlow's lectures published in England under the title "The Work of the Digestive Glands," shows that his methods are far from "ideal" or perfect from the physiological point of view, and that even those vivisectional methods speedily become pathological instead of physiological, and that disease and suffering are by no means absent. On page 183 Professor Pawlow says:—"I had by no means the intention at first to set up conditions of disease; I operated solely for physiological purposes, and kept my animals alive for months or years. How many and how profound pathological processes have under these circumstances come into existence before my eyes! I have seen in connection with a disturbance of the functions of the liver an enormous ascites develop, at another time an ascending paralysis of the central nervous system, or, again, a general lacerability of the blood-vessels, and so on." On page 171 he speaks of a dog which got an ulcer in the artificial gastric cul-de-sac, which gave rise to violent bleeding and produced a secondary peritonitis. And in describing pancreatic fistulæ he tells us (on page 7) that only one in every four or five dogs will be found which tolerates the operation without any nursing, meaning thereby that it is a difficult operation, which takes a great deal of nursing, or the animal will die. I suppose that is what is referred to. I have brought this before the Commission, although it may seem irrelevant, because Professor Starling, in quoting Pawlow's experiments, has at the same time brought forward a plea for more experiments in the future under Certificate B, that is, the certicate which allows the animal to recover from the anæsthesia and to be kept alive, and under which several operations are at present performed legally on one and the same animal. But Pawlow shows clearly himself that he has not attained any ideal conditions by any means. He uses the very words here in contradistinction to Professor Starling. Professor Pawlow himself says: "Our solution of the problem"—that is of these fistulæ—"is evidently by no means an ideal one" (page 8), and he tells in this book which I have brought with me how these fistulæ—pancreatic and other fistulæ—frequently produce corrosion and various skin troubles, which must give a great deal of discomfort and pain to animals. He tells a story of one of the dogs in his laboratory, and I am relating this because these are the vivisectional methods of the

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* *The chapter on "The Metabolism of the Carbohydrates," in which this description occurs is written by J. J. R Macleod, M.B.—L. L.-af-H.*

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future. Professor Starling has referred to them repeatedly before the Commission. Professor Pawlow tells the story of a dog in which a pancreatic fistula had been established, one of those animals to which Professor Starling refers as just having had the anatomy altered a little, so that the animal is "quite happy," says Professor Starling; there is no inconvenience. I do not remember his exact words, but he repeats several times that the animals are "quite happy." Professor Pawlow says that one morning he found that one of these dogs had torn down part of the wall in the laboratory, to which it was chained, and he found the animal was lying on the pulverised wall, evidently to try and take away the pain and the suffering that came from the corrosion which was caused by the constant trickling of the pancreatic juice; and the next morning he found that this dog had again, in its efforts to save itself, torn down another part of the wall to which it had been chained. And then Professor Pawlow found that he could devise certain means of taking away part of its pain and discomfort by allowing the animal to lie on sand. But I wish specially to point out that these experiments are by no means free from pain or discomfort.

7277. (*Chairman.*) Are you giving us the account now of an experiment which can be done in England without anæsthetics, or, at least, of a case in which a dog may be kept alive after the experiment?—I am quoting this to show one instance of experiments which can be done, and are done, in England now under Certificate B, and which have been specially brought out by Professor Starling as the "ideal" experiments of the future.

7278. But what you have been telling us happened in Russia?—Yes, because Professor Pawlow has been referred to.

7279. (*Sir John MacFadyean.*) Was that an exceptional case that you read, or was it the normal result of the experiments?—Professor Pawlow in several pages here relates the great difficulties he has had in making the state of these animals as normal as possible, but he by no means pretends that they are in a normal state.

7280. But would this be an average case or a special one?—This is a passage on page 6 in Professor Pawlow's book. It says: "In a very short time the abdominal wall becomes macerated by the escaping juice, and in places even fairly large bleeding patches appear. These continuously irritate the animal, and prevent the collection of pure juice by means of the funnel." Then, further on, he says that even now, when he has succeeded by many new methods, and after much trouble in preventing this corrosion and maceration of the skin, he seeks better methods, because the continual escaping of the pancreatic juice puts the animals in an abnormal condition, which interferes with their general state of health.

7281. But perhaps it was the exceptional painless cases that Professor Starling had in his mind when he spoke about the ideal condition. How do you know that he referred to the bad cases?—I wish specially to point out that Pawlow himself, after having quoted all his difficulties and improvements, says: "Our conditions are yet far from ideal."

7282. (*Sir Mackenzie Chalmers.*) These were early experiments?—No; these are the latest. I have the book here.

7283. What is the date?—1902. A new edition is being prepared, and, I hear, will be out in three weeks.

7284. (*Sir John MacFadyean.*) But Pawlow lectured in this country within the last two years, did he not?—I believe he lectured here last autumn.

7285. May he not have described the improved methods in those lectures?—I think he described the latest improvements, as he terms them, there; but it is after describing his improvements that he says of his methods: "Our conditions are yet far from ideal."

7286. But it was prior to 1902?—I do not think there has been any change since 1902, but I am not competent to answer that. I was not present at the lectures.

7287. You wish us to infer that Professor Starling's remark about Professor Pawlow's operations being ideal refers to those which you have quoted from the book?—Yes; that is what I understand.

7288. That is what you offer to us?—Yes, that is it; and may I also say that those conditions which are desired to be ideal, but which are not in reality ideal, are exposed by another scientist—Paulesco, of Bucharest—who has written in the "Journal de Physiologie et de Pathologie générale," Paris (January, 1906), that he, too, has found that the establishment of biliary fistulæ leads to many abnormal conditions in animals which interfere with and frustrate the experiments. That anæsthetics interfere with the results of certain experiments is pretty generally acknowledged. That all physiological experiments are disturbed by the use of anæsthetics is freely admitted by the school of Pawlow. It is true that this school adds a theoretical and aspirational condemnation of pain, too; but in practice it is shown that pain is considered a lesser evil, and whenever there is a real conflict between moral and scientific claims, the vivisector chooses adherence to the claims of his science.

7289. (*Chairman.*) That is yours—you are not quoting?—That is my own.

7290. But, when you say that, what you are saying is that you apply it to English vivisectors. I suppose when you say the vivisector you mean the English vivisector?—I apply it to all vivisectors, including English.

7291. Then do you mean that the English vivisector, when he can get a better result with pain, defies the law?—Yes; I assume that. And I have a right to assume it at the present moment, because Professor Starling has said before this Commission that were there no such things as anæsthetics he would yet admire the man who performed experiments without anæsthetics—that is to say, the man who tortured animals for the sake of science.

7292. That is quite a different thing?—No; that is the same thing, I believe.

7293. That is quite a different thing from saying that he disobeys the law?—I only bring that forward in support of what I have just said—that if there is a real conflict between moral and scientific claims, the vivisector would choose adherence to the scientific claims.

7294. That is to say, that he would disregard the law, and perform an operation which caused pain because he got a better result?—I am not at present suggesting that anybody breaks the law.

7295. (*Sir John MacFadyean.*) Is there ever such a conflict in your opinion as you suggest there?—There is sometimes such a conflict as I have shown, and on the Continent no secret is made of such a conflict.

7296. Then you do mean to suggest to us that English vivisectors do break the law when that conflict arises?—Am I obliged to say again that I have not come here to suggest that British physiologists break the law.

7297. (*Chairman.*) But you use an argument which has no force whatever unless you are suggesting that?—I have come here to bring evidence of the existence of pain even in England.

7298. The sooner we come to that the better pleased I shall be. What we want is not suggestions that by a certain interpretation of a passage in a book such operations are encouraged, but real facts to show that they are actually performed?—I believe I am coming to that too, if I am allowed to proceed.

7299. The sooner we get to that the better?—With vivisectors and the defenders of vivisection cruelty is generally interpreted as the unnecessary infliction of pain and suffering, whilst in their opinion the supposed necessities of science at once justify the infliction of pain and suffering. This touches the very root of the unreliability of vivisectional anæsthesia. Foreign textbooks show great frankness in connection with the priority of scientific considerations to moral ones. For instance, Professor Robert Tigerstedt, a renowned physiologist, Professor at the University of Helsingfors, Finland, writes in his "Textbook of Human Physiology"—a book which is the standard textbook of German students, and which has now been translated into English, in 1906 (page 5): "Only if the purpose of the experiment makes it necessary is the operation performed on unanæsthetised animals"; and he adds: "Furthermore, it is many times of advantage in the experiment to suppress the voluntary movements of the animal, and this is done by administering the American arrow poison curare." A digression from textbooks into the records of experiments given by vivisectors in their journals and writings shows, if possible, still more conclusively their dislike of anæs-

thetics. A typical instance is afforded by a paper on "The Pathology of Asthma," by T. G. Brodie and W. C. Dixon, reprinted in "Collected Papers from the Research Laboratories of the Royal College of Physicians, London, and the Royal College of Surgeons, England" (edited by Brodie), Paper 19, Part I., 1903: "A considerable difficulty we met in all our early experiments, and which we only learnt to explain after much trouble, was due to the action of the anæsthetic. A large number of our early experiments gave negative results, which we ultimately found to be caused by the use of chloroform or ether as the anæsthetic. These, we found, paralysed the nerve endings in the bronchial muscles, and we only succeeded in obtaining constant results on vagal stimulation when their use was reduced to a minimum. We therefore adopted the practice of anæsthetising the animal with the smallest quantity of chloroform and then destroying the medulla and brain. No anæsthetic was then required." And further: "Thus slight reflex constrictions have been described as following excitations of the sciatic, superior laryngeal, central end of the vagus, the cornea and the nasal mucous membrane, but the tracings given in support of these statements are far from satisfactory. When working upon anæsthetised animals our attempts to produce these reflexes were almost entirely negative. It therefore became necessary to avoid the use of an anæsthetic altogether, or only to administer it for a very short time. As it was also necessary to keep the medulla uninjured, we worked upon decerebrate animals; but the conditions were still far from ideal, for the decerebration involves a rather lengthy operation under anæsthesia, and the injury produced in the majority of cases appears to set up a constant inhibitory effect upon the medullary centres. When working in this way we therefore decerebrated the animal and then kept it for some three or four hours before proceeding with the experiment, in order to allow ample time for recovery from the anæsthetic. But even under these conditions the results obtained have not proved as successful as we had hoped, and while giving the reflex effects we have at present succeeded in recording, we feel that there are still many undecided points to be worked out before we can understand the normal working of these centres." I have quoted that to show the difficulty that the physiologist meets with when he endeavours to employ anæsthetics, and how anæsthetics interfere with experiments.

7300. That is to say, that carrying out the law presents certain difficulties to physiologists. That is what they say?—Yes. In a book entitled "The Physiology and Pathology of the Cerebral Circulation," published in 1896, by Leonard Hill, M.B., who says he has opened "scores of living animals," there are clear indications that chloroform and ether are distasteful to the vivisector. The method of the experiments was the following: "A cannula was placed in the central end of the carotid artery. A second long cannula was passed down the external jugular vein, and on same side into the right auricle. The torcular heriphili was trephined, and a third cannula was screwed into the hole thus made. The cannula in the carotid artery was placed in connection with a mercurial manometer" (pp. 40 and 41). "My object," says Mr. Hill, "has been to make the method of research complete by simultaneously recording the arterial pressure, the general venous pressure, the intra-cranial pressure, the cerebral venous pressure." On page 40 we read: "The animals employed for this research were in most cases dogs. Morphia was the anæsthetic generally used; but the results have been controlled by the use of other anæsthetics. In no single case has morphia been found to obscure results, while ether and chloroform frequently do so." And on pages 58 and 59 we read: "Administration of chloroform produces a fall of arterial pressure and a rise in general venous pressure. . . . Ether produces a slighter fall of arterial pressure and a slight rise of pressure in the right auricle. . . . Morphia produces no peculiar effect on the cerebral or general circulation. . . . Thus morphia is far the best anæsthetic for researches of this nature. . . . the vaso-motor mechanism is in no way damaged by morphia, as it is by ether and chloroform." Sensory nerves were stimulated. We learn that: "Stimulation of any sensory nerve causes a rise of general arterial pressure and intra-cranial or cerebral venous pressure passively follows the rise." Curare was used, absinth, brain extract, and strychnine injected.

7300a. Is the object of reading that, to suggest that morphia is not an anæsthetic?—That brings me to my next point in connection with vivisectional anæsthesia and the "Shambles of Science," *i.e.*, that the use of morphia as an anæsthetic for vivisectional operations, especially when combined with curare, constitutes cruelty, and that such use of morphia is illegal under the present Act, as morphia is not, in the words of the Act, an "anæsthetic of sufficient power to prevent the animal feeling pain." Even in experiments before students morphia is used. I have seen severe experiments performed under morphia, for instance one described in the "Shambles of Science," under the title "An experiment that is not supposed to be useful." This experiment, which involved opening the chest of a dog widely, removing the ribs, setting up artificial respiration, enclosing part of one of the lungs in an oncomotor, and stimulations of various kinds, was, according to the lecturer himself (Dr. Brodie), performed under morphia. Other severe experiments described in the book were, in the words of the lecturers, performed under "morphia, and scarcely any anæsthetic at all," and under "morphia and a little chloroform."

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7301. (*Colonel Lockwood.*) Where is that described?—In the "Shambles of Science."

7302. (*Sir Mackenzie Chalmers.*) Are these the lectures that you attended?—These are the lectures described in the "Shambles of Science."

7303. Did you see this experiment?—Yes, I did.

7304. (*Colonel Lockwood.*) Under what head is it described?—The experiment that was performed, in the words of the lecturer, "under morphia," is described on page 55 of the first edition.

7305. (*Sir John MacFadyean.*) Was the animal curarised?—Nothing was said about that.

7306. Is that the experiment of which you say in the "Shambles of Science" that "we can be almost certain the dog has been also curarised"—that occurs on page 60?—Yes, that is the experiment.

7307. A minute ago you said that nothing was said about its being curarised?—I said that nothing was said about it.

7308-9. But you think it is almost certain that it was?—What we thought at the time was that it was certain it was curarised. I have already given instances of the vivisectors' preference for morphia and their dislike of anæsthetics. It is only natural that they should attempt to prove that morphia has the power of abolishing pain in vivisected animals. During the trial of Bayliss *v.* Coleridge, Mr. Bayliss said on the second day that morphia is an effective anæsthetic. I do not think I need go into the many statements to that effect that have been made. But these attempts to prove that morphia is an anæsthetic are futile. Vivisectors can have no greater authority than their own Claude Bernard, whose special work on the action of anæsthetics and narcotics is universally known and quoted, and whose name at the present moment appears in almost every textbook, and whose illustrations of vivisectional operations are reproduced in English textbooks. Claude Bernard quite appreciated the utility of morphia from the vivisector's point of view, though he was honest enough to acknowledge its utter uselessness as an effective anæsthetic for vivisected animals. These are his words: "The animal still remains sensitive, a touch on the cornea induces the closing of the eyelids, but he is quite still, and lends himself without a movement to the most delicate operation. . . . He feels the pain, but has lost the idea of self-defence."

7310. (*Chairman.*) Do you suggest that the great majority of physiologists consider that morphia is not an anæsthetic?—I have not formed an opinion as to whether the majority or the minority do or do not; but I can bring forward a great amount of evidence of those who do not consider it to be an anæsthetic.

7311. When you used the expression about vivisectors, you have no better authority. There are a great many authorities who differ, as you know?—But Claude Bernard is very much appreciated, if one reads Sir Michael Foster's "Life of Claude Bernard," how he looks upon him as a father, and asks to be allowed to look upon the physiologists of France as his brethren.

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7312. We have heard a good deal of Claude Bernard since we have been sitting here?—I am coming to modern physiologists, if I may, in a moment.

7313. (*Sir William Church.*) Would you be good enough to tell us whether that statement, which we have had frequently before us, and which we have just had from you, by Claude Bernard, was spoken of morphia generally, or of a particular quantity of morphia in relation to an animal?

7314. (*Sir Mackenzie Chalmers.*) Have you the quotation there?—I have the quotation; but I do not think I have got the exact dose of opium that he refers to. I shall be pleased to send it, because I have the exact reference at home.

7315. (*Sir William Church.*) Will you please give us the exact reference to this passage of Claude Bernard?—I will.

7316. You are not yourself aware whether he made that statement of morphia in any quantities?—He made it about the action of opium on animals generally. These are his words: "The animal still remains sensitive, a touch on the cornea induces the closing of the eyelids, but he is quite still, and lends himself without a movement to the most delicate operation. . . . he feels the pain, but has lost the idea of self-defence." That is taken from Bernard's "Leçons de Physiologie Opératoire," page 155. Prominent veterinary surgeons who, above all, should have experience in this matter, deny that morphia is an efficient pain-destroyer. Among them I may mention Mr. Sewell, who has said that, in his opinion, morphia is not an anæsthetic for a dog in any sense. And Professor Hobday, who, I feel sure, is the best authority on animal anæsthesia which I can quote ——

7317. (*Chairman.*) You are going a little too far in stating what witnesses said at the trial. If they are alive they can be brought here to give the evidence?—Then I will leave out the trial. I now refer to Professor Hobday, and I am very anxious to bring this before the Commission, because Professor Starling said that Professor Hobday is coming to give evidence; and Professor Starling evidently thinks that Professor Hobday will give evidence in illustration of the general ideas in connection with vivisectional anæsthesia prevalent among vivisectors; but I do not think that Professor Hobday will give evidence of that kind, for in his book, "Surgical Diseases of the Dog and Cat," published last year, there is an important chapter on the art of giving anæsthetics to dogs and cats. In contradistinction to Mr. Leonard Hill, and many other vivisectors, he considers chloroform the ideal anæsthetic to choose for the average adult dog. Instead of supporting the vivisectors' contention, that morphia is an anæsthetic for dogs, he says that, "compared with man, the dog is extraordinarily insusceptible to morphia" (page 46). He also says on pages 45 and 46 that "It (morphia) is classed as a narcotic rather than as an anæsthetic; and although it certainly deadens pain for minor operations, it does not do away with it altogether, as is the case with chloroform and ether. It is distinctly a useful aid in that the dazed, stupid condition in which the patient is put, renders it more tractable and easily managed. Professor Cushny has appeared before the Commission, and, in answering questions 5150 and 5270, has said that morphia is an effective anæsthetic for dogs, and that a large dose of this drug acts as an anæsthetic. These statements are of special interest, because in his "Text Book of Pharmacology and Therapeutics or the Action of Drugs in Health and Disease" (the fourth edition, 1906), Professor Cushny expresses entirely different opinions—opinions which make his statements before the Commission strangely contradictory, or which show that Professor Cushny holds one kind of theories when he speaks as a scientific pharmacologist, and another kind when he speaks as an ardent defender of the doings of vivisectors. On page 208 of this book, he says of the action of morphia on dogs: "The sensation of pain seems to be much lessened, but not entirely abolished, and reflex movements are difficult to elicit. After larger quantities an exaggerated sensibility to external stimulation seems present, for the animal starts convulsively at loud sounds and on pinching, but when left undisturbed lies in profound sleep."

7318. Do you suggest that Professor Cushny there is offering an opinion that the animal was feeling pain?—My position is this: I had prepared this statement of Professor Cushny's in my evidence which I was to have given before the Commission when I was coming in March, as very strong and weighty evidence in favour of my view that morphia is not an effective anæsthetic in dogs. To my great surprise, on reading Professor Cushny's evidence before this Commission, I found that he entirely contradicted what he had said in his own textbook, where he says that larger quantities in the dog produce an exaggerated sensibility, and also that pain seems to be lessened, but not entirely abolished.

7319. (*Sir John MacFadyean.*) Are these not doses which are still non-poisonous?—I do not think that is inferred. He says that larger quantities produce an exaggerated sensibility.

7320. Is it possible, I suggest, that they are still non-poisonous?—That argument cannot be used here, because Professor Cushny says that larger quantities produce an exaggerated sensibility—the more you give the more exaggerated sensibility to external stimulation seems to be present.

7321. You think that follows?—I think it follows. I have the book here. He says at page 208: "The sensation of pain seems to be much lessened, but not entirely abolished, and reflex movements are difficult to elicit. After larger quantities an exaggerated sensibility to external stimulation seems present, for the animal starts convulsively at loud sounds and on pinching, but when left undisturbed lies in profound sleep."

7322. (*Sir William Church.*) But do you wish us to infer from what you have read and stated of morphia or opium that it is impossible, first of all, to stupefy a dog so that it cannot feel pain; and, secondly that it will not die afterwards from the effects of opium?—I think morphia is as little an anæsthetic for dogs as it is for human beings; and I am entirely supported there by Professor Hobday, who does not consider morphia in any way an anæsthetic.

7323. I wish you would give me an answer, please. Do you wish the Commission to take evidence from you that morphia, if given in large quantities, will not produce complete anæsthesia?—Yes.

7324. I mean in quantities that are fatal?—Yes.

7325. That is your statement, that it will not produce anæsthesia?—What I wish the Commission simply to take as my evidence is that I do not consider morphia to be an anæsthetic at all.

7326. You say that you do not consider it; but do you state that in your opinion an animal cannot be made perfectly unconscious with morphia, so deeply unconscious that it will not continue to live after a certain time?—I suppose you can poison it?

7327. Do you deny that you can do it in man?—I suppose you can poison a man to death with morphia, as you can poison a dog to death with morphia, I am not denying that, but at present I am speaking of the anæsthetic properties of morphia, and I say that it has none.

7328. Do you mean that a man, when unconscious from morphia, feels pain?—No, I do not mean that.

7329. Do you mean that when an animal is unconscious from morphia it feels pain?—There is a very great difference between an anæsthetic and a narcotic. If morphia were an anæsthetic it would be included in the numerous textbooks on anæsthesia.

7330. Never mind about the textbooks. I wish simply to get your opinion before the Commission. Do you think that when a man is under the complete influence of a sufficiently poisonous dose of morphia or opium he feels pain?—That case is somewhat apart from my case here. I must still maintain that I am speaking of morphia as an anæsthetic, not as a poison.

7331. (*Chairman.*) How long may a person or a dog live after it has received a poisonous dose, a dose that will ultimately kill it, without its coming to?—That question I think had better be asked of some physiologist.

7332. Is it not rather material in answer to what Sir William Church asked you? Supposing that a dog lived for half an hour under a dose that was really poisonous—supposing it had resisted that poisonous dose, could it not be operated upon during that half hour?—It is very difficult for me to answer. Witnesses often answer questions in a few words which give quite a wrong impression because they are not

allowed to state their whole point of view, and I do not wish to give an erroneous impression.

7333. (*Sir Mackenzie Chalmers.*) May I take it that your point is that if a large dose of morphia is given the animal may die, but that up to the point of death it may feel pain?—That is my point; and that is brought out by the evidence of Claude Bernard and many others.

7334. That is your point?—Yes.

7335. (*Sir John MacFadyean.*) Is the contrary impossible?—May I also state that large doses of morphia have been known to act as a violent stimulant?

7336. (*Sir William Church.*) I beg your pardon, but the question I asked you was in the case of a dose which is perfectly fatal, when the animal is not excited, but when it has become perfectly unconscious. Do you think that animal feels pain?—Then my answer is that we do not know, and that the evidence is overwhelming on the side that that animal may be feeling pain although it may be lying perfectly still.

7337. Do we or do we not know whether a man in that condition feels pain?—I think we know that a man fatally poisoned with morphia does not feel pain.

7338. Then on what grounds do you infer that an animal in a similar condition feels pain?—Because morphia, as Professor Hobday points out, acts differently on dogs and men. He says that dogs are extraordinarily insusceptible to morphia.

7339. Then your answer to me is that Professor Hobday says so?—Yes.

7340. (*Sir John MacFadyean.*) Do you know whether Professor Hobday has ever administered a poisonous dose of morphia to a dog? Are the experiments that he has carried out not cases in which he has employed non-poisonous doses? He dare not employ a poisonous dose on his patient?—That I do not know.

7341. But that is very material if you quote his evidence as being conclusive?—May I also say that such terribly poisonous doses of morphia would interfere entirely with physiological experiments? There again I quote Claude Bernard and various other authorities. For instance, experiments on the secretion of the salivary glands are frustrated by too much morphia or opium; and I think if a dog were poisoned to such an extent with morphia the object of the experiment would be defeated.

7342. (*Sir William Collins.*) Do you quote Professor Cushny to show that if it be true that a dog under morphia betrays an exaggerated sensibility, at any rate in that stage, it is not in an anæsthetised condition?—Yes, that is why I quote Professor Cushny; and I have one more sentence from Professor Cushny's book relating to morphia upon cats. May I also say that the opinions of Professors Charles Richet and Dastre in France, and Professor Sollmann in America, are that morphia is not an effective anæsthetic? I quote Professor Charles Richet especially on the point of anæsthetising animals in laboratories.

7343. (*Sir William Church.*) Do you mean by these gentlemen saying that it is not an anæsthetic that if it is used in large quantities it does not prevent the animal feeling pain?—I mean to say that if large quantities would induce anæsthesia, I am sure that these gentlemen would not say so definitely that it is not an anæsthetic. Professor Cushny gives his experience of the effect of morphia on cats and other animals and we learn that "In the cat and in all the other *felidæ*, and in the horse and ass morphin seems rather to increase than to diminish movement. The animal runs about the room, or in a circle, and seems unable to remain at rest for a moment. At the same time a depression of the intelligence and of the power of perception makes itself manifest, for no definite attempts at escape are made, and obstacles are not avoided so carefully as by the unpoisoned animal. Eventually, convulsions similar to those seen after strychnin are developed" (p. 207). In connection with that, but I think the Commission will not care to hear it, I have brought considerable evidence to show that a great many experiments are performed on cats in this country under morphia; but I will leave that as there is no doubt thrown upon it, I should think.

7344. (*Colonel Lockwood.*) Under licence, I suppose?—Yes, under licence.

7345. (*Chairman.*) At any rate, if they are we should have the witnesses who were present at them to tell us what took place?—But it is not illegal in England, of course, to use morphia as an anæsthetic, and my point of view rather is that it should be illegal.

7346. (*Sir Mackenzie Chalmers.*) It is not a prohibited anæsthetic?—No. My point of view is that under the present Act of 1876 it should not be counted as an efficient anæsthetic. That is why I have brought all this evidence forward.

7347. We have been told that morphia is very often given as aiding other anæsthetics which are always given. You do not object to that?—I think one can have no objection to it when merely preparatory to anæsthesia, but I object to experiments which are done only under morphia or under morphia and curare, and especially on cats. Professor Hobday I know said to me that morphia is not an anæsthetic for dogs, and still less so for cats.

7348. (*Sir John MacFadyean.*) In non-poisonous doses?—Professor Cushny says that the larger the dose the more exaggerated the sensibility.

7349. But Professor Hobday did not say that?—But Professor Cushny does. I did not discuss that particular point with Professor Hobday, but I have no doubt that he would say the same. If it acts on cats in that way, so that they are unable to remain at rest for a few moments, it cannot surely be an anæsthetic. I should now like to be allowed to draw the attention of the Commission to the difference between vivisectional operations and surgical and veterinary operations under anæsthesia, as attempts are often made to identify the two.

7350. Have you yourself attended at some veterinary operations?—I have attended at some veterinary operations, and I have also attended some vivisectional operations, and surgical operations too, and therefore I am anxious to draw a distinction. Veterinary operations are not disturbed, but aided, by deep surgical anæsthesia. Vivisectional operations, dealing as they do with the delicate measuring of functional activities, are often disturbed by deep anæsthesia. Suffering after veterinary operations can be mitigated in every way. Suffering after vivisectional operations often forms part of the experiments, its causes and development being carefully recorded. Through the courtesy of Professor Hobday I was some time ago allowed to be present at an abdominal operation on a dog which was in a state of perfect surgical anæsthesia, in which every sign of consciousness or pain was entirely absent, and the dog lay in an absolutely limp and motionless condition through the action of chloroform administered by a special anæsthetist who devoted his whole time and attention to this, and who kept the anæsthesia uniform by constant and careful doses of the anæsthetic. The difference between this anæsthesia and conditions which I have seen in the physiological laboratories is very great. The whining and struggling dog which I saw on February 26th, 1903, at the Physiological Laboratory University of London, in which the abdomen had been opened, the intestines pulled over to the right side, and the kidney placed in an oncometer, the neck opened, and which was subjected to the usual stimulation of nerves, compression of veins, and injection of foreign substances, is an instance of a victim of the light anæsthesia which the vivisector prefers.

7351. (*Colonel Lockwood.*) That you saw yourself?—Yes. It is described in the "Shambles of Science" under the title "A Troublesome Dog"; it was troublesome because it whined.

7352. (*Sir John MacFadyean.*) When you say it was a victim, you intend to suggest that it was conscious of pain?—I intend to suggest that so far there were signs of consciousness.

7353. What do you wish us to believe, because there is no point in it if the animal was quite unconscious?—The difficulty of proving this is that nobody, neither the Commission nor I, nor any vivisector, can say whether that particular dog felt pain. I simply say that the dog whined and cried and seemed to be conscious, and we never can go by any other signs. How can we know whether an animal feels pain; we can only go by signs?

7354. I merely asked you whether you wished us to believe that the animal was conscious?—I wish to point this out as a typical instance of the kind of anæsthesia that is used in vivisectional experiments, and the great difference between that anæsthesia and the anæsthesia that I saw in Professor Hobday's surgical room. The object of the veterinary surgeon is generally the alleviation of pain and suffering; the object of the experi-

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mental physiologist and pathologist is sometimes the production of pain and suffering. And this brings me to my next point—namely, that books intended for students do not even avoid referring to the production of pain in animals as part of the ordinary routine of experimental experience. In his book "The Elements of Human Physiology" Professor Starling speaks of the stimulation of the (sensory) afferent nerves of the heart (page 249) in these words: "We may investigate their functions by stimulation of the central ends of the divided nerves. They may produce one or more of four effects: (i.) Pain as evinced by the movements of an animal not fully under the influence of an anæsthetic (we should be more correct if we said that stimulation of these nerves produced reflex movement)." Professor Starling no doubt speaks from personal experience here, and yet he has told the Commission, in answer to Question 3451, that though he has been engaged in the experimental pursuit of physiology for the last 17 years, he has never seen pain inflicted in any experiment on a dog, cat, or rabbit. On page 373 Professor Starling enlarges upon his knowledge of the effect of painful stimulation, and tells us that "any painful stimulation of a sensory nerve, especially in the abdomen, excites a reflex inhibition."

7355. (*Chairman.*) I do not know enough of these terms, but when he uses the word "reflex," does that imply that they do not feel pain?—That has really very little to do with it. A reflex movement may be painful or it may not.

7356. It may be painful?—Yes, but he speaks of the production of pain specially.

7357. (*Dr. Gaskell.*) On what page?—On page 373, "painful stimulation of a sensory nerve"; and on page 249. Another book which refers to the production of pain in a similar way is Mr. Hodgkinson Lack's "An Introduction to Physiology" (published 1905), and written for the student and future medical practitioner. This book is certainly not a very important one, but it is written in a way which is characteristic of the spirit with which even the younger generation of vivisectors treat experiments upon live animals. Anæsthetics are not mentioned, and animals are written of in this style (page 68): "Take a healthy frog and dissect out the steadily beating heart." On page 73, on afferent nerve fibres (vaso constrictor): "Dissect out the cervical sympathetic in a rabbit. Section it, and the arterioles in the rabbit's ear relax, and the ear becomes flushed with blood." Or (vaso dilators): "Dissect out the chorda tympani in a dog, and section it, no marked changes will be noticed in the submaxillary gland" (page 74). Or page 124 (the function of the renal tubules): "An animal is taken, and the medulla is sectioned below the level of the vaso-motor centre, and therefore there is a fall in general blood pressure. . . . The animal is kept alive by means of artificial respiration." On pages 52-53-54 stimulation of a spinal nerve and its roots is dealt with, and the author gives a diagram with indicatory letters, and points out in a table the various effects of stimulation, pain being several times included among the results. I wish specially to emphasise that the indifference or the callousness which generally characterises these books must tend to lessen the humane feelings of, and to demoralise, all those who take up the study of physiology, and also that the conspicuous absence of special warnings against cruelty in a field of research, manifestly open to abuse, even in the eyes of those who do not consider it inseparable from cruelty, brings proof that the boasted humaneness of the modern vivisector is an attempt to quiet the public, and to allay the steadily-growing opposition to vivisection.

7358. (*Chairman.*) Do you read that last passage as an instance of callousness?—I read the last passage and refer to the last book as an instance of callousness.

7359. Why? It is written, I suppose, for medical purposes?—It is written for the student and future medical practitioner, and is meant to serve as an incentive to further study.

7360. But what is there that is callous about it if he is actually describing the facts? He is not, as I understand, using any brutal expressions about it; he is describing it in the ordinary way?—The callousness would come in in the fact that this is a book which is supposed to be an educational work, and that, whilst he has time to write of all the details of the experiment, the curare and artificial respiration, he does not even mention anæsthetics throughout the book.

7361. He does not need to mention them at that time?—Also the style in which the animals are spoken of: "Take an animal." "Take a healthy frog, and dissect out the steadily-beating heart."

7362. Supposing you were describing the action of a cook, or of somebody who is recommended to kill a fowl in a particular way, you would not expect the cookery book to give directions to the cook to be particularly careful over it. I cannot see where the callousness comes in?—It is all, of course, a matter of opinion. What seems callous to some people does not seem callous to others. I can say confidently that to the humane section of the community this book would seem callous because of the omission of any reference to anæsthetics.

7363. It does not seem callous to me; and everyone who does not agree with your view of that passage is inhuman, I suppose?—No.

7364. (*Sir John MacFadyean.*) Might I ask the name of the author of that book?—I have said that this book is not among the most important ones. It is Mr. Hodgkinson Lack. His name appears in the Medical Directory for 1905, but I cannot find it in the Directory for 1907. However, he was a licensed vivisector in 1905. I believe he lives in Edinburgh, but I do not know much about him. I only know that the "British Medical Journal" in a review some time ago did not seem to admire the book, but said that it was written for cramming.

7365. I was just going to ask you, was it not a cram book when students are expected merely to get up facts with regard to results?—Even if it is ——

7366. Do you suggest that it is an invitation or incitement to students to go and do the things which are described in it?—I suggest that, because the author himself says that the book is meant to serve as an incentive to further study, and as an incentive to further study I maintain that it could have been accompanied by a little humane counsel or a few words at least about anæsthetics.

7367. (*Sir Mackenzie Chalmers.*) Is there not a distinction between study and research?—Yes, there is, but thoughts generally lead to action, and therefore I would say that humane thoughts ought to prevail in these books, as actions are the fruits of thoughts.

7368. (*Sir John MacFadyean.*) Do not you think that the author was much less ambitious, that he merely expected his book to serve as a cram?—His ambition did not prevent him speaking of curare and artificial respiration. I wish his ambition had led him to talk of anæsthetics, too, which it might easily have done

7369. (*Chairman.*) However, you quote that as a strong instance of the callousness of the medical profession?—No, not of the medical profession. I was speaking of vivisectors.

7370. (*Sir Mackenzie Chalmers.*) Physiologists?—Yes, licensed vivisectors.

7371. (*Chairman.*) Who are supported by a large majority of the medical profession?—It remains to be seen, of course, how many they are supported by.

7372. I did not know that it was disputed that they were supported by a large majority?—No, perhaps not.

7373. (*Sir William Church.*) All these text books and various other books that you have quoted you quote to show that they do not suggest humanity to students, but if anything you say that they would rather lead to callousness on the part of students, because, in your opinion, they are calculated to incite students to do these operations for themselves, and also to cause them to be callous as to the pain that they may inflict?—I have not quoted them for the purpose of showing that they may incite students to take up illegal vivisection; that has not been my point of view; but I have quoted them to show that the general tendency in them is one of disregarding altogether all moral or humane considerations, and that the study of physiology at the present moment cannot but be entirely indifferent to the humane and moral claims.

7374. But then I want to ask you further whether it is not the fact that anybody who wishes to experiment or to witness these experiments has to either perform them or see them done in a licensed laboratory?—Yes.

7375. And do you mean us to infer that the tone of the directors of these laboratories is one tending towards cruelty?—In quoting these text books I have criticised the tone of them, but I am quite prepared, if I am asked the question, to say also that in my personal experience I have found nothing in the teaching of physiologists which justifies the claim that is made that vivisectors nowadays do everything to further humaneness. I have seen nothing which justifies the claim of vivisectors that the modern vivisectors are endeavouring to inculcate humaneness. That is the red thread in that part of my evidence.

7376. The object of your having quoted all these textbooks to us this morning is to show that the directors of the physiological laboratories in general are regardless of the sufferings of animals, and do not impress upon their students the necessity for humanity at all?—That cannot have been my object, because the authors of these textbooks are not all directors of laboratories—at least, I have not put that forward; but I have put forward these books, and have quoted from them to show the lack of any signs of a wish to inculcate humaneness in the theory and practice of experimental physiology.

7377. Why should they, because all this vivisection to which you allude goes on in laboratories where, of course, the experimenters come into communication with the directors of those laboratories, and generally—at least, I believe so—they receive advice as to their action with regard to animals?—That claim might have been put forward if vivisectors had not put forward this other claim on which I base my statement, that everything is done to inculcate humaneness. And then, further, everything is also done to make young students interested in experimental physiology, to take it up. Professor Starling has advocated his views before the Commission that students should first attend vivisectional demonstrations, and further take up vivisection themselves for the purpose of acquiring manipulative skill, and under those circumstances, as vivisectors are so anxious to spread their teachings and to reach the younger generation, I maintain that their indifference from the point of view of humaneness and mercy and kindness and all such things shows that they are indifferent on that point, an indifference which sometimes amounts to callousness.

7378. You would not go so far as to say that the directors of physiological laboratories generally teach their students that they should study by means of scientific experiments, perfectly irrespective and regardless of pain, that they need not consider that at all. That is rather the effect of your remarks this morning on my mind?—I do not think I have suggested that. In fact, my suggestion is not that there is any deliberate encouragement of cruelty, but that there is an entire indifference to the humane side of the question.

7379. (*Chairman.*) Will you proceed?—I wish specially to emphasise that the indifference and callousness which generally characterise these books must tend to lessen the humane feelings of and to demoralise all those who take up the study of physiology, and also that the conspicuous absence of special warnings against cruelty in a field of research, manifestly open to abuse, even in the eyes of those who do not consider it inseparable from cruelty, brings proof that the boasted humaneness of the modern vivisector is an attempt to quiet the public and to allay the steadily growing opposition to vivisection. Among such demoralising literature, a book just published by Messrs. Macmillan and Company, in London, is conspicuous. It is "Collected Papers on Circulation and Respiration," by Sir Lauder Brunton. In 1907 Sir Lauder Brunton calmly publishes a paper (included in this volume) on "Experiments on the Influence of Digitalis on the Pressure of Blood in the Arteries" (page 103). (I have that book here, too) which, it is true, records experiments performed before the present Act, but which, nevertheless, are of a peculiarly brutal nature, and couched in words which cannot but give the direct lie to the assertions of vivisectors, that the old-fashioned experiments would be met with disgust to-day. These experiments, which involved opening the neck of dogs and sheep, exposing the carotid arteries, and inserting the nozzle of a hæmadynamometer, and injecting the digitalis through the open jugular vein, have, I believe been referred to before this Commission by Professor Cushny as having been done under anæsthetics (Q. 4655). Professor Cushny also asserted that had no anæsthetics been used the experiments could not have been performed owing to the movements of the animals. That is at numbers 4660A and 4663, and also that the animals were not allowed to recover. The following extracts from Sir Lauder Brunton's own paper show the inaccuracy of Professor Cushny's statements: Experiment I., November 25th, 1865——

7380. (*Chairman.*) But really it is extremely immaterial whether an operation before 1876 was or was not performed under anæsthesia; it was not illegal at the time, and it is taking up time unnecessarily. You said something about Sir Lauder Brunton alluding in brutal terms to some operations before 1876. Will you read what those brutal terms were?—I say my point of view is not that these experiments were performed in 1865. My point of view is that the republication in 1907 proves that vivisectors are not ashamed.

7381. I quite understand that, but you said that it proved that he was callous and brutalised on the subject of operations, because he spoke of these operations which were before 1876 in brutal terms?—Yes.

7382. What are the brutal terms?—I am coming to them.

7383. Would you come to that—because that is the only material thing?—I said that these experiments were nevertheless of a peculiarly brutal nature, couched in words which cannot but give the direct lie to the assertions of vivisectors that the old-fashioned experiments would be met with disgust to-day.

7384. Will you refer to the terms in which he speaks of them?—May I refer to their peculiarly brutal nature? The experiment was performed on a pointer. "It was tied down to a table and chloroform administered while the carotid was being exposed." This animal is described as struggling (p. 105). The same animal was used for Experiment II. on November 29th. The experiment began at 1.58, and among the author's remarks I note the following: "Time, 2.53.—Dog appears to be sick, but does not vomit, the muzzle preventing him from opening his mouth at all. Dog moans. Time, 2.55.—Groaning much increased and louder. 2.56.—Appears to be endeavouring to vomit. 2.59.—Convulsions. Stoppage of heart's action. Dog dead." The post-mortem revealed that "The veins and right side of the heart were engorged and swollen, stuffed like sausages, as Caspar describes it." On the same day, Experiment III. was performed on a full-grown sheep. The experiment began at 8.35 p.m., the sheep being tied down on the table. No chloroform was given. Extract from remarks: "9.22——

7385. (*Sir William Collins.*) Does Sir Lauder Brunton say that no chloroform was given?—No chloroform. I have the book here.

7386. (*Sir Mackenzie Chalmers.*) What is the date of it?—1865.

7387. Were they in England?—Yes.

7387A. (*Sir William Church.*) Was it not in Scotland, to be quite correct?—They were in Britain; they may have been in Scotland. "9.22" (this is a sheep).—"Animal excited by examining the wound. 9.45.—Sheep struggling. 9.47.—Sheep struggles." The next morning, on November 30th, the sheep "seemed still much depressed," and between 1 and 2 p.m. it was again tied on the operating table. This time chloroform was administered, and the other carotid opened. Extract from remarks on this experiment: "2.11 p.m.—Sheep appears completely free from the effects of the chloroform. 2.14.10.—Violent struggles take place. 2.16.15.—An apparently convulsive movement of the head. 2.17.10.—Violent struggles. Another struggle. 2.22.10. — Animal violently struggling. 2.33.—The sheep was allowed by all to be dead." On December 20th the fifth experiment was made on a large dog.

7388. (*Chairman.*) Is this the same sort of thing that follows?—Yes. I will not read it all; I will just give an extract. On December 20th the fifth experiment was made on a large dog, a kind of bloodhound. Chloroform was given, but Sir Lauder Brunton explains that after he had exposed the carotid, which lay very deep, the dog was seized with convulsive tremors. Then he describes the course of the experiment in these words: "Howling loudly, as if suffering much" (p. 111). Then he speaks of the eye, and says: "Pupil less contracted, seems normal, sensitive." Another experiment—Experiment VII.——

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7389. I do not think that multiplying instances is any good. Surely three are enough?—It is characteristic that in describing another experiment he states that "the dog appears to be free from chloroform, and seems to feel pain, as he groans" (p. 121).

7390. That is exactly the same thing; but those are operations that cannot be performed now; the Legislature thought they were improper and cruel, and put a stop to them; therefore it does not help us much now to describe those experiments. But, supposing they had to be described, how would you describe them in different language? Do you say that it is inaccurate language?—I would never publish them now. I should have been thoroughly ashamed of them.

7391. Do you mean to say that in an article to be read by scientific men, you think that a scientific man is doing something brutal in publishing it?—I think so, if his book is likely to be read by a very large number of students. And, further, the Commission has heard these same experiments described from the scientific point of view. That scientific view has been put before the Commission by Dr. Cushny, and therefore I beg leave to put our view before the Commission. I put them before the Commission from the moral point of view; they are the same experiments, and from the moral point of view I say that they are repellent, and I care not whether they are useful or not; from the humane point of view they are utterly worthy of—I will not use a term.

7392. They are illegal now. The law has said that they are illegal on the ground that they are cruel, and there is an end of it. I do not see the object of reading out an account of them. I thought you read it for the purpose of showing that Sir Lauder Brunton, as a leading physiologist, rather gloated over things of this kind when he described them. I can see nothing except an ordinary description. If these things are to be described at all, they seem to me to be described in the ordinary language that you would expect from an educated man?—I read them simply as being material to my point of view, to show that the modern literature of vivisectors and physiologists tends to demoralise those who read it, and I brought, as the very strongest instance I could bring, the republication in 1907 of such terrible experiments in a book which can be bought for 7s. 6d. by any medical student.

7393. (*Colonel Lockwood.*) And to destroy the opinion that vivisectors wish to get abroad that they are humane in their ideas?—Yes. It has often been said that the old-fashioned experiments would never be allowed; that they would be met with disgust and hatred to-day.

7394. (*Sir William Collins.*) Are these the same series of experiments in regard to which Professor Cushny answered the noble Chairman, at question No. 4660a, by saying, "I am quite sure Brunton would use anæsthetics"?—Yes.

7395. (*Sir John MacFadyean.*) Is your train of argument this: that, assuming that in the past painful experiments, which would not now be allowed, had really contributed useful knowledge with regard to the cure and prevention of disease, all reference to those experiments and to the facts they disclosed should be blotted out of published books?—My point of view is that the vivisector who is responsible for them would certainly not republish them in 1907 if he really had any feelings with regard to humaneness, or want of humaneness, in carrying them out.

7396. So that you would really establish a sort of Press censorship to obliterate all record of any past painful experiments, although they had contributed very useful knowledge?—I think that certainly from an educational point of view it would be much better and more in accordance with the claims made by vivisectors.

7397. (*Chairman.*) That, I understand, ends that part of your evidence?—That ends that part.

7398. And we come next to your own experience of experiments?—Yes.

7399. (*Chairman.*) Coming to these experiments described in the "Shambles of Science," it struck me that it would be a convenient course (but if it is not you will tell me so) if I asked you—to take as an instance the "Painless Experiments" whether as regards the facts stated in your book you adhere to the statements you made there, or whether you wish to alter anything?—No, I adhere to everything.

7400. Then I will take it in that way, and any questions can be asked you upon it with a view to making you modify or explain anything?—If you please.

7401. I will take it very shortly then. You have already told us that you proceeded to attend a series of lectures in 1902, which you afterwards described in the book called the "Shambles of Science"?—Yes.

7402. The first of them, I see, is called "Painless Experiments," at page 12; I have got the older copy?—Yes; page 11, I think.

7403. That was an experiment on a frog, and I think what you say there is that the frog was not properly pithed, or, at any rate, that sufficient was not done to destroy its nervous sensation—its feeling?—It was one of the ordinary experiments on frogs which are not properly pithed; and I have some evidence that I am very anxious to bring out in connection with that very subject, if I am allowed to do so.

7404. If it is what somebody else says about it on that particular case, I am afraid we must have the witness before us. We cannot have your account of what another witness may say about it?—It is a theory that I wish to bring forward in support of what has been said in this chapter on "Painless Experiments." It is a theory of my own, but it is one which is supported by other people.

7405. As to what happened to the frog?—No, not as to what happened to this particular frog, but as to frogs generally in physiological laboratories.

7406. We shall get into some very wide questions if we go into that. We had better treat it simply. You can hardly call that in support of your own account of what took place?—It is in support of what took place, and it is solely in support. It is in support of my account of what took place; but I can very well, if it is the wish of the Commission, wait and answer all questions first.

7407. You say that it applies to this particular case?—It does.

7408. Then we will take it now?—This experiment describes certain very ordinary experiments upon frogs which are often performed in physiological laboratories, and which form the very ABC, if I may use that expression, of experimental physiology. These frogs were, in the opinion of orthodox physiologists, incapable of feeling pain because the brain had been destroyed. The theory I wish to bring forward is my own firm belief that it has not been proved that a frog is incapable of feeling pain so long as it is capable of these complicated purposive movements which show sensation and volition, and several other qualities, which, at any rate, cannot be associated with any animal that is said to be dead—because very often these frogs are talked of as being "dead"; and my contention is that these frogs are not dead. That is my first point. And the second is that it is not proved that they are incapable of feeling pain.

7409. You have not, I presume, tested your theory by any experiments on frogs?—No, I have not. Then, in connection with that, I think that Professor Starling's evidence before the Commission brings out very clearly that students are at the present moment allowed to experiment upon frogs. I have the exact reference here.

7410. (*Colonel Lockwood.*) Without licence?—Without licence—because students do not generally hold a licence. It was in answer to Question No. 3747 that Professor Starling said of students: "At present they have to get a large extent of their practical experimental knowledge from a frog; a frog is a long way from man." And then he goes on to advocate that students should be allowed to experiment on a rabbit. This statement seems to show clearly that Professor Starling does not consider it a contravention of the Act that students should at present be allowed to perform experiments themselves on frogs. I find similar statements in other books, especially in a book called the "Physiological Action of Drugs," by M. S. Pembrey and C. D. F. Phillips, in which there are statements in the preface which show that physiologists at the present moment look upon experiments on frogs as legitimate for students to perform. Now, the Act applies, so far as I understand, to all vertebrate animals, and I am

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not going to repeat the mistake which I believe the Editor of the "British Medical Journal" made in 1897—I suppose it was a slip *—of saying that the frog is not a vertebrate animal. The frog is supposed to be a vertebrate animal, and as such I maintain that the frog should come within the influence of the Act of 1876.

7411. I daresay you will be asked some questions about your account of what you saw by various Commissioners; I do not propose to ask you any other question myself?—May I say further in connection with this that the phenomena that we describe in this chapter are quite ordinary phenomena? There is nothing extraordinary in them. Brainless frogs are capable of jumping and croaking; they can feed by catching passing insects, and the longer they live the more do they resemble intact animals. The whole question whether brainless animals are capable of feeling pain is an exceedingly important one in connection with antivivisection and the whole vivisection question. The frog is so low down in the zoological scale that the spinal cord is capable of taking up the functions of the brain when the brain has been taken away; and we find this in a lesser degree the higher up we go in the zoological scale; but even dogs can live without their brains. Goltz had a dog which lived more than a year and a half without its cerebral hemispheres. He believed that this dog had lost understanding, reflection, and memory, but not sensation—that it was capable of emotions and voluntary power, that hunger and thirst were present. These dogs of Goltz experienced satisfaction when fed, became angry when attacked, saw a very bright light, avoided obstacles, heard loud sounds, and could be awakened by them. Professor Sherrington, in a well-known book, called "Schäfer's Text Book of Physiology," on the subject of cutaneous sensation, refers to the dogs that were experimented upon by Goltz in these terms: "The dogs from which Goltz has removed the cerebral hemispheres reacted to 'painful' stimuli in a way which was interpreted as evidence of physical pain." I have some further extracts from books by Dr. Ferrier and Professor Starling to show that rabbits deprived of their brains are capable of feeling pain.

7412. (*Sir Mackenzie Chalmers.*) When deprived of the brain, or a portion of the brain?—The whole brain. Dr. Ferrier says, on page 146 of his book, "The Functions of the Brain," a very well-known book: "Animals deprived of their cerebral hemispheres are still capable of exhibiting, in response to various forms of sensory stimulation, special and general, reactions, more or less complex, which do not at all differ in character from those which we associate with feeling or emotion." . . . Rabbits scream piteously, and exhibit the various signs of agitation characteristic of intense pain if their toes are pinched, or any sensory nerve severely stimulated." And Professor Starling, in his "Elements of Human Physiology," though not dealing with it from the psychological point ——

7413. (*Chairman.*) We are getting rather a long way from the frog in the second experiment?—It all bears upon the question.

7414. I think it is getting rather far from that question, because I presume there is no doubt that physiologists would agree that dogs and rabbits come under the Act?—Yes.

7415. And any operation of taking out their brains would be something done under the Act?—May I just bring one instance? There is a tendency at present, just because anæsthetics are so distasteful to vivisectors, which I have already referred to, to perform experiments on animals that have been deprived of their brains; and the vivisectors would then consider that those experiments are absolutely painless, and they would no doubt be performed under licence alone—in other words, counted as equal to employing an anæsthetic. Thus Dr. T. G. Brodie and Miss Winifred C. Cullis performed some experiments on the secretion of urine (they are described in the "Journal of Physiology" of May, 1906), and they say: "We therefore" (that is because anæsthetics were for some reason not suitable) "worked upon animals (dogs) which have been killed by pithing." Pithing there does not include, evidently, the destruction of the spinal cord; because I admit that an animal that has had its brain and spinal cord destroyed is incapable of feeling pain; but my point is that it is not proved that animals which only have had the cerebral hemispheres removed are incapable of feeling pain. Dr. Brodie, after saying first, "We therefore worked on animals which have been killed by pithing," continues: "Such animals recommence to breathe within a few minutes. . . . Such animals survive in this condition for some four to eight hours." As they had been "killed" in this case, that expression at least seems somewhat incongruous.

7416. (*Colonel Lockwood.*) What page is this?—"Journal of Physiology," Volume XXXIV., number 3, 1906, page 234.

7417. Have you got it here?—Unfortunately I have not brought it, but I can send it to the Commission. I believe that Miss Winifred C. Cullis, who was demonstrator of physiology at the London School of Medicine when I attended lectures there, held no licence; and I should think that if she holds no licence this experiment would simply be counted at present as an experiment not within the Act, because the dogs are supposed to have been killed. Yet they survived for from four to eight hours.

7418. (*Chairman.*) Were experiments tried upon the dogs during those four or five hours?—Yes.

7419. (*Sir William Church.*) Would you tell the Commission how they were enabled to carry on a certain amount of vital action?—They were enabled because according to Goltz and other observers a dog can be kept alive for 18 months without its brain.

7420. But how were these particular dogs that were experimented upon kept so that the vital action could go on during the four to six hours during which the experiments were done?—I think that vital action goes on perfectly if the cerebral hemispheres have been removed.

7421. This has nothing to do with the cerebral hemispheres. You tell us that these dogs were pithed, and then Professor Brodie and Miss Cullis carried out some experiments during four or six hours, I think you said, on the urine?—Yes.

7422. Would you tell the Commission how these dogs were enabled to go on performing certain vital functions?—I do not think anything is said in that chapter as to how they were enabled to do so.

7423. You have not tried to inform yourself of the reason why these dogs differed from any other dog that is pithed and then kept alive. Are you not aware that pithing is an immediately fatal operation in an animal if you leave it alone. You understand what pithing is?—Yes, of course.

7424. Cutting across the spinal cord?—No, the cutting may not be across the spinal cord. That is just another point that I wish to bring out before the Commission; that pithing is a word which covers so very many things. Real pithing, I maintain, means destruction of the cerebral hemispheres and the spinal cord.

7425. Will you tell us what it meant here in this particular case; that is what we want to know?

7426. (*Sir Mackenzie Chalmers.*) Do you know how this dog was pithed?—No information is given as to how it was pithed, and I cannot tell the Commission. I used the words which were used by Dr. Brodie and Miss Cullis.

7427. (*Chairman.*) Does Dr. Brodie say the dog was pithed?—He uses the word "pithed" exactly as I read it. What I wanted especially to bring out was that pithing is used for a great many different things. It is used by some vivisectors and physiologists in the sense, which I believe is the accurate and proper sense, of the total destruction of the brain and spinal cord. It is used by others merely to convey destruction of the cerebral hemispheres.

7428. (*Sir William Church.*) Would you kindly tell us the different physiologists who use the term in those different senses?—I can if you will allow me time to look it up in the books which I have brought.

7429. I do not mean now, but will you send it to us afterwards?—Yes; I thought of preparing a list of them, and then I thought it would not be required.

7430. (*Chairman.*) Perhaps you will give us a note of it later on?—Yes; and I will also look up that par-

* *See "Zoophilist" of July, 1897, and June, 1902.—L. L.-af-H.*

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ticular point as to Dr. Brodie and Miss Cullis's special form of pithing here. I will endeavour to get it for the Commission.

7431. You mean what is referred to in the chapter on painless experiments?—Yes.

7432. Then the next chapter is: "An Experimental Production of Blood Clotting by Injection of a Nucleoproteid." The suggestion there, I think is that the rabbit which was being operated upon gave signs which satisfied you that it was not being adequately anæsthetised?—Yes, Mr. Thane, the Inspector under the Act of 1876, has referred to this chapter before the Commission, and he has characterised our suggestion contained in this sentence: "And would not the tetanic spasms that followed counteract the effect of a possible anæsthesia?" as ridiculous; and he then goes on to quote from a paper by Professor Halliburton, published in the "Journal of Physiology," Volume XVIII., page 138, an account of the very same processes, though couched in more scientific, and, of course, non-sympathetic language. What we call struggles is there interpreted as "slight stretching movements." We say "The eyes become bloodshot and start from their sockets"; whilst the scientific terms are: "Extreme exophthalmos and dilation of the pupils." We describe that the animal fell back dead, as it was supposed, and that it resumed its struggles after having received a stimulus; whilst the scientific paper admits the very same thing by saying that "sometimes after ceasing to breathe for a minute, the animal gives a few breaths more, and sometimes it resumes breathing normally." If Mr. Thane had taken the trouble to quote other paragraphs from the "Journal of Physiology," describing similar experiments with blood-clotting by Professor Halliburton,* he would have found the phenomena which we describe and insist on describing as struggles, referred to as "pronounced stretching movements of the fore and hind limbs which occur immediately before the death of the animal"; and in the case of a large black dog, weighing 30 lbs., experimented upon at 4.44, the result was stretching movements and spasms. The struggling movements are referred to as becoming more pronounced at 4.57, whilst at 5 o'clock an additional injection resulted in extreme stretching movements. An absolute line of divergence between extreme stretching movements and struggles might be difficult to find. The spasms described by Professor Halliburton in his paper tally with the spasms described by us. Mr. Thane considers the suggestion that tetanic spasms might counteract the effect of a possible anæsthesia ridiculous. I maintain that it is not impossible. Our suggestion is intended to convey that the presence of convulsions indicated that the animal was not under the influence of an anæsthetic. The fact that anæsthetics are constantly used as anti-spasmodics and known to allay convulsions generally, indicates that deep anæsthesia and convulsions are two incompatible conditions. In the "British Medical Journal" for June 29th, 1901, there is a description of an experimental inquiry into the pathology of gastric tetany by Professor Halliburton and Dr. McKendrick. Tetany fluid, prepared from the vomit of a patient suffering from gastric tetany, was injected into a cat under anæsthesia, and the writers remark that "no spasms or convulsions were produced, whether these would have occurred had not the animal been anæsthetised it is impossible to say." These words show that Professor Halliburton does not consider it impossible that the anæsthesia was responsible for a suppression of spasms, which leaves room for the counter suggestion.

7433. No doubt questions giving an opportunity of making that statement would have been asked you by other members of the Commission. I think perhaps as you have told us that you adhere to your acounts of these experiments, that will be the shortest way of dealing with them. I put the question to you in that way so that there should be no mistake as to what is meant by them. That deals with the experiments in the chapters beginning with "painless experiments," and ending with "a grand demonstration"—that is the last of the pure descriptions of experiments. You say that those accounts are correct?—Yes.

7434. Then I think I will not ask you any further questions, but other members of the Commission may desire to do so, and if, when they have asked them, there is something that you wish to add in explanation you will have an opportunity of doing so?—If you please.

7435. (*Colonel Lockwood.*) You maintain that the quotations given in the books you read tend to show that science, and not humanity, are inculcated in the physiological books of which you have given us extracts?—Yes, that is what I intend to convey.

7436. That is what you gave us those extracts for?—Yes.

7437. Have you any reason to believe, from personal knowledge, that the law as it at present exists is evaded in the laboratories?—Of course, several of the accounts in the "Shambles of Science" would tend to show that the law, if not evaded, as it stands admits of cruelty.

7438. We have had some conversation about light anæsthesia, and you read from some books on the subject of light anæsthesia as, I suppose, opposed to complete anæsthesia. But would you acknowledge that light anæsthesia may produce complete insensibility as well as deep anæsthesia?—I would not acknowledge it, and I do not believe that surgeons as a rule operate under light anæsthesia. There were some very interesting letters on that subject, published some time ago (I have the references if they should be required) to the effect that surgeons as a rule in our great hospitals do not operate under light anæsthesia; and one surgeon in particular, Mr. Edmund Owen, in referring to a certain number of operations which, from their nature, cannot be performed under deep surgical anæsthesia—that is operations on the throat, on the palate, and certain other operations—ends his letter to the "Times" by saying: "Why should it be necessary to spread out these hideous things before the public gaze?" (March 15th, 1902). And another of these surgeons said that operations under incomplete anæsthesia would be "as embarassing to the operator as painful to the patient."

7439. If done without anæsthetics?—If done under light, incomplete anæsthesia.

7440. Therefore, what you maintain is that operators on mankind would never use light anæsthesia; they would always work under complete anæsthesia?—They would always endeavour to get complete anæsthesia except for certain operations where such anæsthesia would be dangerous.

7441. But they are not averse to using light anæsthesia on animals from the fear that their experiment might be spoiled if they used complete anæsthesia? That was your point, was it not?—Yes, that is my point.

7442. You have seen some operations on animals?—Yes.

7443. Have you ever seen a distinct breach of the law that you could point out to us?—Of course, I do not know if you can go over the whole trial again, but there was, in my opinion, a distinct breach of the law.

7444. As stated in the "Shambles of Science"?—As stated in the "Shambles of Science."

7445. (*Chairman.*) The question that was asked was whether you had been present when any such operation had been done?—My answer to that must be, that I have been present at several experiments at which the anæsthesia was not complete or satisfactory from a humane point of view; and then I must leave it to the Commission to infer whether that is a breach of the law or not; I have not taken it from that point of view.

7446. (*Colonel Lockwood.*) Could you verify the names of the operators?—I can give the names of all the operators.

7447. Where you saw animals which you believed were not under complete or satisfactory anæsthesia?—I can give you an instance.

7448. Can you give us the reference?—That of "A Troublesome Dog."

7449. You give that as one instance?—I give that as one.

7450. Who was the operator?—Dr. T. G. Brodie, assisted by Dr. Alcock.

7451. And have you any other?—All the experiments in Professor Starling's laboratory—that is, all the ex-

* *Journal of Physiology, Vol. XVIII., 1895. "The Intravascular Coagulation produced by Synthesized Colloids," by W. D. Halliburton, M.D., F.R.S., and T. W. Pickering, D.Sc. p. 283.—L. L.-af-H.*

periments performed by Mr. Bayliss, did, in my opinion, give very little guarantee that the animals were insensible, especially the one of which an account is given under the title of "The Struggling Cat."

7452. Who performed that operation?—Dr. Bayliss, in Professor Starling's laboratory.

7453. In "A Long Experiment," is that one you witnessed yourself?—Yes, all these I witnessed myself.

7454. And in all those, or most of them, you believe that the animal was sensible to pain of some sort?—More or less. I believe, to make a general statement, that the anæsthesia was not satisfactory from the humane point of view. And there were certain circumstances connected with the experiments which led me to infer that—notably, in some cases the omission of the constant renewal of the anæsthetic, which always has to be done; and in other cases the admission of the lecturer or demonstrator that morphia had been given, morphia and a little chloroform; and in other cases the behaviour of the animal, as, for instance, "The Struggling Cat," or "The Troublesome Dog."

7455. May I take it that you object to experiments on living animals altogether (it is only with a view to eliciting your opinion); or that under complete anæsthesia, if you believed the object was necessary to save human life, you would not have objected?—I object on principle to all experiments on living animals, and I do not believe they could be useful in saving life.

7456. You object to all experiments on living animals?—Yes.

7457. Whether anæsthetised or not?—Yes.

7458. As demoralising to the man who operates and to the public who see it?—Yes.

7459. Supposing that the law is continued in some shape or other, could you assist us by any suggestion beyond the suggestion you have made, that opium should never be used as an anæsthetic because you do not think it is one? Could you give us any suggestion as to alterations in the law that would mitigate any sufferings that might happen under the present law?—As a first alteration of the law I should certainly suggest the prohibition of demonstration experiments before students, as being, to my mind, abolition piecemeal, which would take away a very important part of vivisection, and would certainly do very much to help the anti-vivisection cause. Then, though I am not in favour of the exemption of one animal and the leaving out of others, I should say that all experiments on dogs, cats, monkeys, and all the higher animals are especially objectionable, and especially experiments on monkeys. I have a monkey myself, and have personal experience of its extreme intelligence and sensitiveness.

7460. From your experience of what has gone on and what you have read, do you believe it would not be hostile to science if those higher animals were left out?—I think it would be very much objected to by the representatives of science.

7461. That would be another point. Do you believe that it would not be dangerous to science?—I do. I believe that all experiments on living animals could very well be dispensed with by science.

7462. And would you suggest any further safeguarding of animals by way of more inspection?—No, I would not suggest more inspection.

7463. You think that more inspection would be useless?—I think that inspection would perpetuate the practice.

7464. Therefore the addition of inspectors, or surprise visits, or more numerous inspections, would all be useless from your point of view?—Yes. Of course, I think the present inspection an utter failure; but I do not feel justified in saying that I should agree to any steps for further inspection.

7465. It would not help your cause a bit more?—No.

7466. (*Chairman*). When you say that you are against all operations, does that include operations for testing the effect or the safe dose of new drugs on animals?—Yes, it does; it includes all inoculations and testing experiments; because I am convinced, in fact, I know, from having seen the effects of such inoculations at the Pasteur Institute in Paris, what suffering they involve. It is not only the prick of a pin.

7467. (*Sir William Church.*) Would you kindly tell me what is the date of, I think it was Dr. Halliburton's textbook, which you referred to in connection with your "Dog Injection with a Substance Derived from a Lunatic"?—I referred just now to the "Journal of Physiology." I did not refer to any text book.

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7468. I think you quoted Dr. Halliburton as proving that very little was known with regard to the action of choline?—I will give the exact words.

7469. I only want the date of the book?—It is the seventh edition of Professor Halliburton's "Handbook of Physiology," formerly Kirkes' "Physiology," published in 1905.

7470. That is the edition you quoted from?—Yes.

7471. That is what I wanted to know. Thank you?—I have found the quotation. It is page 177.

7472. I do not think I need trouble you to read it again. I think you thought that his experiments which he had performed on cholin were of no practical value at all?—I quoted his own words to show that he himself found so.

7473. I do not think he quite says that?—He says: "It is possible that such tests may be of diagnostic value in the distinction between organic and so-called functional diseases of the nervous system"; and I quoted that experiment as one that could not properly be defined as absolutely necessary, because, for one thing, it has no therapeutic value, its diagnostic value is doubtful, according to Professor Halliburton himself, and its physiological value is not absolute, because on this very page Professor Halliburton himself maintains that cholin could just as well be tested by a purely chemical test. These are his words: "The tests employed to detect cholin are mainly two: (1) A chemical test, namely, the obtaining of the characteristic yellow octahedral crystals of the platinum double salt from the alcoholic extract of the cerebro-spinal fluid of blood; (2) a physiological test, namely, the lowering of arterial blood-pressure (partly cardiac in origin, and partly due to dilatation of peripheral vessels), which a saline solution of the residue of the alcoholic extract produces."

7474. (*Chairman.*) When you said "absolutely necessary" just now, you meant for the purposes of demonstration?—Yes, that was my first point.

7475. (*Sir John McFadyean.*) I thought you said a minute ago that Professor Halliburton himself alleged that these tests were of equal value? That does not follow from what you read; they might both be tests, and one might be useful in certain circumstances, and the other might not, and one might be better than the other?—He does not say anything about it.

7476. But I think you used language which implied that Professor Halliburton said that they were of equal value; that one could be used in place of the other?—The words of Professor Halliburton imply that they are of equal value. He says there are two ways of testing cholin, (1) and (2); and I maintain that when there is a chemical test which would be equally useful, it is not necessary to include the physiological demonstration.

7477. (*Sir William Church.*) This lecture that you attended was at the University of London?—This particular lecture was at the University of London.

7478. But it was for advanced students, was it not, not for ordinary medical students?—It was for ordinary medical students; all the medical students from the London School of Medicine for Women who wanted to go to these lectures could go. Any medical student can attend them. They are called advanced classes.

7479. They have to apply for admission?—Yes, you have to apply. I think any student under the University of London is allowed to go. We applied for tickets from the Principal of the London University.

7480. (*Colonel Lockwood.*) What is the course called?—It is called "Advanced courses of physiology, open to all students within the University of London."

7481. (*Sir William Church.*) But it is an advanced course for students?—Yes, it is called an advanced course.

7482. Would you look at page 76 of your book. You say: "The lecturer tells us that the animal is anæsthetised with morphia and some chloroform, and adds: 'These unusual circumstances under which the experiment is performed will perhaps interfere with

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the results.'" Was it the morphia and chloroform which constituted the unusual circumstances? What do you imply by that?—We imply simply the question. We ask whether Professor Halliburton meant that the unusual circumstances were the chloroform and morphia. We imply nothing more than the question.

7483. Then why do you put in the book: "Was it the morphia and chloroform which constituted the unusual circumstances'?—That is just what we did not know. That is why we put it as a question. We did not put it as a statement; we put it as a question.

7484. (*Sir Mackenzie Chalmers.*) A question with a meaning attached to it, though?—Perhaps.

7485. (*Colonel Lockwood.*) That is the experiment with a substance derived from a lunatic?—Yes.

7486. (*Sir William Church.*) Did you not gather that it was an experiment which he thought was better performed under morphia and chloroform than under A.C.E., or chloroform alone?—The lecturer said nothing about it. He told us that it was performed under morphia and chloroform, but he gave no reasons why he had chosen those two drugs.

7487. (*Sir Mackenzie Chalmers.*) Might not the quotation have this meaning: that it was unusual for him to give both morphin and chloroform, as he usually only gave chloroform or the A.C.E. mixture?—It might mean that. Of course, I cannot commit myself to say what he meant. It is merely put as a question.

7488. But still it might mean that?—It might. It might also mean that on other occasions only morphia was used.

7489. And if what I suggest was his explanation, should you accept it?—Yes, I think I should accept it.

7490. (*Sir William Church.*) You know that experimentation with cholin is a new thing; the action of cholin has only lately been found out?—Yes.

7491. That you were aware of when you went to the lecture?—But I think that things which are just found out should not be tried before students immediately.

7492. I am not speaking of students; but you were aware of that?—Yes, that was just why I urged that it is not a necessary experiment, because it was so new.

7493. But you do not suggest that this demonstration is one which would be performed before ordinary medical students?—Yes, the medical students who attended those lectures were ordinary students.

7494. You make no difference between an ordinary medical student who has to attend a certain number of physiological lectures and an advanced student?—No, I think any medical student had the right to attend those lectures.

7495. The demonstration was given specially, was it not, to advanced students; it was headed "Demonstration for Advanced Students"?—When we came to attend these lectures, the conditions were these: There were placards in the London School of Medicine for Women that these courses were given in various places, saying where you could obtain tickets, and anybody who applied was admitted. We applied in that way, and got our tickets, and were admitted.

7496. The chemical test that you have alluded to is a difficult one, is it not?—I have never seen the chemical test. I should not think it is more difficult than many others.

7497. The physiological test is more easily obtained—the effect of cholin on blood-pressure?—I do not know whether it is more easy to try it on a live dog than to try it in a chemical test tube. I should say that the chemical test tube ought to be nearer at hand.

7498. Are you aware whether it has been found that, as the result of the researches that have led to the discovery of cholin and its action, it has ever been used practically or not?—I believe that the discovery of cholin has a certain practical value in the treatment of nervous disease, but I am unable to see that the injection of cholin into live dogs is of any value whatever, because it has no therapeutic value; it is merely a product of disease.

7499. Was not the object of the experiment to show the action of cholin upon blood pressure?—Yes.

7500. And has it been found already of much use, as Dr. Halliburton says it may be found? I wanted to know whether you were acquainted with any of the investigations which have been made in it from a practical point of view?—No, I am not acquainted with any use that has been derived out of experiments on dogs with cholin.

7501. You are not aware that it has been already used to distinguish between cases in which there is nerve degeneration, and cases in which no degeneration is present?—That I am quite aware of. That is why I said a minute ago that the discovery of cholin would undoubtedly be of great value as a means of distinguishing between various different diseases. But I say again that that does not touch the point of injecting this product of disease into a live dog. That has no value whatever, so far as I can see.

7502. How are you to investigate it and find out what the effect of cholin is and whether it is prevalent in the blood of the patient, unless by experiment?—By the chemical test which Professor Halliburton refers to as one method.

7503. The chemical test is a more difficult one?—I do not think it matters if it is more difficult; chemical experiments are generally difficult. I do not think it is more difficult than others.

7504. Your impression was that the animal was not fully anæsthetised in the case which you describe?—I do not think we make any suggestion that the animal was under insufficient anæsthesia. The suggestion is made that the animal was killed in a very inconsiderate way.

7505. I should like to ask you a question or two about how it was killed afterwards. But I drew myself, perhaps quite incorrectly, the inference from your question at page 76: "Was it the morphia and chloroform which constituted 'the unusual circumstances'"? that you were rather under the impression that morphia having been used, the dog was not under full anæsthesia?—The inference there is whether the use of morphia and chloroform constituted unusual circumstances. The inference seems rather to be what are the usual circumstances, what happened on other occasions.

7506. Then you do not doubt that this dog was under complete anæsthesia?—I cannot say that we were satisfied, but there was nothing special during that demonstration to suggest any bad form of light anæsthesia.

7507. You thought that the method of killing was improper?—The method of killing is really what is mostly criticised in this chapter besides the nature of the experiment—that the assisting demonstrator did not even take the trouble to see that the animal was really dead.

7508. But surely he did. Would you tell us what happened?—It was exactly as it is described in this book.

7509. (*Sir Mackenzie Chalmers.*) Would you describe it in your own language?—The demonstrator attached a glass tube to the opened jugular vein, and first we were told that the animal was to be killed. Then he blew a little air into the jugular vein and then sat down, so far as I remember, among the audience, and the lecture went on, and the animal was supposed to be dead; but after a minute or two the dog began to struggle and to show signs that it was not dead, and for some time nobody observed it.

7510. (*Sir William Church.*) In what way did it show signs that it was not dead?—By struggling. "It has begun to struggle, and seems to be in great pain." "At last the lecturer happens to look at the 'dead' dog. He says something to the assistant demonstrator, who leisurely rises from his seat and strolls up to the animal." The suggestion there is rather that the assistant demonstrator ought to have got very upset over his bad killing, and should have rushed to the animal and killed it quickly.

7511. (*Chairman.*) You said in your book "strolls" to the animal?—Yes.

7512. You say now that he "rushed"?—The suggestion we make is that he should have been in a greater hurry than he was.

7513. (*Sir William Church.*) You know that blowing air into the vein is a way of killing animals?—Yes, I know that; I have seen it stated somewhere that it has

to be done very carefully—that different animals require a different amount of air.

7514. You do not suggest that blowing air into its veins enabled this animal to feel pain?—No, not that in itself.

7515. It was really that the assistant demonstrator was not successful in his first blow?—And also that he did not care sufficiently that he was not successful; that when the animal struggled his attention was not so directed towards the animal—that he did not notice it at first, but it took some time, and that that implied a certain amount of carelessness.

7516. How long a time elapsed between these two blows, do you think—the first blow and the second?—That I could not possibly say at this time; it may have been three minutes, it may have been four; it may have been five minutes.

7517. You do not remember?—I could not remember now.

7518. Would it be five minutes?—At the utmost five minutes, I should say. It may have been three or four minutes.

7519. Do you think that the struggling of the little dog indicated that it was coming out of its anæsthesia?—Yes, I think it did.

7520. That is why you think it was careless that it was not watched?—Yes, the whole attitude of the lecturer and the demonstrator I think indicated carelessness.

7521. You are quite sure that these struggles did not follow the second blow of air into the veins, that they were before?—They were before.

7522. Did any follow the air being blown into the veins?—They followed the first blowing into the veins.

7523. But the second?—We do not say that he blew in air a second time. That it is not said. It is said that he resorted to his scalpel. I do not think there is anything said about a second blowing in of air.

(*Chairman.*) No, you say that the scalpel was used?

7524. (*Sir William Church.*) What I asked was when the struggles took place; whether they took place before or after the second blowing of air into the veins?—They took place after the first blowing; there was no second blowing, so they could not take place after that. They took place after the first blowing.

7525. (*Sir John MacFadyean.*) But you say that he repeated it?—No. We say "In order to stop any further pranks played by this obstinate life, he resorts to his scalpel."

7526. I beg your pardon; I was reading another passage to you: "He repeats it and then sits down among the audience." I take it that you describe that as one operation; but, nevertheless, you say that he repeated the operation of blowing into the veins?—But that happened before he sat down among the audience. Those were done whilst he was still standing by the dog. I did not count that as two blowings. I understood that the question was whether the demonstrator first blew the air into the animal and then sat down, and then went and blew in air again, and then afterwards resorted to the scalpel.

7527. You count the first two as one?—Yes.

7528. (*Sir William Church.*) I did not understand that before. Are you quite sure that these struggles which you thought meant returning to consciousness, were not the effect of the air being blown into the vein?—I do not know whether the effect of blowing air into the vein is struggling.

7529. You had never seen it done before?—I had never seen it done before, but I have never heard or read anything to that effect.

7530. You are not aware that struggles and convulsions are the way in which death generally takes place in these cases where air is blown into the veins?—Even if it were so it is no proof that the animal may not have been returning to consciousness.

7531. I am not saying whether it is proof of that or not; I suggest that the explanation of the struggles and convulsions may have been the blowing of air into the veins?—I am perfectly sure that in a deeply-anæsthetised animal the introduction of air into the vascular system would not result in struggles.

7532. What makes you perfectly sure?—The general behaviour of an animal that is under deep anæsthesia—the totally limp condition and absence of struggles.

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7533. Have you ever worked at the question yourself? I do not know how far you worked in physiology; have you any practical knowledge? You say that you have never before seen air blown into the vein to kill an animal. Now you tell us that you are quite sure that if an animal is properly narcotised or anæsthetised it could not die with struggles and convulsions?—Yes, I am perfectly sure of it.

7534. Have you any reason for that certainty?—I have the reason of my general knowledge of the effects of anæsthesia. I have never seen, of course, for instance, air blown into the vascular system of a human being, but it is my general knowledge of the demeanour of the patient, whether animal or man, under anæsthesia, which makes me believe that if an animal is properly anæsthetised, it does not struggle when air is blown into the jugular vein.

7535. But may not your inference be incorrect; you are only giving us your inference?—Yes.

7536. Can you give us any facts with regard to it?—I daresay I could get some facts with regard to it.

7537. We are told—at least, I have been told; I may have been told incorrectly—that convulsions and struggles are the way that death occurs in these cases where air is blown into the vein, whether the animals are narcotised or not?—Nothing of the sort is said in this case about the animal coming out of the anæsthesia. It is simply said, "It has begun to struggle, and seems to be in great pain."

7538. (*Sir Mackenzie Chalmers.*) "Seems to be in great pain"—that is inconsistent with anæsthesia?—It is.

7539. (*Sir William Collins.*) Have you seen certain observations on the "Shambles of Science," prepared by Dr. Thane, and dated December, 1903, which have been supplied to the Commission?—No, I have not. I shall be exceedingly glad to see them.

7540. (*Chairman.*) You will understand that this was a confidential report made by Dr. Thane to the Home Office after the publication of your book. The confidential document has been handed to us, and you will be examined upon it; and it is simply because questions will be asked upon it that it can be handed to you for your convenience; but it is not handed to you for any purpose of publication?—I quite understand.

7541. (*Sir William Collins.*) I notice, in regard to a matter upon which questions have already been put to you about a dog injected with the substance derived from a lunatic, that Mr. Thane states in this paper: "The lecturer gives the following account of the alleged revival of the dog: 'The animal was fully anæsthetised throughout with morphia and chloroform. After the experiment had been successfully performed, I said we should now kill the animal by one of the most rapid methods available—viz., by blowing air into the dog's veins. The vein already previously exposed for the administration of the drug was used, and air blown in by my assistant in the experiment. The first blow was not, as it usually is, successful, because he omitted to clamp the vein when he stopped blowing, and a good deal of the air came back again. So he gave another blow, this time taking the usual precautions to clamp the vein afterwards. The blood pressure fell to zero, the heart ceased to beat, and the dog ceased breathing. This occurred in a few seconds. My assistant returned to his seat, and I said 'The animal is now dead.'" Have you any remarks to make upon that?—Yes; that would be the two first blowings, and then the assistant sitting down and Professor Halliburton remarking, "The animal is dead." Is there any account of the revival?

7542. Up to that point, you have no exception to take to the statement?—No, I think it is exactly the same as in "Shambles of Science."

7543. Then he proceeds: "The sudden failure of the circulation and respiration produced the usual asphyxial convulsions. These, which are due to either want of blood or presence of excessively venous blood in the nerve centres, especially in the medulla oblongata, do not indicate any return to consciousness. Fearing, however, that the convulsions might alarm

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some members of the class, I asked my assistant to kill the animal, which he did, and so put a final stop to the movements"?—I think that account is exactly the same, though in different terms, as that given in "The Shambles of Science"—the two blowings, the sitting down, and the movements of the dog supposed to be dead.

7544. You notice that he states that the usual asphyxial convulsions do not indicate any return to consciousness?—Yes, I quite expected that answer to be given.

7545. You think it is a matter of opinion rather than a matter of fact?—Yes, I quite expected that answer.

7546. Would you say that asphyxial convulsions are absent where the narcosis of chloroform has been carried out so as to terminate fatally?—I should think so. I have already quoted some evidence on that point, that, inasmuch as anæsthetics are used so much as antispasmodics and to allay convulsions, I believe that such convulsions are incompatible with perfect anæsthesia, deep anæsthesia.

7547. You are not speaking on that from your own experience?—No.

7548. (*Chairman.*) When you say you quite expected that answer from Professor Halliburton, do you mean to imply that it was untrue?—I do not mean to suggest that; but I mean to suggest that the point of view of the vivisector is so different from the point of view of the person who looks upon these experiments from the humane point of view, that you can only expect two totally and entirely divergent opinions; it always will be so.

7549. (*Sir Mackenzie Chalmers.*) You mean that the same phenomena produced two absolutely different views?—Entirely, and they always will.

7550. (*Sir William Collins.*) In the case of No. 13—"A Grand Demonstration," pages 139 to 148 of your book—I see Mr. Thane, referring to that, says: "The 'struggles' are denied altogether." That appears to be a conflict of fact?—That surprises me, because they were so evident that anybody must have seen them, and if they are denied, I should like to ask why so many efforts were made to anæsthetise these three animals. That is the only reason why the operation is recorded this book. They failed—constantly failed, showing the great difficulties accordingly—as the chapter ends by saying: "This was an example of careful, complete, and public vivisectional anæsthesia. What about the careless, incomplete, and private?" We looked upon this demonstration as giving an example of real carefulness, so far as carefulness can be practised.

7551. You say that you saw the struggles?—We saw the struggles as they are described in the chapter, "A grand demonstration."

7552. Then as regards page 142, Mr. Thane says: "The dog, it is known, had been fed by the attendant not very long before; it is untrue that it 'had not been kept long enough within the precincts of the physiological laboratory'"?—But we say in this chapter that this dog had recently come in, according to the statement of Professor Starling, made before the class. "For this experiment it is best to use a dog in a starved condition," says the lecturer. "This one, however, came in only recently, and his pancreatic juice has been secreted in digestion." Professor Starling said that, thereby suggesting that somebody had made a mistake in feeding this dog. In fact, these experiments are always performed after starvation. In Professor Starling's book, "Elements of Human Physiology," there is a phrase in these words: "If a dog be starved for six successive days and be made to do hard work on the last three, then it will be found that there is no difference in the excretion of urea," showing that starvation is sometimes a scientifically necessary preparation for an experiment. So it was in this case, and Professor Starling merely referred to a thing which is quite usual.

7553. Then you deny that it is untrue that it had not been kept long enough within the precincts of the physiological laboratory?—I deny it, and my denial is based on Professor Starling's words, that he complained that it had not been kept long enough.

7554. As regards page 147, Mr. Thane refers to "the miserable white rabbit." He says the "writer omits to state that it was dead"?—May I ask when it was suggested that the rabbit was dead? Surely not during the experiment when the intestinal extract was injected.

7555. The only light thrown by Mr. Thane on it was this: "'The miserable white rabbit,' the writer omits to state that it was dead. At page 145 it was sent out in order that it might be killed by an overdose of ether; and that is what had happened"?—But we do not suggest that the rabbit struggled after it had been carried out. Our description of its struggles is until the moment when it was carried out. Then we say nothing about it until we go out, and we meet the boy carrying the rabbit into the lecture room again, when the rabbit lay quite still. "The little creature lies still on its board." If it be true that it was dead, then I say that there is nothing in the "Shambles of Science" that contradicts that in any way. We say that the animal struggled until it was carried out as described, and then we saw nothing of this rabbit in the laboratory room until we were leaving, when we met the boy carrying in the rabbit again, and then we say: "The little creature lies still on its board, the head is wet, the hairs stick to the skin, the eyes are protruding and look like glass balls."

7556. You think it was superfluous to state that it was dead?—In other words, we met the rabbit in the door as we were going out, and we make that statement there in connection with that particular period, whether it was dead or alive.

7557. (*Sir John MacFadyean.*) You say, "It lies still"?—Yes, if it was dead it would lie still.

7558. You think that is not calculated to lead the reader to believe that it was still alive?—There can be no wicked suggestion there, because the suggestion would be that it was properly anæsthetised. There can be no harm in that statement.

7559. (*Sir William Collins.*) At any rate, you thought it was superfluous to state that it was dead?—We had no opportunity of seeing. The animal was carried in again when we were going out, and we saw that it was lying still—which shows how accurately we observed it if it was really dead.

7560. Then on page 9 of his report, Mr. Thane, referring generally to your work, says: "Two suggestions run through this work, expressed either openly or by implication. One is that the animals are not properly anæsthetised. This, in the only case in which it has been possible to test the question, has been proved emphatically to be untrue. I believe there is no better foundation for the insinuation in the other cases. Of the 13 lectures dealt with, eight took place at the University of London. Dr. Waller, the director of that Laboratory, is, one may say, an expert, probably the first in the country, on the physiology of anæsthetics. He has given much attention to this subject, and for the last two or three years has devoted, I believe, his whole time to the study of the action of anæsthetics on the nervous system." Is there any remark that you wish to make on that passage?—May I say, in answer to that, that I entirely and utterly disagree with Mr. Thane there; that in the case of the brown dog, to which he presumably refers, it was proved that the anæsthesia had been satisfactory; and, secondly, as the brown dog has been brought in so much before the Commission, I should like to be allowed to make a statement in answer to what I have seen in the printed evidence of Mr. Thane.

7561. (*Chairman.*) You refer to the evidence given before the Commission?—Yes, evidence given before the Commission.

7562. You can refer to anything which has been already put before us by a witness.

7563. (*Sir Mackenzie Chalmers.*) Was the brown dog the case in Bayliss *v.* Coleridge?—Yes.

7564. The jury heard your evidence on that case, did they not?—Yes, they heard my evidence.

7565. And they did not agree with your conclusion?—No, they did not.

7566. (*Chairman.*) You are not going to read your evidence given at the trial?—No, not at all.

(*Sir Mackenzie Chalmers.*) It is the same case?

(*Chairman.*) It is the same case, but the evidence will be on facts which have been brought before us.

(*Sir William Collins.*) And it is dealt with in the paper by Mr. Thane.

(*Witness.*) I have not put down a single word in the statement I have here in connection with the trial on the brown dog which is not absolutely essential to my answer to the statements which have been made by Mr. Thane and Professor Starling concerning that brown dog, and I therefore ask permission to give the statement.

7567. Will you proceed?—I now come to the evidence which I wish to give in immediate connection with the "Shambles of Science," and with special reference to the statements which have been made before this Commission. The book was published July 10th, 1903, by Mr. Ernest Bell. During a period of barely four months there were more than 200 reviews and notices in daily papers and journals of various kinds, and the book attracted a great deal of attention. Had the book been libellous, this period afforded ample opportunity for action. Previous to the publication of the book, Mr. Stephen Coleridge had asked us to give him a statement describing a certain demonstration at which we had been present. This was in illustration of a lecture by Dr. W. M. Bayliss at University College, on February 2nd, 1903.

7568. (*Chairman.*) Is not this going into the case at the trial?—No, it is not; it is only going into the case of the trial so far as it has been brought before the Commission. Certain facts have been stated by Mr. Thane that the book had been withdrawn.

7569. (*Sir William Collins.*) Which answers of Mr. Thane are you referring to?—The brown dog has been brought up before the Commission, I believe, two or three times, if not four times, already. It begins at Questions 1261 to 1283, including 1282.

7570. I see at Question 1261 Mr. Thane refers to a report which he made to the Home Office, for which he was thanked?—Yes.

7571. That is the same report from which I have been reading extracts to you?—That brown dog has been brought before the Commission in answer to Question 1551, and there are a great many questions up to No. 1569.

7572. (*Chairman.*) I am quite aware that it has been referred to, but I do not quite see how your repeating the statement that you made to Mr. Coleridge has anything to do with it?—It is essential that I should mention it, because I wish to prove that there was no connection between the trial and the book. The book was not libellous. It has been stated that the book was withdrawn. Mr. Thane said at Question 1276: "May I add that the 'Shambles of Science' has been withdrawn from publication?" (*Dr. Wilson.*) Only a particular chapter, I think? (A.) No, the whole book is withdrawn, I believe. (*Dr. Gaskell.*) I was told yesterday that a new edition had just come out, and was seen in a shop? (A.) Then that is quite relevant. But I should like to look at the letters that were sent in; because my recollection is that a letter was sent from the publishers saying that it was withdrawn." Then follows the whole letter, in which Mr. Bell promised to withdraw the book. As that has been brought before the Commission, I thought I might be allowed in a few words to refer to it, and to show fully and accurately what occurred.

7573. (*Sir Mackenzie Chalmers.*) Yes?—It is not very long; it is a very short account.

7574. (*Chairman.*) Well, we will see?—This was in illustration of a lecture by Dr. W. M. Bayliss at University College, on February 2nd, 1903. The animal experimented upon was a brown dog, which attracted our attention in a special way because it bore marks of previous vivisections, and because of its movements upon the operating board. It was afterwards admitted that we had been present at the third vivisection of this dog, and that the animal had moved, though the movements were declared by Mr. Bayliss and Mr. Starling to be due to a special disease and not to consciousness. Mr. Coleridge read the statement which we had given to him at a large meeting in St. James's Hall in May, and as a result of this Mr. Bayliss instituted proceedings for libel. During the trial, which took place in November, 1903, the connection between that statement and the chapter "Fun," in the "Shambles of Science," became established, and in consequence of this Messrs. Hempson, solicitors to Mr. Bayliss, threatened Mr. Bell with legal proceedings unless he signed an undertaking to withdraw all copies from circulation, and not to print or publish any further copies. Without committing myself to any criticism of Mr. Bell's action, I wish to point out that we gave no undertaking to withdraw the book from circulation, that we in no way concurred in Mr. Bell's decision, and that the rest of the chapters of the book could in no way be affected by the result of the Bayliss *v.* Coleridge trial. This, our opinion, was strengthened by a remark made by the Lord Chief Justice during the trial. He said, referring to the "Shambles of Science": "This book is not libellous."

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7575. I have never found any report of that taken by itself. Do you mean to say that he said there was nothing in the book that was libellous?—He said, referring to the "Shambles of Science": "This book is not libellous."

7576. Has that been reported?—It has not been reported, but at the time there were so many people who had heard the statement that a special list was circulated and signed by a number of people who had heard it. The reason why it was omitted from the report I do not know.

7577. I am quite sure it was never said.

(*Secretary.*) It is not in the transcript.

7578. (*Chairman.*) I am quite sure that the Lord Chief Justice never said it?—I know that he said it.

7579. If he said it, he said it with qualifications, and in a context which could not give it the meaning which you are giving it now?—He absolutely said those words. I have brought with me the list signed by a number of people who heard it.

7580. I do not care if a thousand people signed it; I should say that it had not got his authority. I am quite sure that the Lord Chief Justice never would confirm it?—I am sure that he said it.

7581. He says in the course of the trial that it is an undoubted libel to speak of a person having committed an illegal act?—He could not say that, because the book itself is not libellous, and cannot be libellous, because no names are mentioned. How could it be libellous when no names are mentioned?

7582. You do not understand enough law, I am afraid, to know that it is not necessary to mention persons' names to constitute a libel?—May I just say that had the book been libellous there was plenty of time to institute actions of libel against us. The book was out for four months.

7583. We are not here to try the question whether the book is libellous or not. What you are entitled to say is that inasmuch as it is made a point against you that you have withdrawn your statements because Mr. Bell withdrew that chapter from the book, that was not done by your wish or authority?—That is all I beg leave to say.

7584. You must confine yourself to that?—I am obliged to answer your questions, my Lord. I did not start this.

7585. You read a long statement?—I am only answering the statements that have been made. If there is anything which goes beyond that in this statement, I shall at once stop reading; but to the best of my belief I have not put anything in which is not absolutely necessary in answer to the statements that have been made.

7586. All I wish you to do is to confine yourself simply to that point. I quite understand that you say that the withdrawal of that chapter was a thing with which you had nothing to do, and which you never authorised?—Shortly after the withdrawal of the book an address of confidence was presented to us ——

7587. This is going beyond?—This is just to show how the book came out again.

7588. We must proceed on some regular system?—May I just put this to you? It has been said that the book has been withdrawn, in other words, that the book does not exist any more because it was a libellous production.

7589. We know that the book has been printed since —that a new edition has been printed with your authority?—That is all I wish to get clear.

7590. We have got all that before us?—May I now proceed with my direct answer? Professor Starling has said in his evidence, at No. 3545, that we gained admission to the London School of Medicine for

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Women by stating that we "wished to take up medicine as a serious pursuit." This is untrue and manifestly absurd; for, as Professor Starling should know, one cannot study medicine by merely taking up the subject of physiology, and we made no application for any all-round medical training. Mr. Thane has told this Commission that we have misrepresented the question of anæsthesia in our book. I beg to submit that we have stated nothing which is not amply borne out by other records and experiences of vivisectional anæsthesia, some instances of which I have laid before the Commission. Out of 12 chapters which contain descriptions of various experiments, two record attempts to produce satisfactory anæsthesia; three describe experiments under morphia and a little chloroform, which took place at the Physiological Laboratory, University of London. Of these animals, one is described as being quiet, and one as giving a good deal of trouble by struggling and whining, the third as struggling after it was supposed to have been killed by air blown into the jugular vein. The descriptions of three other experiments which took place at Professor Starling's laboratory, University College, contain no such record, for the lecturer, Mr. Bayliss, gave the students no instructions as to the existence or conditions of anæsthesia. During the trial of Bayliss *v.* Coleridge, he admitted that it was not his practice to tell the students about the methods of anæsthetising the animals (2nd day, 354). During one of those demonstrations, that described under the title "The Dog that Escaped," Mr. Bayliss stated that the animal was slightly curarised. Professor Starling has already answered questions on this chapter, and I must point out that he grossly misquoted it, and then characterised his own misquotation as "wilful deception."

7591. (*Sir William Collins.*) What is the number of the question?—4278. These are his words:—"The writers say: 'If the dog was under curare, if it had artificial respiration.'" Whereas anyone who takes the trouble to read the chapter can see that we did not use the suggestive "if" in either case, but, on the contrary, stated definitely, on the authority of Mr. Bayliss himself, that the dog had had a dose of curare, and, according to our own observation, that no artificial respiration had been established which was the cause of the early death of this particular victim.

7592. What is the page of your book?—That is called "The Dog that Escaped," page 67 in the first edition.

7593. Where are the references to "If the dog was under curare," which you say Professor Starling misquotes?—Professor Starling says that we said, "if the dog was under curare."

7594. What is the page in your book?—There is no such page; that is just my point. He entirely misquotes it.

7595. You mean that it does not occur in your book? —Yes.

7596. (*Sir Mackenzie Chalmers.*) That he has imagined the quotation?—Yes.

7597. (*Chairman.*) You say that he is misquoting some particular passage; what is the passage?—This is the passage, on page 68: "He struggles as much as he can till the lecturer injects something into the jugular vein; a peculiar stillness follows. 'The animal is now slightly curarised,' says the demonstrator; 'we want to excite the sympathetic and the vagus end.'" We do not say "if it was under curare," "if it had artificial respiration"; but we state definitely, on the authority of Mr. Bayliss, that the dog had had a dose of curare, and, according to our own observation, that no artificial respiration had been established. That is why I say that Professor Starling has misquoted the chapter. One experiment was performed under conditions which led us to infer that the animal had been curarised, although nothing had been said by the lecturer about anæsthetics or curare. Shall I give the reference to that?

7598. (*Sir Mackenzie Chalmers.*) I think it would be convenient if you would?—It is the chapter called "The only completely satisfactory method," page 129 of the old edition. This inference was based on Dr. Brodie's own words in his book "Essentials of Experimental Physiology," to which I have already referred, and his preference for curare which is expressed therein. One description, entitled "Painless Experiments," deals with the common experiments on frogs, for which anæsthetics are out of the question. Of the two remaining, one was performed, according to the lecturers, under morphia and scarcely any anæsthetic, and the other under morphia alone. Mr. Thane has given as his opinion to the Commission that there is no foundation for our statement that "very often vivisection is received like a kind of entertainment; there is laughter and merriment, and nearly always there is evidence that most of the students find the proceedings exceedingly 'exciting.'" I fear Mr. Thane can never have been present at any of these demonstrations, and at any rate that the 28 experiments out of the total of 19,084 which were witnessed by him in 1903 did not include any of those described in the "Shambles of Science." Medical students are not notorious for dignified seriousness, and though I have no intention of suggesting that the infliction of pain and suffering would in itself give rise to merriment, I think the general taste for fun and rowdyism in medical students is sufficiently well known to justify the sentence quoted. In 1884 the "Medical Times" referred to the London medical student as "a little wild, a little rough, and guilty perhaps of occasional lapses from virtue." Though I believe much improvement may be found in the present-day students, there is certainly still room for more virtue. Our experience that the students find the proceedings exceedingly exciting has been shared by many, as for instance the late Professor Bigelow, Professor of Surgery in Harvard University, who said: "Watch the students at a vivisection. It is the blood and suffering, not the science, that rivets their breathless attention." And the late Sir Benjamin Ward Richardson, who said about demonstrations before students: "Intellectually, I do not think my classes were assisted, in the main, by the experimental demonstration. I am sure it limited my sphere of usefulness, by leading me, in the limited space of time at my command, to omit some parts of physiology of a simpler, less controversial, and more useful kind. I am bound to say, too, that, morally, I did not recall the effect as produced all that could be wished" ("Biological Experimentation," p. 144). A statement occurring on page 152 (first edition) of the book was put to Mr. Thane, and considered by him to be a misrepresentation. The statement is the following: "In the pain-tortured bodies, trembling under the sharp steel bathing in their own blood, and in vain trying to tear the straps that fasten them to their crosses of agony, twitching under the piercing currents of artificial electricity, there is no trace left of the normal life in the nerves, in the blood, in the wonderful co-ordinated mechanism of the internal organs." This passage should be read and quoted in connection with the following: "Or, when the nervous system, with its myriads of delicate cells, the powers and innermost functions of which offer such insolvable mysteries to our outer senses, is injured by curare, morphia, ether or chloroform, how can we under such conditions get the faintest idea of their true nature?" For these two paragraphs were written to show that pain and anæsthesia are equally unsatisfactory physiological conditions, an assertion which the recent attempts of physiologists, notably of Professor Pawlow's school, at producing normal conditions fully bear out. In the work by Professor Pawlow, which I have quoted before, there are such opinions expressed which entirely tally with the opinions expressed in these two pages in the "Shambles of Science." Professor Pawlow says on p. 15 in his book "The Work of the Digestive Glands" (in dealing with operative technique in animal experimentation): "Such a discussion of operative methods appears to me necessary, chiefly because it becomes more evident every day that in the ordinary method of the so-called 'acute' experiment carried out at one sitting, and complicated by free bleeding, many sources of error lie concealed. The crude damage done to the integrity of the organism sets up a number of inhibitory influences which react upon the functions of its different parts. The body as a whole, in which an enormous number of different organs are linked together in the most delicate fashion for the performance of a common and purposive work, cannot in the nature of things remain indifferent to forces calculated to destroy it."

7599. (*Sir William Collins.*) On that point with regard to anæsthesia which you have just mentioned, I do not think you have disputed in your evidence that you regard chloroform as a true anæsthetic?—I regard chloroform as a true anæsthetic if used in the proper way.

7600. Both in men and in dogs?—Yes.

7601. In the lower animals generally?—Yes.

7602. Although you have some doubt as to morphia,

you consider that chloroform may be regarded as a true anæsthetic?—I do not consider morphia an anæsthetic at all.

7603. You regard it as a narcotic?—Yes.

7604. Would you say that narcosis would be an improper term to apply to the conditions of a patient under chloroform?—The two terms are frequently mixed in a very unfortunate way; people often mix the two terms and use "narcosis" when "anæsthesia" should be used.

7605. I am sure that you are well able to give us a good definition of each?—I should say that the difference between narcosis and anæsthesia is that narcosis is an artificial condition of sleep in which pain is abolished to a certain extent but not altogether, whereas anæsthesia is a condition where pain is entirely abolished and consciousness is entirely lost; which is not the case in narcosis—consciousness is not entirely lost then.

7606. Then you include in your definition of narcosis a presumption that sensation is not wholly lost?—Yes.

7607. So that in all your references to narcosis we must read in that presumption?—It is all a matter of words. Narcosis may be pushed perhaps so far that it verges on anæsthesia; but those differences which I have indicated I think hold true and are the real differences.

7608. But when you refer to narcosis or narcotic conditions you imply by that a condition in which pain is not lost?—A condition in which pain is not totally abolished.

7609. Sensation is not lost?—Exactly.

7610. (*Chairman.*) Is not tobacco often called a narcotic?—Yes, but not an anæsthetic.

7611. (*Sir William Church.*) You have just said that, if pushed, narcotics are anæsthetics?—No, I have not admitted that. I have said that the two terms are frequently confused, and that it is thinkable that narcosis may be pushed sufficiently far to approach what we call anæsthesia; but I distinctly see the difference between the two, and I am not prepared to admit that they are identical.

7612. (*Sir William Collins.*) Do you mean that it is not a difference of degree, but a difference of kind?—Yes, it is a difference of kind.

7613. And that the discriminating difference is that in the case of narcosis sensibility to pain is not lost?—Narcosis is induced by drugs which have not the power of producing perfect unconsciousness; it is produced by drugs which have other effects.

7614. I understood when you kindly gave me a definition of narcosis, that you held that it included within it that abolition of sensibility to pain had not taken place?—I said certain pain. I believe I said that pain in narcosis is abolished to a certain extent, but not totally abolished.

7615. But that sensibility to pain in some degree is always present in a condition of narcosis?—Yes.

7616. So that it would not be right to use the term narcosis in speaking of any condition in which sensibility to pain was in any degree retained?—No; but I must add that this would be my definition of the term, because when one reads the literature on the subject one frequently finds the terms confused. People often talk about narcosis by chloroform and narcosis by ether; and according to the definition which I have just given, that use of the term would be inadmissible.

7617. I was rather anxious to know what you meant when you used the term narcosis?—I can only tell you what I believe.

7618. At any rate, you agree that chloroform and possibly other substances are anæsthetics; I suppose ether would fall in the same class?—Yes.

7619. It is possible to abolish entirely sensibility to pain both in man and in lower animals?—Yes, I quite agree.

7620. Presuming that a vivisectional experiment was carried out under chloroform or ether or one of those true anæsthetics and under conditions in which the anæsthetic was faithfully and continuously applied throughout the experiment, what moral or ethical principle in your opinion would be violated by such experiment?—In the first place, I do not believe that vivisectional operations generally can be carried out under deep surgical anæsthesia, for the reasons I have already stated in my evidence; there are scientific objections, of which I have given instances. Secondly, my moral objection to vivisection is the whole principle of exploiting the lower animals for our supposed service and for our use. I take my stand as an opponent of vivisection rather on that ground than solely and entirely on the ground of pain.

7621. Then your answer, I understand, is twofold. The chief head of your objection is to the exploiting of the lower animals in the interests of man?—The two answers to your question will go together.

7622. I thought you stated that the latter was the more important?—I said that I did not believe but I knew, that vivisectional operations frequently required conditions which cannot be described as perfect surgical anæsthesia; and of that I have given instances to the Commission. And then I said in answer to your question whether I should object to an experiment on a living animal supposing it was unconscious, that I should object to it all the same; because I take my stand against vivisection not only and entirely on the ground of pain, but on the broader humanitarian ground of objecting to the general exploitation of animals for our use.

7623. Do you doubt that operations under what you call surgical anæsthesia are painless?—No; in some cases consciousness may return, but I could not go so far as to say that it frequently or often happens.

7624. That is to say, in a vast number of cases of surgical operations under what you call surgical anæsthesia, the operation is painless?—Yes.

7625. Then I understand that you simply do not believe those who state that vivisectional experiments on animals can equally be carried out under anæsthesia?—I cannot believe it, because I have facts to the contrary based on all the literature on the subject.

7626. (*Sir John MacFadyean.*) You have already made it perfectly plain in answer to previous questions, that you are absolutely opposed to experimentation upon animals in any shape or form?—I have.

7627. I should like to ask whether you are ready to admit, or on the contrary would deny, that anything useful for the cure or prevention of human or animal disease has ever been discovered in that way?—That is a question which it is very difficult to answer; it is all a matter of relative terms. What one considers useful another person might not consider useful. Personally, I believe that the abolition of vivisection will be accompanied by great changes and great development in the whole science of medicine; that new methods of healing will come in, and higher methods, as we know that the coarser medication and the coarser drugging are going out of fashion. From my own point of view, I am prepared to say that nothing which I should call useful has been obtained by experiments on the lower animals.

7628. Do you yourself attach great importance to your own opinion on the question of therapeutics?—I do not attach more importance to my own opinion than the average person does to his. When I am asked for my opinion I can but give it.

7629. I mean as compared with the average specialist on these subjects?—If you ask my opinion as a lay person, I give you my opinion as a lay person.

7630. That is just what I wanted to know; whether you offer it as a lay person?—Yes, I offer it as a lay person.

7631. I really did not know that, or I should not have asked you the question at all. But I will ask you to assume as a fact supported by an immense body of evidence, that discoveries which are valuable for the cure and prevention of human and animal diseases have been made by experimentation on the lower animals; and then I want to ask you this other question: Do you think it right to continue to take advantage of those useful discoveries?—First of all, I do not admit—I cannot admit—as I said, based on my previous answer, I cannot assume, that such inestimable advantages have been gained.

7632. You must not read more into the question than I said. I did not say inestimable advantages?—I beg your pardon: I did not mean to do so.

Miss L. Lind-af Hageby.

1 May 1907.

7633. I did not say whether large or small; I asked you to assume, which does not mean that you are to assent to it. I assume from what you told me before that your knowledge is not wide enough to entitle you to form an opinion; but it is reasonable to ask you to assume that there is a great deal of evidence which many people accept as proving that useful discoveries have been made, and I wanted to know—it is an ethical question bearing on the justifiability of experimentation—whether you think it is right that we should continue to make use of these discoveries in practical medicine. I ask you to assume that for the sake of argument?—Assuming for the sake of argument that such advantages have been derived by vivisectional experiments, personally I should never make use of anything which I consciously knew had been derived by such means.

7634. I will ask you to assume as one of these useful discoveries, the discovery, for instance, of tuberculin, which many people think is calculated to play a very useful part in exterminating or reducing tuberculosis among the lower animals. Do you really think it is immoral on the part of farmers to employ tuberculin, or would you actually discountenance it; would you be opposed to the Government compelling the use of tuberculin, for instance, as a means of suppressing tuberculosis?—Tuberculin is one of the lasting monuments to the failure of drugs obtained through experiments on living animals. The terrible Koch fiasco a few years ago——

7635. You misunderstand me?—No; may I finish? When tuberculin was sent out into the world as a remedy for human beings and finally was discarded as a remedy, it was a great fiasco.

7636. But really I must protest again. I asked you to assume as a matter of argument that the tuberculin was really useful?—I cannot assume that.

7637. I did not wish you to give evidence to the Commission on the value of tuberculin as a diagnostic for tuberculosis?—You chose an instance which was too tempting to leave alone—that was, the fiasco of tuberculin.

7638. Will you forgive me for saying that I do not care in the least for your opinion about the value of tuberculin, but I wanted to know whether, assuming it for the sake of argument to be of great value, you think it would be right to take advantage of it, or whether you would actually deprive farmers of the advantage which they can derive from the systematic use of tuberculin?—Then I cannot assume that with regard to tuberculin without saying that I consider it a great fiasco. It was a great fiasco with human beings, and I do not believe that it is valuable as a test for tuberculosis in cattle.

7639. I am sorry that I am the first person who has put to you a question which has failed to get an answer When you make use of the term "exploiting animals for human use," would you explain very shortly to the Commission what you understand by that expression. Is the keeping of animals in confinement—what we call the domestication of animals—covered by the expression exploitation of animals for human use?—No, it need not be.

7640. Is the killing of animals by methods involving avoidable pain in order that their flesh may be used for human food, exploiting them for human use?—I answer that killing animals for food by methods which involve pain is certainly exploitation of animals.

7641. Are you a vegetarian?—I am a vegetarian.

7642. On that account?—On that account.

7643. You say that you believe that all experiments on animals could be dispensed with, and that science would not suffer on that account?—Yes.

7644. Do you mean by that that up to the present time experimentation on animals has provided nothing of any value whatever—that it is absolutely valueless?—I think I answered that question before. I said that it is a matter of opinion as to what is valuable. To my mind vivisection has produced nothing which is valuable; but I am quite aware that there are other minds to which those results are exceedingly valuable. It is all a matter of comparative values, and I believe that the abolition of vivisection, when these methods are discarded, will be followed by new scientific methods and new scientific light, and that science need not suffer. Science has immense possibilities.

7645. I must put to you one more concrete case referring to a subject about which I know something myself. I believe it to be a fact that a valuable method of immunising animals against cattle plague was discovered within the last ten years—I mean the use of what is called the serum of animals which have recovered from cattle plague or have been hyper-immunised against it. The belief which I hold is shared by, I think, almost everybody who has examined the evidence, or had an opportunity of studying the thing practically. The method has been employed on an immense scale, and is believed to have saved millions of money. Will you tell me whether you think it is in the least likely that that discovery would have been made without experimentation on animals? It involved a great deal of experimentation on animals?—I should not dream of denying that that method has been discovered by experimentation on animals; in fact, that is something that we all know; but the point is whether some other method might not have been employed and could not have been employed with equal advantage. I believe on the whole in natural immunity versus artificial immunity, and I am not at all inclined to admit that the principle is a true or right one.

7646. But when you speak of natural immunity, are you aware that cattle plague was threatening to devastate the whole of the animals in South Africa; there was no time to provide a new breed of cattle with natural immunity. Will you give us some suggestion as to an alternative that might have been used?—I believe that probably there was some cause of that outbreak of cattle plague which could have been avoided. I believe in methods of prevention by sanitation above all.

7647. But it was too late to avoid it; it had been introduced into the country?—I do not know anything of the particular case which you are quoting, but I think that there is enough in the literature of the failure of sera and the whole principle of serum-therapy to warrant very strong opinions against the introduction of unnatural immunity.

7648. That is all the answer that you are prepared to give with regard to that question?—I believe that slaughter and isolation have been proved to be the best remedies in cases like that.

7649. Do you seriously suggest that it would have been better to refrain from attempting to immunise cattle in South Africa, and to have killed them all off?—I have no evidence that the cattle were immunised.

7650. I ask you to assume that, and I think you will not fall into error if you assume that immense numbers were immunised.

EIGHTEENTH DAY.

Wednesday, 8th May 1907.

PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. HENRY MORRIS, F.R.C.S., called in; and Examined.

Mr. H. Morris, F.R.C.S. — 8 May 1907.

7651. (*Chairman.*) You are the President of the Royal College of Surgeons of England?—Yes.

7652. And you have been desired by the Council of the College to give evidence before the Commission?—I have been.

7653. The Council, when a letter was read from the Commission, I believe, discussed it, and agreed to a resolution. They discussed it, and agreed to the resolution contained in my précis.

7654. Would you just read us that resolution?—"The Council of the Royal College of Surgeons of England express the opinion that the Act of 1876 is sufficient protection against any abuse of vivisection, and earnestly hope that the progress of surgical science will not be hindered by further restrictive legislation."

7655. You were not present, probably, at the Royal Commission on Vivisection in 1876?—No.

7656. And your evidence is relating to the advance in surgery and to its relation to experiments on living animals since 1876?—Yes. I may say that I have read the evidence of 1876—perhaps not entirely, but very nearly from cover to cover—so that I know the nature of the evidence of 1876. But this evidence of mine is based upon material facts and discoveries which have occurred since 1876.

7657. You propose in your evidence to refer to the advances in surgical knowledge which have been made, or in part made, since 1876 by experiments on animals—(1) As to the scientific and practical conditions essential for the proper healing of wounds; (2) as to the precise causes of many diseases the real nature of which was previously quite unknown to the profession; (3) as to the immunising or curative influence in certain infective diseases of the sera of immunised animals; and (4) as to the importance at the present time of experiments on animals for carrying on research as to the nature and origin of cancer in man and in animals?—Yes.

7658. Experiments on animals in relation to aseptic surgery and the healing of wounds have been of material advantage, in your opinion?—Most material. I quote Lord Lister. We could not have arrived at the knowledge we possess at the present day had it not have been for experiments on animals.

7659. You do not say that that knowledge has arisen from experiments on animals alone?—No.

7660. But there are other modes of investigation which have been going on in research?—Certainly.

7661. Chemical investigations, for instance?—Yes, each of the forms of investigation has been essential, but experiments on animals have been equally, if not more essential or important than the other forms of investigation.

7662. And it is not easy, in your opinion, to separate the advantages which have been derived from each particular source?—It is impossible.

7663. It is your decided opinion as an experienced surgeon that vivisection has been essential?—I have no hesitation in expressing that opinion.

7664. I use the word vivisection, you understand, because everybody uses it. Experiments on animals are not all what we should properly call vivisection, but I use it as meaning experiments on animals?—Yes. Mr. Lister (now Lord Lister), in giving evidence before the Royal Commission in 1876, stated that he could not have made his way in the subject of antiseptics without the assistance he had derived from experiments on the lower animals. It was by means of experiments on animals whilst studying the phenomena of inflammation that Lister ascertained many very important and fundamental facts. Thence he went on to the study of the blood, more especially the coagulation of the blood, thrombosis, etc. The facts ascertained in both these directions formed the basis of his subsequent work on the treatment of wounds. Lister, having come to the conclusion that the disasters which attended wounds were directly dependent on putrefaction of the discharges in the wounds, directed his attention to attempts to prevent this putrefaction, but for a long time without any marked success, because the causes of this putrefaction were unknown. It was after Pasteur had shown that putrefaction, like other fermentations, is caused by microbes growing in putrescible substances, that the problem how to protect wounds from bacteria presented itself to Lister. Lister himself demonstrated in his experiments on lactic fermentation that there must be different kinds of bacteria having different functions. So far, the early beginnings of aseptic surgery were only in part directly associated with experiments on animals.

7665. Pasteur's and Lister's experiments that you speak of were partly on animals?—Yes, both of them were very largely on animals. May I interpose a brief summary of Pasteur's work in three or four lines here?

7666. If you please?—Pasteur's works may be classed in three series—three great discoveries: (1) That each fermentation is produced by the development of a special microbe; (2) that each infectious disease is produced by the development within the organism of a special microbe; and (3) that the microbe of an infectious disease, cultivated under certain detrimental conditions, is attenuated in its pathogenic activity—from the virus it has become a vaccine, and as a vaccine is used for protection and cure.

7667. The important thing for our inquiry is, how far these discoveries were assisted by experiments on animals, and how far experiments were or were not essential?—With regard to (2) and (3) experiments were essential. Pasteur discovered that infectious diseases were produced by different microbes, by experiment.

7668. On animals?—Yes, and it is true that in the first instance it was by an accident he discovered that the microbe of an infectious disease, when cultivated under circumstances which are detrimental to that microbe, converts the microbe and its effect from a virus or poison of the disease into a curative or protective agency against the disease. Those discoveries were made by experiments on animals.

7669. We are not all of us, by any means, skilled

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in surgical or medical phraseology. What is the precise meaning of cultivating a bacillus?—Growing it on different media, like potato or broth, and things of that kind, and then producing the bacillus in multiple from the organism which he put upon that material, and also producing the toxins from it.

7670. You mean that the bacillus, as it were, multiplies itself in this new substance?—Yes.

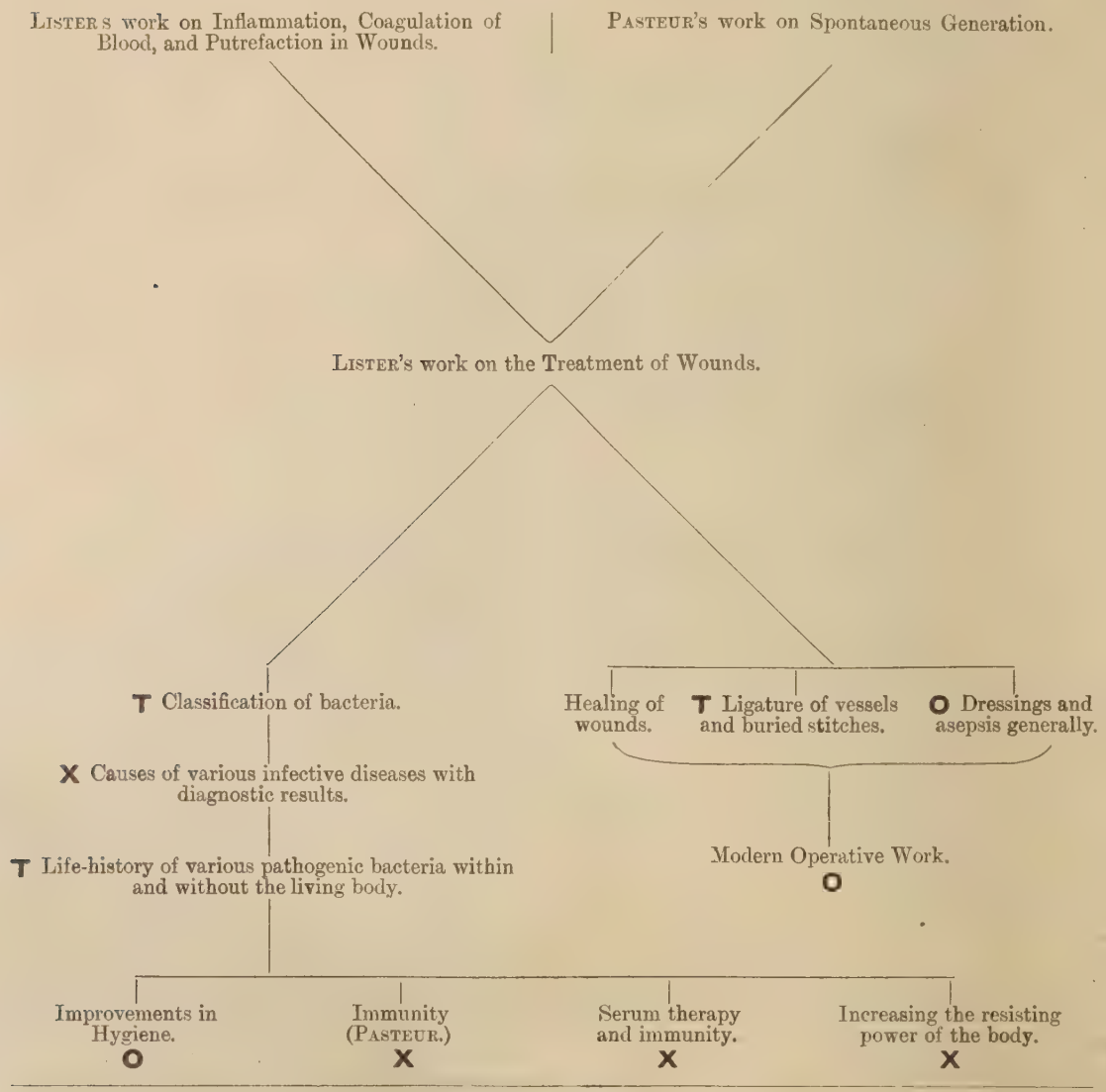
7671. In what way is the bacillus so produced less effective as a poison than the one that is taken out of a living animal?—For instance, this cultivation, if you accept the term now, may be kept for a longer or shorter time, and be impaired in its poisonous effect by being kept; and then when that material is injected into an animal, it does not produce the full poisonous effect upon that animal, but it produces a modified condition, which prevents it from taking the disease.

7672. That is to say, the virus is stronger where the bacillus is found in the blood of a living animal than it is in this cultivated substance or mixture?—When it is fresh. Perhaps I may take the case of the chicken cholera, by way of making what I want to convey clearer. Pasteur found out that the poison of chicken cholera, after it had been kept some considerable time, did not produce the disease when injected into chicken, and that in those chickens which had that old material injected, when submitted to fresh and more active cultures did not develop the disease; whereas other chicken which had not been previously treated with this old culture did develop the disease, and died after being injected with the fresh culture. Does that convey to you what I mean by one of Pasteur's great discoveries? Then, besides that, take, for instance, the tetanus bacillus being taken from a dog which died of tetanus. If that is introduced into other dogs, it produces the disease; but under certain circumstances, which I have mentioned in my *précis*, the poison becomes so weak that it can be injected without producing the virulence of the disease. It is the same with anthrax. The anthrax bacillus has been submitted to various altered conditions by Professor Sclavo and others, and this, when introduced into animals, serves as a means of preventing the disease or of curing it, instead of developing it in its full force.

7673. Then you have a table, which will appear here in your evidence?—If you please.

LISTER'S WORK AND WHAT HAS FOLLOWED FROM IT.

(Drawn for me for the purposes of this Statement by Mr. W. Watson Cheyne, F.R.S., C.B. Member of the Council of the Royal College of Surgeons.)


Lister's work on Inflammation, Coagulation of Blood, and Putrefaction in Wounds.
Pasteur's work on Spontaneous Generation.
Lister's work on the Treatment of Wounds.
T Classification of bacteria.
X Causes of various infective diseases with diagnostic results.
T Life-history of various pathogenic bacteria within and without the living body.
Healing of wounds.
T Ligature of vessels and buried stitches.
O Dressings and asepsis generally.
Modern Operative Work.
O
Improvements in Hygiene.
O
Immunity (Pasteur.)
X
Serum therapy and immunity.
X
Increasing the resisting power of the body.
X

Explanation of Signs.

O Profoundly influenced by knowledge obtained by experiments on animals, especially those leading to the diagnosis and life-history of bacteria.
X = Impossible without experiment on animals.
T = Dependent to a great extent on experiments on animals.

The above "tree" indicates the development of Modern Bacteriology from Lister's work on the treatment of wounds and the relation of the various researches to experiments on animals.

I would say that as the result of the most intense study and work, Lister constantly improved his methods of wound treatment, with the most remarkable results, in the prevention of septic diseases, as well as of certain diseases which at first he had not looked on as due to the entrance of bacteria, such as erysipelas and tetanus; the latter had not previously been thought to be associated with decomposition in wounds. But knowledge of these matters was only increasing slowly and imperfectly till the appearance of Koch's work on the infective diseases of wounds. Koch showed, entirely by experiments on animals, that there are two great classes of bacteria—viz., those that cannot grow in the living tissues—the non-pathogenic bacteria; and those that are able to grow in the living tissues, and growing there produce various forms of disease. Koch further showed that the troubles which arose in connection with wounds might, on the one hand, be due to the absorption of the chemical products of the growth of one or both of these kinds of bacteria in the discharges of the wound; or, on the other hand, to the entrance of pathogenic organisms into the tissues, and their growth in the body. In this way he demonstrated the occurrence of septicemia in mice, due to small bacilli; septicemia in rabbits, due to cocci; pyemia in mice, due to micrococci, and many other things of the same kind.

7674. What exactly is the distinction between a bacillus and a coccus?—A coccus is a small round body, and a bacillus is a rod shape or a dumb-bell shape or a drum-stick shape. They are exceedingly microscopic, of course.

7675. Are these experiments on mice and rabbits applicable to septicemia in man also?—Yes, septicemia in man and the cause of septicemia in man have also been demonstrated and proved accurately by the experiments on mice and rabbits.

7676. What is the connection between discovering septicemia in man and in animals; how is it arrived at by these experiments on animals?—By cultivating the organisms which are found in the discharges or the blood of men suffering from septicemia or pyemia.

7677. Would you not find it in the blood of the men themselves, supposing you made an incision in them?—You may find it there, as has been the case in many diseases in animals. You may find organisms and appearances constantly in the blood or the tissues, but you have not the proof that those are the causes of the diseases until experiments have been made on animals with these materials or organisms, and the same disease has been developed in the animals so inoculated or treated. For instance, if you take the tubercle bacilli found in the tubercle nodules of the human being, and cultivate it, and inject the cultivated material into animals; or even, without cultivating the bacilli outside the body, if you inject the bacilli direct from the urine or from the blood of man, you can prduce tubercle in the animal. But it had to be proved that the disease was due to the bacillus by experiments on animals.

7678. That is to say, you might find that particular bacillus in the blood of a man who had a particular disease, but you could not be sure that that was the cause of the disease without experimenting on animals?—That is so. I was going to say that in that particular disease of tuberculosis, many years before it was identified as being due to the presence of tubercle bacilli, it was known to be an infectious disease, and due to some material in the animal or the person suffering from it.

7679. You go on, in your *précis*, to Koch's work?—Koch established three great things; he laid down three great postulates which have a bearing on this whole subject. These were, that you must find the microbe of the disease, and then, having found it, you cultivate it in appropriate media outside the body and inject it into animals, and then reproduce the same disease over again in those animals so as absolutely to identify the disease with the particular organism by recovering it in the tissues or fluids of the injected animals.

7680. What are the three postulates? You say the third of the three postulates necessitates experiments on animals, but you have not told us what they are?—The first is that you must discover the micro-organism which is the cause of the disease, the coccus or bacteria as the case may be, in every case in which the disease exists. Then, by making a pure culture, separating it from everything else, and making the pure culture of this particular organism, you introduce it into an animal, and it produces the disease there; and then you will find in the blood and tissues of that animal the same organism as you found originally in the individual who was attacked by the disease.

7681. That is the third postulate?—Yes.

7682. The third of these you say necessitates experiments on animals?—Certainly.

7683. I was going to ask, does not the second also? No. Not the cultivation of the organism outside the body.

7684. When you say that experimentation is essential, can you suggest any other means by which it could be done?—No.

7685. Based on these postulates, the discovery was made of the bacteriological origin of a great variety of diseases, both in man and in animals—*e.g.*, tubercle, tetanus, diphtheria, all sorts of wound infections, such as septicemia, pyemia, sapremia, erysipelas, and infective gangrene, as well as gonorrhœa, plague, cholera, pneumonia, and so on.

7686. (*Colonel Lockwood.*) What is sapremia?—It is a blood poison. It is the name given to that form of blood poison where the intensity of the disease and the exhibition of the poison depend upon the amount of the poison; so that, if you have a mild quantity of the poison you get a mild form of the disease, and by increasing it you get an increasing disease. It is known as "septic intoxication."

7687. (*Sir William Collins.*) Is it not a form of blood poisoning that is independent of any micro-organism?—No.

7688. You would not say that?—No, I would not.

7689. (*Chairman.*) The particular bacillus which is responsible for each of those diseases you say has been discovered by experiments on animals?—Yes, and identified and proved to be the cause of the disease.

7690. (*Colonel Lockwood.*) And classified?—Yes, more or less classified into ærobic and non-ærobic, if you understand those terms; and again classified into those organisms where the poison is contained within the body of the micro-organism or is outside the actual body; those where the animal itself causes the disease and those where what is called the toxin or an emanation from the micro-organism causes it.

7691. (*Chairman.*) Tetanus organism, for example, is found in the ground?—Yes, it is one of those that does not live in the air, and is called anærobic therefore, and its poisonous effect is produced by its toxins rather than by the bacillus itself.

7692. When you say that the poisonous effect is caused rather by its toxins than by the bacillus itself, what is the distinction between the bacillus and its toxin?—The bacillus is the organism, but the toxin is some emanation from it.

7693. Is not the way that all bacilli work by their toxin?—No, some of them work by themselves.

7694. Without emitting any toxin?—Yes. As you have asked on this point, I think I could give an illustration, for instance, of the importance of classification of bacteria, and how experiments on animals prove the importance of it in identifying different bacilli in the blood of a diseased animal. Doubts were thrown upon the accuracy of Pasteur's work in reference to anthrax, and certain animals died, which were said to have died from anthrax, but not from septicemia. Pasteur, knowing that the bacillus of anthrax is an ærobic organism, and that the bacillus of septicemia (or using the word blood poisoning in that sense) is an anærobic organism, cultivated from discharges or blood the anthrax bacillus, when exposed to the air, and not the septicemia bacillus, and the septicemia bacillus in an atmosphere of carbonic acid, and not the anthrax bacillus.

7695. What do you mean when you say, "And not the anthrax bacillus"?—From the same fluid he was able to cultivate one bacillus in a culture exposed to the air, namely, the anthrax bacillus, and the other bacillus, namely, the septic vibrio of septicemia, only from a culture exposed to an atmosphere of carbonic acid; and then by taking those separate products of the cultivations he produced the anthrax disease by the one material and the septicemia by the other material,

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in different animals. Thus, by the knowledge that the septicemia organism was anærobic and anthrax bacillus was ærobic, he was able to by these different modes of cultivation to get each of these organisms separately from the same blood; and then with each of these organisms separately he produced the corresponding disease in a separate and isolated form.

7696. That is to say, each can only live and multiply in its own element?—Yes.

7697. The ærobic bacillus, you mean, is one that lives in the air?—Yes.

7698. And the anærobic, either in the body or in the ground?—Yes, tetanus, for instance, is an anærobic.

7699. But if it is in the body of a human being?—It will live in the body of a human being—the tetanus bacillus will do that; but outside the body it is existing and persisting in the earth, and in dust, and so on, where it is not exposed to oxygen.

7700. You gave us the particular bacillus that you said was an ærobic bacillus; what was that?—The anthrax bacillus. The septicemia bacillus is an anærobic bacillus.

7701. What is a specimen of an ærobic bacillus?—The septic vibrio of septicemia. I only bring that matter forward to show you the importance of these experiments on animals; that by the introduction of pure cultures of these separate micro-organisms you are able to absolutely identify the nature of the disease of the body from which it is taken, and to produce that same disease again in the animal which is inoculated.

7702. These are experiments in which the only operation consists in inoculating?—Yes, they are inoculating experiments.

7703. With a lancet or syringe?—A syringe.

7704. And any suffering in the animal is a suffering of the disease, really?—Yes.

7705. You go on to speak of the value of experiments as regards improving methods of treatment and prevention?—Yes. Of course, the whole of the aseptic and antiseptic methods of the treatment of wounds is the outcome of this knowledge of the micro-organisms in the putrefective discharges being the cause of the disease. The discovery of the causes of these diseases has led to the early and precise diagnosis of them, and to a more intelligible and effective manner of treatment than was previously possible. In the prevention of the spread of diseases and in hygienic science, advances have been made, owing to experiments on animals, which could not have occurred had such experiments been prohibited. Further, the study of bacteria, and their effects on animals under various conditions, led Pasteur to open up the whole question of immunity. In the course of his experiments on chicken cholera, Pasteur wished to inoculate some fowls, and for this purpose took a cultivation of the bacteria of chicken cholera, which had stood in a flask in his laboratory for some weeks. He found that the animals so inoculated did not acquire the disease as they ought to have done, and on re-inoculation of the same with a fresh cultivation, he further found, to his surprise, that they did not develop the disease; while fresh fowls, which had not been previously subjected to the first injections, did. In other words, he had protected or immunised the fowls by the first inoculation, although they had not shown any symptoms of the disease. This accidental discovery led him to obtain and employ attenuated cultures, which could reproduce the disease in a form having attenuated virulence, and at length a veritable vaccine to completely protect against the virulence of the disease. ("The Life of Pasteur," by Rene Vallery Radot, vol. II., p. 100-1.) These discoveries have proved of great benefit both to animals and human beings, in proof of which it is only necessary to name anthrax, swine erysipelas, and rabies. "Rouget of Swine" (Pasteur's Life, p. 125, vol, II.). The experimental study of pathogenic organisms has also led to the discovery of protective and curative sera of proved value, and of great importance in treating a number of diseases, the most striking instance being diphtheria. The marvellous improvements which have been made since the Royal Commission of 1876 in the treatment of wounds, and in our knowledge (a) of proper disinfection; (b) of the sterilisation of dressings and instruments; (c) of the action of antiseptics on wounds; and (d) of the behaviour of various substances used as ligatures, when buried in the tissues, are due to the precision and clearness with which the nature, the virulence, and the habitats of the various pyogenic organisms have been made out.

7706. When you say buried, do you mean temporarily or permanently buried?—Some of them have been permanently and some temporarily. The old plan of ligaturing was very frequently a disastrous one, because it entailed the application of a silk thread to a vessel, and it was left projecting from the wound. Suppuration was very common in those days, and in the process of separation of the ligature and the ligature coming away, as it was spoken of as doing, secondary hæmorrhage frequently occurred. Improvements were being sought, and various methods were employed, such as a cu-pressure, tortion by many long needles passed in, and twisting the vessels round, to do away with the danger of having these projecting ligatures; and then it went on to the employment, through Lister's experiments, of buried ligatures, using various material, such as ox-aorta gut, cat-gut, and silk.

7707. Which need not be taken out?—Which were tied, and the ends cut short, and left to be surrounded and grown into, or absorbed by the tissues.

7708. I suppose the important thing was that those should be in themselves aseptic?—Absolutely.

7709. And could be absorbed?—Yes, either absorbed or encysted, many of them being absorbed. That was a matter of the very first importance in connection with doing away with the liability of suppuration of wounds and of secondary hæmorrhage. By getting an immediate union, a union by first intention, as we call it, of the wound, these things round the vessels, cut short and left in the tissue, became buried and shut out from all liability of doing harm, which ligatures, hanging tail-like out of the wound, so often caused.

7710. Were these ligatures, which could be safely left in the wound, ascertained by experiments on animals?—Yes.

7711. It was partly the method and partly the materials that was of importance?—Yes.

7712. As regards the materials, would those be employed?—Yes, they were used upon animals in ligaturing the vessels of animals and so on; but also, you see, the connection with experiments on animals in this matter is in the healing of wounds by the discovery of the causes of the suppuration of wounds and the causes of septicemia, pyemia, etc. It all steps together. The experiments led to the knowledge of the preparation of these litagures, as they led to the knowledge of how to get the wound to heal without supperation.

7713. (*Sir William Church.*) The importance of the experimentation on animals was that it demonstrated what were the sources of dangers in these ligatures?—Yes.

7714. (*Chairman.*) All that arose on the question I asked you about the word "buried." Perhaps you will go on about the operations?—I have made it clear that they are simply buried in the wound.

7715. Yes?—Since the danger of septic troubles has been removed, enormous strides have been made in surgery in recent years. Many operations which used to be looked upon as too dangerous to be undertaken, except under stress of necessity or as offering a last chance of saving life, are now performed with absolute safety; many new and important ones, which were formerly never dreamed of, have now become matters of successful and almost daily performance. Finally, whereas the mortality, even after quite simple operations, was at one time so appalling that the patients were often deterred from submitting to, and the surgeon from performing them, operations of considerable magnitude are now undergone with the confidence which comes from knowing that the mortality is, in many instances, but a fraction of an unit per cent. To-day the abdominal cavity of man is opened, every organ within is inspected and palpated for diagnostic as well as for operative purposes, the wound is closed, and the patient recovers from the effect of the anæsthesia, and takes a completely uneventful course towards convalescence. This course is very commonly without pain, without mental shock or distress, without discomfort of any kind beyond some anæsthesia sickness, some flatulence, and the weariness of

lying still. Within 10 or 14 days after such an operation the patient has generally quite recovered.

7716. In what way have experiments on animals assisted to that result?—Through the rapid healing and the healthy condition of the wound from the very start to the finish; whereas, before we knew through experiments on animals what the causes of putrefaction and suppuration were, and how to prevent them, the majority of these wounds suppurated.

7717. You mean Lister's and other's experiments with regard to aseptic treatment?—Quite so.

7718. (*Mr. Tomkinson.*) What does palpated mean?—Handling—taking hold of with your hands—examination by touch, I would rather put it; palpating is feeling with one's hand. You can examine an organ by sight as well as by handling. Or, an incision is made into the loin, the kidney is exposed and freed from its surroundings, is drawn out upon the loin, inspected, palpated, incised and examined by the finger inserted into the wound in the organ, and by a bougie passed down from it to the urinary bladder; the kidney is then replaced, and the patient is well and sitting up in 10 or 12 days afterwards.

7719. (*Chairman.*) We have heard a good deal about operations, not precisely the same, but somewhat of this nature, as regards the laying open of the body in animals. Of course, all those operations, whether upon man or upon animals, are, or should be, under anæsthetics?—Yes.

7720. You have told us what your experience is as to the amount of pain suffered by man. Would the pain suffered by animals under such an operation be, in your opinion, the same or greater or less than that suffered by man?—Allowing that an animal is equally susceptible to pain as man, there would be equally the same immunity from pain and suffering, provided that the aseptic condition after making the wound was kept up.

7721. And provided that in each case the anæsthesia was complete?—Of course, so far as the operation is concerned.

7722. I mean during the operation?—Yes, but not the subsequent course. I am laying emphasis here upon the immunity from subsequent pain—the pain during the recovery from operations, which has been such a remarkable and beneficial result of our knowledge of septic diseases derived from experiments on animals.

7723. Supposing an animal were subjected to an operation (that is, perhaps, hardly the right medical term), which involved cutting into the entrails under anæsthesia, would the animal be treated on recovery in the same way as you would treat a man?—Yes, I believe the Act compels that; I think, I am not quite sure, it compels that they should be treated antiseptically, and if they are treated antiseptically after the performance of the operation, the animal, equally with the man, will recover with practically no suffering.

7724. (*Dr. Gaskell.*) Is it not the fact, or perhaps you do not know, that the recovery would probably be quicker than in man?—I do not know that it is quicker, because really the rapidity with which some of the wounds in the human being heal is so very great that it is almost impossible to be quicker; but probably a great number of wounds upon animals do heal in a shorter time than in man.

7725. (*Chairman.*) I suppose the average constitution of a reasonably healthy animal is better than the average constitution of a human being who is kept alive by so much care very often?—Yes, no doubt.

7726. I think we will not trouble you to read the remainder of your précis, but it will be printed as part of your evidence?—If you please. The *précis* which deals with the subjects classified in the answer to Question 7657 continues:

A joint is the seat of a sudden and most painful inflammation which is mistaken for an attack of acute rheumatic fever; it is nowadays punctured, the fluid is withdrawn and examined, it is found to be septic, and the joint is then at once laid open and drained; immediately all pain vanishes, and the patient quickly recovers with a moveable and natural joint, instead of, as formerly, after a lingering illness if at all, and with a stiff joint, a deformed limb, and suppurating sinuses discharging on the surface of the body.

The disease technically known as osteomyelitis or diffuse abscess within bone is no longer looked upon as due to exposure to cold, or as rheumatic; nor is empyema, or suppuration in the chest cavities, any longer attributed to access of air to the pleural surfaces; but both diseases are alike treated by the freest incisions and drainage, with the result that great suffering is relieved and thousands of lives which formerly would have succumbed to these diseases are saved.

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The foregoing are some illustrations of the beneficent results of the combined labours of Lister, Pasteur, and Koch.

Further, I may refer to the recent work of Wright and others on opsonins, which holds out some hope of extending our knowledge as to how to increase the resistance of persons against or how to cure them of such diseases as tubercle, pyorrhœa, and the various diseases caused by staphylococci, such as boils, impetigo, gonorrhœa, etc. Wright has found, and Koch had previously found, that the introduction of the dead bodies of bacteria into animals increases the resistance of the animal to the disease produced by these bacteria, and he has devised a method of ascertaining the resistance of an individual to a particular pathogenic organism. The results show that by proceeding by his methods the resistance of an individual may be increased to such an extent that he is enabled to throw off the disease in a way that he otherwise was unable to do.

In various septic diseases sera have been introduced which are of considerable value. Of these may be mentioned anti-streptococcus serum. At first the results of the use of this serum were disappointing and contradictory. In some cases it seemed to exercise no benefit, while occasionally surprisingly good results were obtained. Further study of the streptococci has shown that there are many varieties of these organisms, and that the serum derived from one variety may be useless against another. This led to attempts to immunise an animal against a number of different varieties of streptococci, and these attempts have in turn led to improved results.

Quite recent results have been published by Fenwick and Parkinson of the beneficial use of rectal injections of an anti-streptococcus serum in gonorrhœal infections, and certain cases of hæmorrhagic purpura. The serum used by them was one made at the Wellcome Laboratories at Herne Hill, and is there known as the "Polyvalent" Anti-streptococcus Serum, in the manufacture of which 43 different strains of streptococci taken from such different diseases as septicemia, puerperal fever, malignant sore throat, acute rheumatism, etc., are employed. (Med.-Chir. Trans. Vol. 89, p. 183, 1906.)

It is expected that this "Polyvalent" serum will remove the present uncertainty of the effects of treatment by other anti-streptococcus sera, and that it may prove an immunising agent against infection in certain operations where aseptic and antiseptic methods are not strictly applicable.

Experiments on Animals in relation to our Knowledge of Shock and how to prevent it.

Since the dread of septic troubles has been removed a very important subject to which surgeons are now directing their attention is the diminution or prevention of shock. Some of the most valuable operations are occasionally attended by the occurrence of severe and even fatal shock. As the result of much experimental work we are improving our methods in the way of preventing this calamity.

The investigations as to the relation of the vasomotor nerves, the medulla, and spinal cord to shock, and as to the influence of injections of supra-renal capsule, of adrenalin and other vascular contractors, all depend on physiological experiments.

Many of the researches on blood-pressure, on the nervous regulation of the blood-vessels, etc., which seem of more scientfic than practical importance, have a direct bearing on this question of shock, and should be encouraged, as they are tending in the direction of helping us to obviate this danger in surgery. All the vascular contractors have been discovered by experiments on animals, and there is much yet to be learnt about such drugs in inflammatory diseases and hæmorrhage (A. R. Cushny).

Other witnesses will not doubt bring testimony to bear on the importance of the experimental study of the action of drugs on the therapeutical treatment of diseases.

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II. and III.

Experiments on Animals in relation to the Bacterial Origin of Certain Diseases, and the Immunising and Curative Influence of Culture Sera.

In illustration of the necessity of continuing experiments on animals if the benefits which are obtainable from them by immunising, preventing, and curing certain diseases in man and animals are not to be checked or altogether to cease, I will mention the following diseases:—

A.—Diphtheria.

The Klebs-Loeffler or diphtheria bacillus was first described by Klebs in 1875, and was obtained in pure culture by Loeffler in 1884. Its isolation from other bacteria which are also found in the mouth was a matter of great difficulty and the work of many years.

Behring and Kitasato in 1890 cleared the way for immunising healthy animals against, and curing infected animals of diphtheria, but it was not until 1897-1898 that Park isolated the particular strain of bacillus and obtained the particular toxin, which was of a power of sufficiently high virulence to produce an antitoxin of regular and standardised strength.

For the continuation of the antitoxin treatment to be possible two sets of vivisectional experiments must go on:—

(1) The inoculation of guinea-pigs to standardise the strength of the toxin and the antitoxin; and

(2) The inocluation of the horse with the toxin; and

(3) The venesection of the horse to obtain the serum or antitoxin.

By No. 1 the minimal lethal dose of the toxin, or poison derived from the pure culture of the diphtheria bacillus, and the minimal quantity of serum required to neutralise the same are ascertained; 10,000 guinea-pigs a year for this purpose have been used in Ehrlich's laboratory, and about 2,000 a year were used at the laboratories on the Embankment for the same purpose.

Animal inoculations are required also for diagnostic purposes, and as a protection of the community against infection from persons convalescent from the disease. It is now the practice in the Metropolitan Asylums Board hospitals, as it has been for several years past in New York, to examine the secretion on the throats of patients who have recovered from diphtheria for the presence of baccilli. Any organisms resembling diphtheria bacillus are tested on animals not only to establish their virulence, but also to recognise that it is the diphtheria bacillus. One guinea-pig is injected simultaneously with a broth-culture of the suspected organism. Another guinea-pig receives the same kind of injection with the addition of a small quantity of diphtheria antitoxin. If the organism is virulent diphtheria bacillus the first guinea-pig will die, whereas the second will be quite unaffected.

At the Metropolitan Asylums Board laboratories the inoculation test on animals is also used for the diagnosis of cases occurring in the hospitals of tuberculosis, glanders, and cerebro-spinal meningitis. The milk supplied to the hospitals of the Board has also been tested for the presence of tubercle by the inoculation of guinea-pigs.

The diminution in the mortality from diphtheria since the introduction of the antitoxin treatment is considerable. There are numerous reports and statistics easy of access which prove the truth of this statement (Clinical Society's Transactions, and Stephen Paget's Book).

As in the case of the animals used for cancer research, so in the case of the horses used for obtaining diphtheria antitoxin, I can from personal inspection testify to the extreme care and attention and the great liberality as to fodder bestowed upon them, and to the absence of any painful or deleterious effect on them as the result of the injection and subsequent bleeding.

B.—Tetanus.

About 1884 Nicolaier showed the real microbic cause of tetanus to be a bacillus in earth and dust, which, when injected into the tissues of animals, produced tetanus in the animals so treated. Rosenbach, about the same time, found the same bacillus in a patient suffering from traumatic tetanus. The identity of the bacillus of tetanus in man with Nicolaier's bacillus of earth was demonstrated in Koch's laboratory in April, 1887. In man the bacillus has been found in the wound-secretion, in the nerves leading from the seat of infection, and in the spinal cord of patients suffering from tetanus. The bacillus has been found also in surface soil, in street dust, and in the dust of old buildings, such as churches, etc.

Though it cannot be said that any one of the tetanus antitoxins has been found to be a curative agent in the disease, the preventive use of tetanus-antitoxin for the immunization of men and animals has given great results (see for the results obtained in the veterinary practice in France, the Med. News, 7th July, 1900; and for the results obtained in a large number of horses in the United States of America, see Med. Annual, 1901).

The preventive use of antitoxin in the human subject was shown in an outbreak of tetanus at Prague in 1899. (Paget's Book.) H. J. Scherck gives a strong proof of the power of antitetanic serum as an immunizing agent in man in relation to the injuries caused by toy pistols during the 4th July or Independence Day celebrations in America. (See "The Journal of the American Medical Association," August 18th, 1906, pp. 500, 505, and 512.)

The modern study of tetanus has done away with the useless operations of nerve-stretching and nerve-dividing, which were based on the fallacious theory that tetanus is an ascending inflammation of a nerve, It has led also to the more frequent, early, and complete excision of the wounded tissues, because it is now known that the bacilli, at the outset, are limited to the tissues in the immediate area of the wound. A certain number of cases of chronic tetanus have been reported as cured by the tetanus antitoxin. Injections into the spinal sheath are reported to have cured three patients out of four; and a fifth case, namely, that of a boy into whose nerves and spinal subarachnoid space the antitoxin was injected, is stated to have been cured. Still, other successful cases have been reported recently. ("The Medical Annual," 1905, p. 80; and 1906, p. 53.)

C.—Hydrophobia.

Rabies is another disease the scientific study of which only commenced four years after the date of the 1876 Act. Though a prolonged search for the specific microbe has not hitherto been attended with complete certainty of success, yet, quite recently, Professor Negri, of Pavia, has found, in 50 out of 52 cases, a micro-organism in the nerve-tissues of animals dead from rabies ("Med. Annual," 1906, p. 418).

Renault and Lannelongue discovered some years ago that rabbits could be successfully inoculated with the saliva of rabid animals, such as the dog, fox, jackal; and later, Pasteur produced the disease in the same way. Though the specific microbe remained unidentified, it was ascertained and put beyond doubt that the essential cause of hydrophobia is a specific virus which can only be reproduced within the living body. Another convincing proof of its microbic origin was established by artificially producing the disease in healthy animals, by implanting in them fragments of brain or spinal cord tissue taken from animals dead of rabies. By transmitting the virus contained in nerve tissue through a series of rabbits, Pasteur was able to obtain a standardised virus of exact strength and having a known period of incubation (six days). He was also able to attenuate the virus in the nerve tissue by drying the spinal cord taken from animals dead of rabies—the greater number of days it was drying the weaker became the virus in the spinal cord. In this manner, he was able to have always on hand cords of different strengths of virus for use in different cases, just as a pharmaceutical chemist keeps on hand the same drug of different strengths. The essence of Pasteur's treatment is to render persons artificially immune before the disease caused by the bite has had time to act. Thus a man who has been bitten by a rabid animal can be immunized by inoculations whilst the disease is still dormant, with a succession of doses of graduated strength until he has within himself enough antitoxin to make him proof against any outbreak of the disease.

It is clear, therefore, that in order to maintain a constant supply of antitoxin, rabbits must be inocu-

lated with the virus to yield, immediately after being killed, spinal cords suitably affected by the disease.

Although up to the present prevention and not cure has been obtained by the antitoxin treatment, yet thousands of human lives have been saved by its immunizing power, and the mortality is absolutely undoubted cases has been reduced to less than 1 per cent. (Stephen Paget). I would like to add here that from a report published in "Annales de l'Institut Pasteur" (p. 411, vol. 1905) for the year 1904, it appears that of 755 persons who underwent the antirabic treatment at the institute only three died. This is a mortality of 0·39 per cent. This mortality will be still further improved if Negri's bodies are found to be the specific cause of the disease, because the suspected animal which has bitten a human being, or another animal, if at once killed and its nervous system examined, will show the presence therein of the micro-organism, and then the immunizing treatment would be commenced at once, instead of waiting to see if the offending animal develops rabies or not. Thus the proof that Negri's bodies are the real cause of rabies would be a discovery of the greatest diagnostic value, because most of the fatal cases which occur during or after the antitoxin treatment have been in persons upon whom the treatment was commenced too late to take effect. Though the detection of the organism of Negri in the nerve tissue of the animal which inflicted the bite would be a matter of microscopical or culture examination, still animal experiments will be required to test whether the organism complies with the three postulates of Koch before it can be admitted to be, or not to be, the cause of the disease. Again, if by an "intensive" modification of Pasteur's treatment a cure is ultimately to be discovered, it can only be by by further experiments on animals.

It is a satisfaction to know that rabies in rabbits is of a painless paralytic kind, and that these animals do not suffer from muscular contractions as men, dogs and other animals do.

D.—Anthrax.

Anthrax (called also charbon, splenic fever, or splenic apoplexy, malignant pustule for the sore at the site of inoculation in man, and woolsorters' disease when the lungs are infected after inhalation of the spores of the bacillus anthracis) occurs in human beings, especially in those engaged in avocations having to do with hides and the wool or hair of animals, and in rag pickers. It affects sheep, cattle, horses, pigs, and goats, and is transmissible to guinea-pigs, rabbits, mice, and sparrows. It spreads amongst animals by means of the germinating spores of the bacillus which attach themselves to plants and grass, and which, when taken in with the food, become the cause of intestinal anthrax. Pasteur showed by injecting guinea-pigs with particles of earth, how flocks were exposed to infection, merely by grazing over certain fields. Anthrax was the first of the infectious diseases definitely proved to be of bacterial origin. As long back as 1849, little fine rod-like bodies were first observed in the blood of sheep dead of anthrax; in 1863 Davaine first proved that these rod-like bodies were living organisms (which he called bacteridia) and were present in the blood of every animal affected with anthrax; and later still, Koch succeeded in making cultures of them and reproducing the disease by inoculating other animals with these cultures. But even in 1876 and later it was denied that the bacillus anthracis was the cause of anthrax, until Pasteur's work finally settled the question in 1881. Inoculations against anthrax have now been employed since 25 years ago, and wherever employed the losses from the disease have sunk into insignificance. Sheep and cattle are inoculated with the attenuated virus known as the anti-charbon vaccine. This vaccine is obtained of different strengths by cultivating the anthrax bacillus in neutralised bouillon at different temperatures and for different lengths of time. It is found that a second inoculation with a vaccine of greater strength than that used for the first inoculation in the same animal, gives a greater degree of immunity and protects the animals against the most virulent form of anthrax. The inoculations are not altogether free of danger, and a certain number of sheep and cattle—though an excessively small proportion—die as the result of the inoculation. Millions of sheep and half a million of cattle were inoculated in France in the 12 years 1882-1893. The result of the treatment of the mortality from charbon, including the mortality from the inoculations, was to reduce it from 10 per cent. to 0.94 per cent. in sheep and from 5 per cent. to 0.34 per cent. in cattle. M. Chamberland (Annales de l'Institut Pasteur, March, 1894, p. 163), from whose report these figures are taken, estimates that the money advantage of these inoculations to French agriculturists amounted at the least to 5,000,000 of francs in respect to sheep, and to 2,000,000 of francs in respect to cattle.

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E.—Glanders.

Glanders or farcy, which originates in the horse and occurs in man by contagion, is a disease caused by a specific microbe, namely, the bacillus mallei. This disease is very rife among horses, and is stated to be on the increase. Tigers, lions, cats, sheep, goats, asses, and other animals, are susceptible to inoculation with the virus of glanders. In man, it occurs in the acute and chronic forms, and is exceedingly fatal in the acute. When man is the subject of glanders, diagnosis is rendered positive by detection of the bacillus in the nodules, the pus from the sore, the sputum from the lungs, and in the fluid of an affected joint. Numerous bacilli are also found in the blood of man more constantly than in the blood of glanderous horses. Treatment is still most unsatisfactory.

Though glanders has been known for centuries, it was not until 1882 that the bacillus of glanders was discovered; later it was cultivated on sterilised potato and on solid, sterilised blood serum at a temperature of 104 deg. F. Inoculation experiments with the culture were made on horses, guinea-pigs, rats, and rabbits, to prove the identity of the cause of the disease. Test inoculations with mallein are largely used on horses before they are put into stables with other horses, to ascertain whether they are or are not in an incipient stage of infection. Up to the present, however, no preventive or curative antiglanderous vaccine has been discovered, though future experiments may, it is to be hoped, furnish one. The knowledge of the cause of the disease has led to greater care being used with regard to sponges, cloths, etc., used by grooms and ostlers who are themselves in consequence less likely to be infected.

F.—Graves' Disease.

Graves' disease is nowadays treated with very marked improvement in all symptoms, and in some cases with complete cure as the result, with antithyroidin—*i.e.*, serum removed from goats after removal of their thyroid gland. ("Medical Annual," p. 54, 1906.)

IV.

Experiments on Animals in Relation to Cancer Research.

In the matter of cancer, it is important to point out that before the establishment of research laboratories, and up to 1902, cancer had been studied almost exclusively from the clinical aspect, and by pathological, histological, and chemical examinations of the dead cancerous tumours. The result has been that our ignorance as to the cause and origin of cancer at the beginning of the twentieth century was almost as profound as ever before. Hence the importance of studying living cancer-tissue by transplanting it from animal to animal; and mice were selected as the subjects of the great majority of the experiments, because the disease occurs spontaneously and frequently in them.

To keep the tumour-tissue alive, and to provide a sufficient and continuous supply of it, large numbers of inoculations are essential:—

(*a*) Because many of these inoculations fail entirely;

(*b*) because in many instances the tumours which arise and for a short time grow in mice, subsequently atrophy and disappear;

(*c*) because of the small amount of tumour-tissue obtainable from each mouse;

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(*d*) because the step by step investigation of the changes which take place at the site of inoculation entails the sacrifice of a large number of animals at every stage; and

(*e*) lastly, because the investigation of the secretions and body fluids of cancerously affected as compared with unaffected animals requires the use of a large number of mice.

If it be asked what, up to the present time, has been learnt or confirmed by all these experiments, the answer is:—

(1) Positive knowledge has been gained that this mouse tumour, which is identical in nature with the human mammary tumour, grows by an inherent growth of its own, and is not due to a change in the tissues themselves of the animal into which it is engrafted.

(2) That cancer can, and does, not so very infrequently, disappear spontaneously.

(3) That the animals in which cancer has grown and then disappeared are immune to fresh inoculation.

(4) That a mouse can be rendered immune by the injection into its body of some of the blood of a healthy or an immunised mouse.

It should be pointed out in reference to these inoculation experiments that they are practically painless; or, if pain can be said to be caused at all, it is but slight and momentary, and to be compared in the human subject with the pain caused by a simple subcutaneous puncture.

Neither is pain suffered by the animal in which the transplanted tissue grows even into a large tumour. It is only in the exceptional cases, in which the tumour ulcerates, that any pain occurs, and as this condition renders the tumour useless for further experiment, the animal is at once killed.

Evidence as to the painlessness of these growing tumours is afforded by the activity of movement, sleekness of coat, and general healthy appearance of the mice, their indifference to pinching or pressure of the tumour, which is not innervated; and further, by the fact that when they have been kept in separate compartments of the same cage they have eaten through the partition, mingled with one another, and a litter of young ones has been the result.

Every cutting operation in these investigations is performed under complete anæsthesia, and precautions are always taken to secure a septicity and immediate healing of the wound.

As regards other animals, inoculation experiments have been undertaken to a very limited extent on horses, dogs, rats, rabbits, frogs, and fish. Only in the case of the rat and dog have inoculations been followed by the development of the disease, and to these the same remarks apply as in the case of the mouse.

Any limitation placed upon the number of inoculations, whether in mice or in other animals, including anthropoid apes, to which resort may ultimately be necessary, will have a disastrous effect on the further development and the prosecution of cancer research in this country. It is at present impossible to determine how far the contrast between the success obtained in mice and the failure in other animals, is due to the discrepancy in the number of inoculations made.

As a member of the Executive Committee of the Imperial Cancer Research from its outset, I have had frequent opportunity of witnessing the complete absence of suffering in animals the subjects of these cancerous tumours, and of observing the extreme care and attention given to all the animals, both in London and at the farm.

Experiments on Animals under Certificates B. and C.

The foregoing observations have had reference almost entirely to inoculation experiments; but I should not complete the description of the importance of vivisection to the progress of surgery were I to omit altogether the mention of experiments of a larger kind.

I must refer in this connection to the localisation of the motor centres in relation to brain abscess, brain tumours, and brain injuries, and to the operation of trephining for these conditions.

I would not for a moment wish to imply that much had not been learnt as to the localization of function from clinical observation apart from experiments on animals; but I do not think it can seriously be contested that the extent and rate of acquisition of our knowledge of this subject, the precision of our knowledge, and the confirmation of what had been otherwise learnt, have been most important results of experiments on the brains of animals.

When it was pointed out that it was, possibly, not so much the original escape of bile, after rupture of the gall-bladder, etc., which brings about the fatal result, as the continual filtration of bile into the peritoneal sac, the question arose as to how far surgery could afford aid in preventing the further escape of bile in this kind of injury. To this end M. Herlin and M. L'Anglas made experiments on dogs and proved that these animals could live after their duct had been tied or the gall-bladder removed. Laparotomy in man followed by suturing the gall-bladder, tying its duct, or removal of the gall-bladder, has since saved a good many lives.

Dr. V. Czerny, to investigate the question whether the larynx could be extirpated without death resulting, performed a series of experiments on eight dogs, all of which underwent total extirpation of the larynx. The first four died from the consequences of the operation. The last four survived and remained well. It having been thus shown that the larynx in not indispensable to life, and that the operation is feasible, extirpation of the larynx became a recognised surgical operation. To what perfection it has been brought, may be seen from the results described in the latest contribution on the subject by Dr. Chevalier Jackson, of Pittsburgh ("British Medical Journal," November 24th, 1906). Of eight consecutive cases of total laryngectomy for laryngeal cancer not one patient died from the operation; one lived 7 years, one 3 years, one apparent cure was lost from observation, three recurred within one year, and one is too recent to record. One died from alcoholism eight months after operation. It deserves particularly to be mentioned that once the feasibility of removal of the larynx having been shown, Czerny's experiments have never, so far as is known, been repeated.

The repetition of operations of an experimental kind on animals has, however, often been the prelude to the adoption of most important operations on man.

Take, for instance, nephrectomy or extirpation of a kidney: Zambeccarius in 1670 and Roonhuyzen in 1672 and several others in the beginning of the 19th century, had shown by experiments upon animals that life could be well maintained after removing one of the kidneys. Still, when Gustav Simon of Heidelberg in 1869 wanted to relieve a woman of a distressing condition, and could only do so by removing the kidney, he was reluctant to operate without first making experiments for himself. He knew that pathology afforded him many instances of persons living after one kidney had been rendered gradually useless by slow disease, but he wanted to confirm, if he even knew of them, the experiments by others which showed that the sudden withdrawal of the functions of one organ could be physiologically tolerated. He wanted also to know what were the immediate risks of the operation as distinct from the risks of interfering with the urinary functions—the risks namely of hæmorrhage, of embolism of the renal vein, of pyemia, etc. Hence his careful observation of fifteen hysterectomies on bitches contrasted with the same number of nephrectomies on dogs. (Hunterian Lectures, 1898, H. Morris.)

Again, when the numerous operations on the stomach and intestines for short circuiting, excision, etc., were rendered possible by the great safety with which the abdominal cavity could be opened and dealt with under aseptic precautions, surgeons were anxious to test for themselves the various methods and modifications of methods, for suturing the bowel, though these or some of these had been employed in experiments on animals, as well as in cases of injury in man, by surgeons of some centuries before.

It shows only a praiseworthy and natural solicitude and anxiety on the part of the surgeon that, before adopting a new operation on a human being, he should, as far as possible, test it beforehand on one of the lower animals under conditions and circumstances as nearly as possible similar to those of his patient.

7727. I understand that there are some matters which you would like to mention in addition to what you have stated in your *précis*?—In the first place I should like to add—it may have been already before the Commission, and, if so, I hope you will stop me, but it may be of some interest if it has not—the matter of antitetanic serum. Tetanus has been, as you may know, a

very common affection after wound inflicted during the celebrations of July 4th (Independence Day) in America.

7728. It was mentioned by some witness, I think, and I think you mentioned it in your own *précis*?—Yes, but I have got some figures here, if you care to hear them. For instance, the use of the prophylactic anti-toxin was introduced in 1904. In 1903 the number of these tetanic cases (this, of course, only goes to a certain extent as being valuable) was 415, and in 1906, after the use of anti-tetanic serum for all those who have been wounded or injured, the number of cases was only 89.

7729. Are you speaking there of New York?—Yes. Of course it depends, you will say, upon the number of people injured. I do not think I have quite got that out—I mean the proportion of injured to those who get tetanus; but there is the fact that the number of tetanus cases in 1903 was 415; and (after the use of the tetanus anti-toxin) in 1905 there were 105, and in 1906 there were 89.

7730. (*Dr. Wilson.*) Would they be wounds derived from fighting or from what is called gravel rash?—Mostly from pocket-pistol wounds, fire-works, and things of that sort—small wounds. They pick up in the streets bits of anything, bits of wood or paper, or anything of the sort from the dust and mess in the street, and put those into the pistols and fire these off as a sort of sport, like back-scratchers in our own streets; and those produce a certain number of wounds; and being produced though by small force, yet by an article which is impregnated with dirt and soil, they are very prone to produce tetanus. And then the introduction, for all those persons so wounded after these celebrations, of the anti-tetanic serum has had that effect in reducing the number of cases which occur.

7731. But would not a great deal depend upon the condition of the streets and the kind of weather—whether it was a fine day or a dirty day?—That does not touch the point, so far as I see. The question is, the liability of wounds to tetanus derived from a particular source, and here is the same cause one year as with the other, producing wounds of the same kind; and by the use of the anti-tetanus serum the proportion of cases of wounded persons who get tetanus as the result of those wounds is much diminished.

7732. (*Chairman.*) I think what Dr. Wilson meant was when there were 89 it may possibly have been a wet day, when all the dust and dirt was kept down, and it did not therefore cause the same number of cases; there may have been over 100 people wounded, and 90 per cent. of the 100 may have died; whereas in the other case it may have been a very dusty day, when the tetanus bacillus was very much about, and the 400 may have shown a less mortality per cent. than the 90. It is subject to that observation?—Of course, the 4th of July comes in the same time of the year, and generally under the same kind of conditions as to weather in America.

7733-4. (*Mr. Tomkinson.*) Were all these cases treated in the public hospitals? Do you know they are all treated with this serum?—I do not know that they are all treated. I know that they endeavour to treat all that are thus wounded. It is some time since I looked at any of these papers, because at the last moment I was rather suddenly summoned before this Commission; but I find, looking down a little lower in my notes, that I have the numbers wounded. The tetanus cases have diminished on the 4th July, but the number of wounded have risen from 4,449 cases in 1903 to 5,466 in 1906.

7735. (*Chairman.*) That quite answers Dr. Wilson?—And then, with 4,449 wounded in 1903, we have 415 cases of tetanus, and with the 5,466 in 1906 we have 89 cases of tetanus. The reference to these figures is "The Journal of the American Medical Association," August 18th, 1906, pp. 505-8.

7736. (*Colonel Lockwood.*) Is that in the United Kingdom?—No; in America.

7737. That is why you mentioned the 4th July?—Yes.

7738. (*Dr. Wilson.*) But do not you think it is possible that they had such a fright in the first instance that they would feel more careful about cleaning any wounds?—These statistics have only reference to a year or two, and it is difficult to arrive at any absolute conclusion from them perhaps. I do not want to press the point further.

7739. (*Chairman.*) I quite see the point. You admit that you cannot take it from the figures precisely—that the argument does not go up to the figures, as it were; but that a general presumption arises from the figures?—Yes. Without taking it as absolutely accurate it is a very striking effect.

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7740. And allowing for other possible causes?—I refer on this same subject to a paper by Scherck. He gives a record of 291 injuries caused by these toy pistols in America during the 4th of July celebrations. The injured persons were immunized by the antitetanic serum without a single case of tetanus occurring, and Dr. Scherck compares these results with those obtained in a year when no serum was used, and in which nearly one-third of the injured succumbed to tetanus. That is merely in support of that statement. Then there is another point I thought it would be of some value to refer to, and that is, in the matter of the treatment of anthrax, or wool sorters' disease, or whatever name you know it by, by means of the anti-anthracic serum ——

7741. Are you speaking of anthrax in mankind?—Yes. I have here from Dr. Legge, of the Factory Department, a letter, and also a copy of the Annual Report of the Chief Inspector of Factories and Workshops. Perhaps that is in the possession of the Commission. I only want to draw attention to what he says there. He says: "There is, I think, a consensus of medical opinion that in cases of cutaneous anthrax it" (that is this serum) "is a very valuable remedy. In internal anthrax administered subcutaneously it did not prevent a fatal issue in any case last year." A little lower down he says that in one case in which the serum was used the person recovered, and he gives here a table where there are cases of recovery after the use of the serum; and he gives me in a private letter which I have here some further information, which, perhaps, it will really be shortest if you will permit me to read.

7742. If you are only going to state what Dr. Legge says, I may tell you that he is coming himself as a witness?—Very well; but I can say it is distinctly favourable, and that at St. Bartholomew's Hospital now the only treatment in the last few cases of anthrax that they have had there has been the introduction of this serum of Sclavo.

7743. (*Colonel Lockwood.*) Are they all successes?—They are all successes; and Dr. Legge will give you a table of cases which are more recent than those which he gave in his Milroy lectures before the College of Physicians in 1904 or 1905.

7744. (*Sir Mackenzie Chalmers.*) He has been working at it during the last three or four months?—Yes. Then another thing, perhaps, also has been before the Commission, and I pray you stop me if it has—that is with regard to the work in the Japanese Army.

7745. (*Chairman.*) I do not think we have had anything more than a casual reference to it?—Baron Takaki has said: "As one example of the results and terminations of treatment I give you a short account of the hospital established at Hiroshima. The hospital was established in April, 1904, and the report covers the time up to November, 1905. During this interval the hospital admitted 203,782 cases, whence 162,885 were transferred to other hospitals; but the average number of patients in the hospital was about 5,000, and the largest number the hospital had at a time was 10,000. Almost all serious cases were kept in the hospital, because they were unfit for transportation. Yet the result was so good that the ratio of death and invalided is a little above one per cent. Almost all the wounds of the soft tissues healed within ten days. Those with injuries to the bone have obtained favourable results, excepting those who were wounded at the siege of Port Arthur. The many men who were wounded in the head and chest have recovered from the wounds received; and there were also many cases of aneurysmal varix, wounds of nerves requiring operations. The number of operations performed at the hospitals amounted to more than 3,500." That is from Takaki's "Military Hygiene of the Japanese Army."

7746. I suppose something would depend upon the race there. You might have a race which had very few unhealthy members in it?—There, again, my Lord, other factors come in, of course, but still, from what we know of armies of all times and in all countries, it is a very remarkable result. These results ought to be contrasted with what obtained in the Franco-Prussian War. Neléton, the famous French surgeon, in despair at the high death-rate attending almost all operations

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on the wounded, declared that he who would conquer purulent infections would deserve a golden statue. That golden statue is really almost deserved when you find under the circumstances of war that only one per cent. out of such a very large number of wounded and operated upon were fatal.

7747. He does not give the percentage in the Franco-Prussian War?—No, not here. Those figures are to be easily obtained, however, of course.

7748. (*Dr. Wilson.*) Have you any statistics with regard to the South African War?—No, but they are published, I believe.

7749. They are not so satisfactory as the Japanese?—I have not got them here.

7750. (*Chairman.*) That is the percentage of those who died of wounds—not of disease?—These are wounded, yes. Then I would like also to mention a statement which I have had from Sir Samuel Wilks, who was the pathologist at Guy's many years ago. He writes, and says: "The discovery of impurities getting into the system being the great cause of the mortality after injuries and operations, led to the improved modes of dressing introduced by Lister. So that the greatest operations may now be performed with safety. All this is well known to the surgeon, but it came home to myself in an almost startling manner from what I witnessed in the post-mortem room. Some time before, when writing a paper on pyemia, I had no difficulty in collecting a hundred cases which had occurred in a very short period previously, when suddenly this terrible malady disappeared, it was gone, never to return." He says he had the greatest difficulty in finding in more recent researches any records of pyemia that have occurred recently.

7751. Of course, the importance of all this to us is only with reference to how far this is owing to experiments on animals. You have already given us your view about that?—Yes, and these are further illustrations—that is all. There is one other matter that I think I have not mentioned in my paper, which I should like to say one word upon, and that is, the desirability of retaining the power on the part of lecturers on physiology of giving demonstrations to classes. I think it is a matter of importance that they should still have that power. These exhibitions are of very great importance in teaching classes.

7752. Is the importance that you attach to that with reference to enabling students to follow what the structure or the nature of the disease is from inspecting the animal, or to teach them how to operate?—I am not suggesting that there should be experiments on animals for the purpose of teaching how to operate; but I am suggesting that it is very important to retain the power that still exists on the part of physiologists to give a certain small number (and they always are but few) of demonstrations to classes who are learning the functions of the body; for instance, to know what the action of the heart is like and what the action of the intestines is like, and the effect of influencing the organs through irritation or section of different nerves. There are a few things of that sort which can only really be fully and completely impressed upon the student by having practical demonstrations of them; and they are performed in small classes (not in a great theatre), before a group of students, upon animals, which are under an anæsthetic and are killed as soon as the experiment is over, and before they are allowed to come round from the anæsthetic. This is merely one of the things that concerns the teaching of the sciences of the profession to which many of us attach a good deal of importance.

7753. Did those demonstrations and experiments take place in your student days?—Yes, I believe Guy's was one of the few places in London where they were shown, but I was a pupil of Dr. Pavy at Guy's Hospital, and Dr. Pavy used to show us a few of these things. There were not many experiments, perhaps only four or five in the course of a whole session; but I can say from personal experience how one was impressed by the sight for a few minutes of a thing of that sort, which no amount of reading or verbal descriptions or lectures would ever have given to one.

7754. Was that before or after 1876?—It is so many years ago that it goes back to before 1876.

7755. Before the Act of 1876?—It goes back to before the Act of 1876, because it must have been in 1867 or thereabouts. I do not remember quite the exact date. Among the questions that were put to some of the witnesses in 1876—I think to Sir James Paget and Dr. Pavy himself—one had reference to a certain regulation which is printed in the College of Surgeons' Calendar in the regulations relating to the education and examination of candidates for the Fellowship. It is there stated that a course of practical physiology must be attended at a recognised school, and questions were asked as to what practical physiology meant. We have, in our Calendar since 1874, had the following note: "By this is is meant that the learners themselves shall individually be engaged on the necessary experiments, manipulations, et cetera, but it is not hereby intended that the learners shall perform the vivisections"; and in case these regulations before you now I should like to explain that although we expect them to have seen the experiments, we do not expect them to perform them themselves.

7756. That I quite understand?—Perhaps 20 men will see an experiment which is shown by a lecturer.

7757. (*Sir Mackenzie Chalmers.*) Who is a licensed person?—Who is a licensed person, and who is, of course, an expert in the manipulations required for; but they are not to be performed by the students individually.

7758. (*Colonel Lockwood.*) Not necessarily?—No.

7759. They could not do so without a licence?—No.

7760. (*Dr. Gaskell.*) Is that regulation still necessary for the Fellowship?—Yes; it still appears in this set of regulations, but it has that note in italics.

7761. There is no such regulation for membership?—No.

7762. It is for the Fellowship?—Yes.

7763. (*Sir Mackenzie Chalmers.*) What is the actual number of Fellows of the Royal College?—There are either 1,300 or 1,350—I think 1,350. But the number of men who come up at each batch—and the examinations is held twice a year—perhaps varies from 50 to 70.

7764. (*Dr. Wilson.*) Would it necessitate a certificate from the teacher that the candidate for the Fellowship has attended such demonstrations?—Yes.

7765. (*Chairman.*) Is that all you wish to add?—If you please.

7766. (*Colonel Lockwood.*) How is it known that experiments on living animals are going to take place at these lectures? Are they called "A Course of Physiological Lectures," and it is understood that they will include experiments?—As I have said, only on very very few occasions are there any experiments on animals, perhaps only on four or five, or, at the utmost, half a dozen, in the course of a winter session.

7767. How will the men know?—Those men who are attending the lectures would be informed at the end of a class—this is the way I have known it done—that at the next lecture, or the next time of meeting, there would be this or the other shown upon a living animal.

7768. Now, supposing I myself, or we will say Sir William Collins, or any member of this Royal Commission, wished to see an operation on a live animal—you will understand my motive for asking the question—and that we did not wish the demonstrator to know that we were coming, how could it be worked? You see my point. My point is that if the man was ready for me other people might say, "He knew that you were coming"?—You mean a surprise visit.

7769. Yes?—Of course, the inspector can go in at any time, there is nothing to keep him out; and the object of keeping anybody else out would merely be in conformity with that section in the Act which provides that these things are not to be for public exhibition. But if you were to go to and say, "I am very much interested in seeing this experiment," there is not a lecturer in London or in the country who would not allow you to come in and witness it, I feel sure.

7770. But you see my point. Other people might say, "It is quite true that you went, and you saw the experiment; but the man, of course, knew you were coming, and so he conducted it more humanely than if you had not been present"?—I do not think anything of the kind. The Commission must take my opinion for what they think it is worth; but I feel satisfied that there is not a single lecturer in the country who would vary his experiment because there happened to be a member of the Commission or a member of the Anti-vivisection Society or of one of the Anti-Vivisection Societies present.

7771. I am quite content to take it so.

7772. (*Chairman.*) It is really a question of the amount of anæsthesia what Colonel Lockwood is aiming at, whether the anæsthetics are always applied fully so as to ensure the absence of pain?—I believe, most certainly, that they are. I should think the truth is that there is probably a waste of anæsthetic—that there is more anæsthetic given rather than that there is too little. In the experiments that I have seen, sometimes when it has been known that I was going to be a spectator, and sometimes when it was not, it has seemed to me that there has been rather an over-administration of anæsthetic instead of an under-administration.

7773. (*Sir Mackenzie Chalmers.*) More than you would give to a human being?—Yes.

7774. And which would entail danger in the human being?—Yes. You see, if a dog dies, it does not matter, because it is going to die at the end of the experiment.

7775. (*Colonel Lockwood.*) You gave some evidence about the Japanese. Do the Japanese make experiments on animals?—I cannot answer that; I should think they do.

7776. But you do not know of the existence of any experiments on animals?—I do not.

7777. But you would, I suppose, allow that a nation like the Japanese, who are more, I was going to say vegetarian than carnivorous, are more likely to recover from wounds, as the Indians do, than a carnivorous European?—Yes, I should think there is much in that.

7778. If we grant that Lister's and Pasteur's discoveries are facts, do you see any necessity for continuing experiments bearing on those discoveries of Lister and Pasteur?—I think there is every reason, every necessity, if we are to go on progressing in medicine and surgery, that these experiments must be continued.

7779. On the lines of Lister and Pasteur?—Yes, certainly.

7780. To perfect and continue?—Yes, and to deal with and to combat other diseases.

7781. Do you claim that these injections are a cure for tetanus, and cholera, and anthrax?—Yes.

7782. You would go so far as that?—Yes.

7783. You made your name, I think, which is a very great one, by experiments on the kidney, was it not?—I never made an experiment in my life.

7784. I do not mean on an animal. I mean operations—I apologise?—Oh, no need for that. I thought perhaps you meant experiments on animals.

7785. No, I meant operations?—Yes.

7786. Were those operations which you have conducted so skilfully, and fortunately discovered, and were you made to do them, by experiments on living animals?—Personally, not at all. The operation that I did in the first place, and was the first to do, was done quite irrespective of any experiment upon animals; that is the cutting of a stone out of a kidney. But that was not the case with regard to cutting a kidney out of the body, because Simon of Heidelberg, who was the first to do that, made a deliberate set of experiments for the express purpose of knowing various things before he submitted a patient to the operation. He, of course, knew what the effect of the loss of a kidney by disease (a slow process) was; but he wanted to know what the loss of a kidney, by being taken suddenly away, would be, and until he knew that he did not feel at liberty to perform the operation of cutting the kidney out. But the kidney which he proposed to remove was healthy—I am speaking now historically of the first operation—and the operation was deliberately planned for the removal of a healthy kidney from the human body.

7787. Total excision?—Total excision; it was a healthy kidney, and the tube running from the kidney to the bladder, which had been cut in the course of previous operation accidentally, had its end sticking out of the flesh. The consequence was that the poor woman was constantly suffering from the annoyance of urine trickling down her side or down her front, and she implored for relief from this by having her kidney removed. That led Simon of Heidelberg to make his experiments to find out what the amount of shock would be, and what would be the risk of producing that form of blood poisoning which is known as uræmia, and what would be the effect upon the health by the sudden taking away of a kidney that was otherwise healthy, except for this leakage from this tube. Accordingly, he made these experiments.

7788. He made those experiments first on an animal?—He made those experiments first on dogs and other animals, and then he applied the results to this living female patient.

7789. As regards your particular operation, you did not do that from knowledge acquired by experiments on a living subject?—Not at all from experiments on animals; but from what I had learnt by experience from operations on the liver and other organs in the human subject.

7790. Have you witnessed any or many experiments under licence?—No, not many. I have seen a fair number. In quite recent years I have seen some, since last year, for instance.

7791. Have you ever witnessed any cruelty on the part of a demonstrator?—No, absolutely none.

7792. Or any levity on the part of those who witnessed them?—No.

7793. And always under an anæsthetic?—No, not always under an anæsthetic; it depends upon the class of experiment. When I have seen an experiment to show any effect of blood pressure, or things of that sort, the animal has been completely anæsthetised; but when I have seen experiments on guinea-pigs and rabbits, or what not, for the purpose, for example, of seeing the effect of the injection of serum (some I saw the other day) those were not done under chloroform.

7794. Have you see any of those experiments on the eye?—I have not seen any experiments on the eye.

7795. But where severe pain would have been inflicted, anæsthetics, according to the licence, have always been administered?—Yes, absolutely.

7796. And the animal destroyed before recovery?—Yes.

7797. Do you look upon morphia as an anæsthetic sufficient to prevent, more than dull, pain in a living subject, used for the purpose of an anæsthetic?—No.

7798. Would you say that you could not give an animal enough morphia to make it totally insensible to pain, if that was the only anæsthetic used?—I do not think it ever is the only anæsthetic used. I bear in mind, of course, that at one time it was the great means of rendering the human subject anæsthetised before the introduction of chloroform and ether; but I think that now morphia is given perhaps as an adjunct to the administration of an anæsthetic. I would rather not express any very definite opinion about it, because, as I say, it does not come into my way; but just as I know in the case of the human subject, so I think in regard to animals; if an experiment were wanted to be done on an animal where a very small quantity of anæsthetic was given, where as little anæsthetic as could be should be given, then, I think, morphia would be a very valuable adjunct administered before the administration of the chloroform. But the complete anæsthesia would be the same with a less quantity of chloroform, if the morphia had been previously given.

7799. But you would not look upon it as a complete anæsthetic, even given in great quantity?—No.

7800. Do you think that there is enough resemblance between animals and human beings to make experiments on living animals useful?—Yes.

7801. I may take that generally from what you have told us?—Yes.

7802. How long can an animal be kept under anæsthetics, would you say?—I have seen an animal kept under chloroform, I suppose, for two hours.

7803. At a stretch?—Yes, at a stretch, but then I have seen a living human subject often kept for as long a time.

7804. (*Sir Mackenzie Chalmers.*) What is the longest time with a human subject that you have seen yourself?—I could not answer that question off-hand, but I should think the longest time I have ever seen a

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human subject under anæsthetics was between three and four hours for the treatment of complete compression for an abdominal (aortic) aneurism.*

7805. (*Colonel Lockwood.*) Up to the present time do you think there has been any hindrance to science and to your profession generally by the regulations issued by the Home Office for experiments on living animals?—No, I cannot say that I do think so. I think the present Act of 1876, under which vivisectional experiments are done, was amply protective against cruelty to animals and sufficiently free and liberal for the due prosecution of proper scientific and physiological inquiry.

7806. Would you object to any further regulations?—Yes. There is one respect in which I should like to see greater freedom. Under the Act, of course, the Home Secretary can grant a licence to anyone. I should certainly like to see the thoroughly trained first aids in the laboratory permitted to do such very slight and mechanical operations as the injection of a small quantity of serum. There are a very large number of animals treated by this serum injection, and if you are injecting a considerable number of them it requires, as things are now, a highly-skilled man to spend a great deal of time in the work of just injecting the serum which these men who are trained assistants in the laboratories could do quite as easily and quite as completely and safely as the director himself does. It is a great waste of time, and is a great hardship upon the director to have to do all that simple routine work.

7807. (*Sir Mackenzie Chalmers.*) What you mean is that you think the licence should cover Certificate A, so to speak, for the assistants?—Yes.

7808. It is only Certificate A you are referring to?—Yes.

7809. You think that when a man of established character holds a licence and Certificate A, the Certificate A should extend to any able assistant of his?—Yes, either on the recommendation of the director, or it may be by having a certificate filled up as now for the director himself; but I certainly think that power should be given to a man who has been for a long time, and is, highly trained in this kind of simple laboratory work, to do it, merely to save labour to the scientific man himself.

7810. (*Colonel Lockwood.*) I suppose I may take it that you naturally would object to the exclusion of dogs from experiments?—I should certainly.

7811. You think it would be a hindrance to science?—Yes, a very material hindrance.

7812. You have read, very likely, the evidence of previous witnesses before this Commission?—I have.

7813. Are there any remarks you would like to make as regards the evidence of the witnesses who are opposed to vivisection?—If I had the material before me I could write pages of objection.

7814. But nothing strikes you particularly as very false or very wrong?—That is rather too wide a question, I think, to put to me, because I have not it all quite at my finger-ends.

7815. Of course, you know the very strong feelings that exists in the minds of many people as to the propriety of any experiments on living animals?—Yes.

7816. What do you think is the foundation of that feeling? Do you think it is simple romance, or that cruelties have existed and do exist abroad, undoubtedly; or what do you think is the foundation for this very strong prevailing feeling?—There are many words one could apply.

7817. Ignorance, I suppose is one?—I should rather not use any words to give offence, but I think it is not unlikely that a good deal of the feeling against all forms of experiments on animals, is due largely to what you have alluded to, the, shall I say, recklessness and the indifference perhaps on the part of some, with which experiments have been reported as having been done, and I believe have been done, in some of the places abroad. I think that that having been spread abroad, and not having lost in disrepute by travelling, is very largely the cause which has led people who have not looked at the thing and have not considered the thing all round, and who do not really know the way in which things are conducted in this country, to take the objection that they have taken.

7818. You do not believe as a man of science, and I have no doubt as humane a man as myself, that there is any real foundation for this strong feeling that exists?—I do not believe there is an iota of foundation.

7819. (*Sir William Church.*) You told the Commission, in answer to a question by Colonel Lockwood, that you do not consider morphia an anæsthetic?—Yes.

7820. But do you think that an animal, a dog or a cat, which receives a poisonous dose of morphia or opium is in a condition to feel pain up to the time that death occurs?—No, I do not think so. They suffer no pain.

7821. But so far as sensation goes, there is perhaps no anæsthetic that we know so much about as morphia or opium, because we know that when a man recovers from a nearly poisonous dose, and recovers with difficulty, he has been quite insensible to all that has happened to him during the time that he has been under the action of the drug?—Yes, we know that equally as we do with regard to chloroform and ether.

7822. Therefore, when you told the Commission that morphia was not an anæsthetic, you meant that if used as a perfect anæsthetic, the animal would not survive?—Yes. I was assuming that Colonel Lockwood meant to ask me whether I thought that in the case of an operation done upon an animal to which morphia had been administered, the intention being, of course, that it should have an affect for the time and the animal should live afterwards, it was as sufficient as chloroform, and I said that I did not think it was. But if you are going to give a poisonous dose to an animal, then of course the operation might be done and the animal would not feel pain.

7823. I wished to put the question because we have been told here by several witnesses that opium is not an anæsthetic, and therefore even when a poisonous dose is given, the animal is probably suffering tortures until death occurs?—That is not so.

7824. (*Sir William Collins.*) I think you said that you had never yourself held a vivisection licence?—No, I have never performed any experiment.

7825. Nor have you practised any vivisection?—No.

7826. Nor have you used vivisection, of course, for demonstration purposes?—No.

7827. Nor for obtaining manual skill?—Certainly not.

7828. I gathered from one answer which you made to the noble Chairman that you rather objected to the use of vivisection for the purpose of obtaining manual skill?—Those are my feelings. I do not think it is necessary.

7829. Should I be right in thinking that many practising surgeons practise vivisection?—I should think not for that purpose.

7830. Or for any purpose?—Well, there are surgeons who practise vivisection no doubt occasionally for the purpose of ascertaining the effect or the technique of a new operation, but not as a means of demonstration, if that is what you are asking me.

7831. Shall I be right in thinking that there are many practising surgeons who have never practised vivisection?—Surely yes.

7832. I only wanted to get it from you as President of the Royal College of Surgeons, whether that was the case or not?—That is so.

7833. I understand that the Council of our College which you represent is not anxious to see, with the single exception that you mentioned of the permission to assistants in laboratories to perform minor injection operations, any modification in the existing law or practice?—I think they are content with it as it is.

7834. Shall I be right in thinking that they would regret a modification of the existing law?—I am sure that you would, if by modification you mean curtailment.

7835. Of such protection as is accorded to animals by the existing law?—I thought you meant curtailment in the way of privilege to perform experiments. I think they would regret that, but I think that they would equally regret to see any curtailment of the proper and due protection of animals.

7836. They recognise, in fact, that vivisection should be practicable, but that it should be practised under restrictions?—Yes.

* *The exact time was 4½ hours. For particulars see 56th Vol. of the Medico-Chirurgical Transactions, p. 390 1873—H. M.*

7837. I gather from your *précis* that you are anxious to state that in your opinion the use of experiments on animals has been a part cause, not by any means the whole cause, of the great improvement to which you have borne witness in the course of your examination?—That is so.

7838. And that you find a difficulty in disentangling the causes that have been at work in affecting that progress?—Yes.

7839. I think you state that it is practically impossible to allocate with precision that which has been due to vivisection and that which has been due to other causes?—Yes, I think so.

7840. That is to say, that you would not wish to underrate the importance of clinical observation, pathological anatomy, and so forth, which have also contributed to that end?—Certainly not.

7841. In the return which you put in which has been prepared by Mr. Watson Cheyne, you place Pasteur's work on spontaneous generation as the fundamental basis upon which the modern aseptic and antiseptic treatment of wounds has been erected?—I do, together, of course, with Lister's work.

7842. As one of the fundamental supports of that superstructure?—Yes.

7843. Should I be right in thinking that Professor Bastian has recently again contested the alleged conclusions of Pasteur as to spontaneous generation?—It is quite certain that he has done so.

7844. That is to say, that it is still an arguable question, so far as recent literature is concerned?—Yes, in his own opinion.

7845. And as regards Lister's work I suppose one would be right in thinking that there have been considerable modifications in the Listerian doctrine between 1876, when as Mr. Lister he gave evidence before the Royal Commission, and the present time?—I do not quite follow that.

7846. For instance, in regard to the use of the spray and drainage, and so forth, there has been a very considerable alteration in technique, if not in the principles upon which Listerism was based?—The technique of the employment of facts which he has laid down, the technique of the employment of the treatment which is based upon the facts which he has discovered.

7847. Should I be wrong in thinking that Lister laid great stress upon the use of the carbolic spray and drainage?—Quite so. Those are all modified now. The spray we know is an abolished thing.

7848. I notice that in this same Table which you have handed in, which has been prepared by Mr. Watson Cheyne three matters are marked with a ×, which implies "impossible without experiments on animals." They are "immunity," "serum therapy and immunity," and "increasing the resisting power of the body"?—Yes.

7849. From your own personal experience would you say that surgery has derived great advantage from those three matters?—I should, certainly.

7850. What would you instance as the most striking example?—On which point?

7851. On any one of the three. Take opsonins, for instance?—I should say that the case I have mentioned with regard to the use of the anti-tetanic serum is an instance of immunity, and as giving increased resistance to the body; and so with regard to anti-anthracic serum.

7852. I was asking from your own experience?—Yes.

7853. That was in America, as I understand?—Yes, but one knows of it in cases in this country with regard to the treatment of tetanus.

7854. Have you yourself used it in many cases of tetanus?—No, not in many cases, because I am happy to say that I have had very few cases of tetanus to deal with.

7855. But you have used it?—Yes.

7856. Could you say in how many cases?—I think only in three cases. I have had very few cases of tetanus; they are not common cases in this country.

7857. Do you use it with a view to prevention or cure?—Cure.

7858. Was it successful in those three cases?—I am sorry to say it was not; that is a long time ago.

7859. Not in any of them?—No, they were all advanced cases; they were cases too late for the successful employment of treatment.

7860. Should I be right in thinking that it is generally stated now that the treatment is likely to be valuable rather as a preventive than as a curative?—Yes, although there have been cures up to quit recently claimed for the employment of the serum.

7861. In the figures which you gave us with regard to the 4th July in America, I do not think you stated whence those figures were obtained—on what authority?—I referred to one paper which is here on the memorandum that I mentioned. I am not quite sure about the other figures. I could tell you if you wished to know, but I think they came from an Annual Report of Surgery.*

7862. Are they official figures?—I do not know about their being official figures, if you mean by that that they came from some Government office.

7863. Do they cover the whole of the United States?—I cannot answer that question either without further reference, because what I have is just an extract merely as to the figures.

7864. Perhaps you will be so good as to add that to your evidence?—Yes, I will.† But I have given the reference to Scherck's Returns.

7865. With regard to what is essential for the diagnosing of tetanus, is it a bacteriological test?—I fear that it is not when the disease is developed.

7866. I mean for the 415 cases of which you spoke in 1903; how would those cases be diagnosed?—I suppose on the clinical symptoms.

7867. Does the Report state that?—That I cannot say; you must allow me not to answer that question, because I could not answer it for certain.

7868. Then you refer to the case of anthrax and some cases at St. Bartholomew's Hospital. Did I rightly understand you to say that in any case in which Sclavo's serum had been used in anthrax a fatal result had occurred at St. Bartholomew's?—No. With regard to St. Bartholomew's, it has been used in the last three cases, and I have got a letter here from Mr. Bowlby referring to one or two cases. But I have not taken up the position of stating that in every case in which serum has been used the patient has recovered from the anthrax.

7869. I thought I gathered that in reference to the St. Bartholomew's cases?—In the last three cases of anthrax at St. Bartholomew's Hospital I said that they had relied upon the serum treatment only with speedy recovery of all three patients. In several cases, of course, both in Italy, this country, and elsewhere, and especially in the earlier times of the employment of Sclavo's serum, not having confidence enough in the serum, surgeons excised the pustule as well as giving serum, but as they gained confidence with the serum they have done away with the operative part of the treatment and have been relying upon the injection of the serum alone.

7870. There again can you speak from your own personal knowledge of the use of Sclavo's serum?—No. I have seen anthrax in Guy's many years ago when I was house surgeon there; but the disease does not come my way now. It is not met with in my hospital.

7871. You referred to the experience of the Factory Department of the Home Office; I do not know whether you have noticed in a recent Report that they point out that cases of anthrax appear to occur without the bacillus being found, and they insist on the importance of having regard to clinical as well as bacteriological evidence of the presence of anthrax?—Yes.

7872. Is it the case that the bacillus anthracis is not always discoverable?—It is not always discovered, but I should certainly say, with the knowledge that I have, that it is nevertheless present.

7873. In the case of anthrax do you think it is safe to argue from the experience in animals, from one set of animals to another and from animals to man?—I think so.

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* *The reference is "The Journal of the American Medical Association," Aug. 18th, 1906, pp. 505–8.—H. M.*

† *The tetanus cases occurred in 25 different States of America.—H. M.*

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7874. Has it been noticed that certain breeds of sheep are quite unamenable to inoculation with anthrax, such as Algerian sheep, whereas other ordinary sheep suffer severely on inoculation?—I cannot speak as to that. The variety of cases in which sheep are affected in large numbers very largely depends on the particular locality and the fact that anthrax has been buried perhaps a long time ago, but as to different breeds I have no knowledge.

7875. You mentioned the case of septicemia in mice?—Yes.

7876. Is that poison the same when it is inoculated in field mice and house mice?—I believe not.

7877. It is quite different, is it not?—Yes.

7878. (*Chairman.*) Do you mean that it requires more, or that it gives the disease in one case and not in the other?—It is a different organism that produces the disease in the different animals.

7879. (*Sir William Collins.*) That is to say that one species of the same animal will not suffer whereas the other will after inoculation with the same poison?—Yes.

7880. You spoke of three points which Pasteur's experiments had established—first the specificity of the microbe, then its variation of virulence under cultivation, and so forth. It is not a fact that experience has shown that organisms originally asserted to be specific have been found by cultivation on various soils to undergo considerable morphological variation?—I think so.

7881. Is it true that many organisms declared to be the causes of certain diseases have subsequently been found to be not so?—I do not follow you.

7882. Take the case of sheep-pox, of which the alleged organism was at one time published with a considerable degree of confidence; was not that subsequently withdrawn?—Yes, and I believe different authorities have taken different views with regard to whether the same thing has been the cause of the disease or not. I believe that is the case now between a member of this Commission, if I may refer to him, and Dr. J. Hamilton.

7883. In reference to what disease?—In reference to the diseases in sheep, especially with regard to braxy and louping-ill, and so on.

7884. And I suppose Koch, by experiments on animals, was led to conclude that the tuberculosis of animals could not be communicated to man?—That was the report that he made here.

7885. As the result of his experiment upon animals?—Yes, but then, of course, there is the opposite view held from further investigations by the most recent Royal Commission in this country.

7886. You rather demurred to my suggestion that sapremia was to be regarded as a form of blood poisoning independent of a micro-organism?—Yes.

7887. Will you tell me what the micro-organism of sapremia is?—I do not think I can at this moment fix on any special micro-organisms.*

7888. In reply to the noble Chairman you stated, I think, that there were bacilli which worked by themselves without emitting any toxin as well as those which operated by way of toxin?—I meant to say there were bacilli whose toxin was the source of the poison and that there were others which by themselves were the source of the poison.

7889. Would you give an example of the latter class?—Tetanus is unquestionably an illustration of the toxin influence.

7890. It was the other class that I wanted?—I imagine that tubercle is largely due to the organism itself as the cause of the disease.

7891. I suppose that you would at any rate contend that there have been great advances in surgical practice which have been made irrespective of experiments on animals?—Yes.

7892. That follows from what you have already stated. The use of anæsthetics would be one of them, would it not?—Yes.

7893. Then the ligature—would that be one of them or not?—Yes, I think the ligature certainly would be. The ligature is a very old means of controlling hæmorrhage long before they thought of antiseptics.

7894. It was used by Ambrose Paré?—Yes, but it goes back to very early history; it was employed, if I remember rightly, by Celsus.

7895. You have drawn a distinction between, or, at any rate, you have mentioned aseptic and antiseptic surgery?—Yes.

7896. Was the late Mr. Lawson Tait one of the champions of the aseptic treatment?—Yes.

7897. Do you remember his views in regard to vivisection?—Oh, yes, I know that he was opposed to it, but then I must say that that would not influence me much.

7898. I was only anxious to know whether you would wish us to take it that what you stated in evidence was a uniformity of view of eminent surgeons or whether there were those who were really eminent surgeons who have taken a different view?—Yes, that cannot be denied, but they are very few in number as far as I know.

7899. Should I be right in thinking that aseptic surgery has taken the place of antiseptic surgery?—I hope so.

7900. You referred to the evidence of Sir James Paget in 1876; did you notice that in reply to one question, No. 382, he states, "I can quite believe that the ardent physiologists put more trust in the experiments on living animals than I should; and certainly those studying therapeutics and diseases think more of them than I should. I think more of the advantage of clinical inquiry. But I am very anxious that there should not be a general condemnation of experiments on animals since it seems to me that there are a number of things actually essential for the life of man, that cannot be ascertainel by other means." Could you associate yourself with that opinion?—To an extent. Certainly I think we all ought to attach the utmost importance to clinical inquiry; but I imagine that if Sir James Paget were giving evidence here to-day, instead of in 1876, he would attach even more importance than he does there to experiments on animals, from the discoveries which have been made since, and from the prospect of further discoveries that will be made.

7901. At any rate, your view would not be that, I suppose, of the late Sir Charles Bell, when he said: "Experiments have never been the means of discovery, and a survey of what has been attempted of late years in physiology will prove that the opening of living animals has done more to perpetuate error than to confirm the just views taken from the study of anatomy and natural motions"?—Although Sir Charles Bell stated that as his opinion at that time, it was not his opinion at other times, and I do not think it was the best representation of what his view really was. Sir Charles Bell made many experiments, and we know what we owe to them, but Sir Charles Bell was a man who felt very deeply the pain that these experiments caused to animals, and I have had from Sir Charles Bell's widow, and from his brother-in-law, the late Mr. Alexander Shaw, a statement that he felt it so strongly that he gave up performing experiments on animals because he hated the sight of the suffering, which they necessarily entailed. But, still, it was a good thing he performed them, because, as you know as well as I do, they were the starting point of our knowledge of the different functions of the nerves.

7902. As bearing upon what you have said, I see that Sir Charles Bell in a letter wrote as follows: "You may think me silly, but I cannot perfectly convince myself that I am authorised in nature or religion to do these cruelties." In other words, it is a conflict between the moral sentiment and scientific experiment?—And the desire to obtain knowledge from scientific investigations for the benefit of humanity. I think all those things entered into Sir Charles Bell's thoughts and feelings.

7903. (*Chairman.*) Were anæsthetics used at that time?—No, and at any rate they were useless to be used for such experiments as Sir Charles Bell resorted to. He wanted to learn essentially the differentiation in the functions of the nerves as between those which convey sensation and those which convey the impulse to muscular action, and he could not have discovered what he was seeking to arrive at had he not made experiments upon animals without anæsthetics at the time of the experiments; and of course, as they necessitated the exposure of the spinal cord, they were necessarily of a most torturing and suffering character.

* *They are the micro-organisms of putrefaction—the bacilli of putrefaction and different species of "Proteus."—H. M.*

7904. About what date would that be?—I suppose about 1820. I forget the date of Bell's death.

(*Sir William Collins.*) He died in 1842.

(*Sir William Church.*) 1811 was the date of his celebrated pamphlet, in which I think I am right in saying he himself attributed his discovery to the experiments he was making in 1811.

7904a. (*Sir Mackenzie Chalmers.*) Chloroform was discovered about 1846?—There again I cannot say the exact date.

7905. (*Sir William Collins.*) The quotations that I put to you are dated 1823 and 1822?—I said I thought it would be about 1826. I had forgotten the date of Sir Charles Bell's death, but it was after he went to Edinburgh, and he went there in 1836.

7906. There is little doubt, is there, that Sir Charles Bell did make experiments, and that he attributed a great increase of knowledge to those experiments, and that he subsequently wrote rather disparagingly of experiments on living animals, because he felt that they were to some extent in conflict with this sense of morality?—Yes, but that he did attach importance to them and that he did find out epoch-making knowledge by experiments I suppose is beyond all question in your mind or mine.

7907. I am afraid at the present moment I am interrogating?—I put that merely as a figure of speech. I suppose it is beyond all question—everybody must admit the epoch-making importance of his discoveries.

7908. Do you think that they need to be repeated?—No. I have only quite recently been asking for further information before signing one of the certificates and schedules as to the necessity of making certain experiments on the spinal cord; because I should not sign the certificate if it was a mere matter of repetition of experiments and for the first-hand knowledge of the individual who wanted to make them. I consider that those experiments ought not to be repeated unless with some very special object in view.

7909. Might I put it to you that *à priori* physiology, which is the science of living things, and surgery, which has so largely to do with wounds, are both likely to be benefited by knowledge obtained by operations on living animals?—I think that both surgery and physiology will be advanced by operations on living animals.

7910. *A priori* it would be very surprising that, physiology and surgery being what they are, standing in the relation that they do to the science of life and the healing of wounds, operations on living animals would not be productive of some additional knowledge by their practice?—It would be very unlikely.

7911. Do you think that *à posteriori* you are able to adduce evidence that that *à priori* assumption has been justified?—Absolutely.

7912. But I understood your view, and that of the Royal College of Surgeons, to be that there should be a limitation by law placed on the practice of vivisection?—Yes.

7913. (*Dr. Gaskell.*) Might I ask one question with regard to Sir Charles Bell's work. Did not Bell publish later a certain amount of material; could you not say that he published some early papers and some later papers?—Yes.

7914. Is it not the early papers that we now consider valuable?—Yes.

7915. I mean that what is really lasting in Bell's work is that part which he himself attributed to experiments on living animals?—Certainly.

7916. (*Sir William Collins.*) Would you give us the date as to what you mean by later and earlier?—I would give the date of the earlier papers as those when he was working largely by experiments on animals; I forget the dates.

7917. Do you think that the papers which were published in 1823 were not valuable?—Certainly they were; but I think those were not some of his earliest papers; he went on working here in London till 1836, and then afterwards for four or five years in Edinburgh.

7918. In the earlier papers you would include the papers of 1823?—Yes, although in answering these questions I would not by any means cast, or seem to wish to cast, any want of appreciation upon the later papers, because he wrote upon surgery; for instance, he wrote several treatises upon surgery which were the result of his experience as a surgeon later in life. But my answer to your question was chiefly in reference to those more scientific investigations.

7919. (*Dr. Gaskell.*) What I meant simply was that those investigations of Sir Charles Bell, which we look upon so highly now, and which have lasted right away to this time, were all done or put forward by him at the time, and in consequence of the experimental work that he was doing?—Yes; and then the later ones that he wrote more especially had reference to his practice as a surgeon.

7920. (*Sir William Collins.*) You attach value to them all?—Yes, I attach value to those, too, of course.

7921. (*Sir Mackenzie Chalmers.*) You have brought before us a unanimous resolution of the Council of the Royal College of Surgeons?—Yes.

7922. The practical effect of that is that your Council unanimously support the existing law?—Yes.

7923. With a possible modification in a small detail?—Yes.

7924. Would you kindly tell us how the Council is composed; what is the number of the Council?—There are 24 members of the Council.

7925. Elected by whom or chosen by whom?—Elected by the Fellows of the College.

7926. (*Sir William Collins.*) Not by the members?—Not by the members.

7927. (*Sir Mackenzie Chalmers.*) The Fellowship of the Royal College of Surgeons is the highest surgical degree in England, I suppose?—We think it is the highest surgical degree anywhere.

7928. At any rate the Council represent what I suppose might describe as the best surgical opinion in England?—I think I may fairly say that it does, although I am a member of it and therefore do not speak personally.

7929. You are President?—Yes.

7930. I am speaking of the Council?—Yes.

7931. We may take it as expressing practically the great body of instructed surgical opinion in England?—Yes; and I may add that they are all, or nearly every every one of them, men who have spent a large part of their lives in teaching surgery as well as in practising it.

7932. I may take it, then, that your opinion is that experiments on animals in the past have been indissolubly mixed up with advance in the knowledge of surgery?—Yes.

7933. And, although it may be difficult to assign the proportions, you think it is absolutely necessary that they should continue for the future?—Yes.

7934. How long have you been practising yourself as a surgeon; you are still practising, are you not?—Yes; my house surgeon appointment was 1867 or 1868.

7935. Your experience as a surgeon goes over 40 years?—Yes.

7936. And during the whole of that time you have been continuously operating and in the operating room?—Yes.

7937. I want to ask you, if I may, one or two questions with reference to suggestions made by previous witnesses. Have you any opinion on this point? When an anæsthetic is administered is there any order in which you can put the disappearance of movement, disappearance of sensation, and disappearance of consciousness?—Disappearance of consciousness long precedes disappearance of movement.

7938. That is what I wanted to know. By consciousness I meant intellectual consciousness, as opposed to mere sensation of pain?—I take them all. Of course, really intellectual consciousness would go first; then the consciousness of pain would go long before the capacity for movement went.

7939. The movements might be even what you call purposive?—Purposive in this way, that they would be made to claw off the thing over the mouth, or made to kick away a hand or any restraining force over the knee—purposive to that extent.

7940. What I meant by purposive was that a person knowing nothing about anæsthetics, seeing an operation for the first time would think the patient was conscious?—Yes, and would think he was suffering horribly. I daresay many people would. I have seen a person, who has not known anything about what was going on, and has not been able afterwards to tell anything about what had been going on, having to be held down by six or eight persons during the latter portion of the administration of the anæsthetic.

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7941. Before the operation began?—Before the operation began.

7942. But absolutely unconscious?—Absolutely; they have not known the least thing of it afterwards.

7943. Does it vary with different individuals?—Yes, because some individuals will not struggle at all—they go off very quietly.

7944. Does it depend upon the nature of the anæsthetic to some extent at all?—It depends upon many causes. One always used to think, especially at Guy's, that people who had taken alcohol and people who had syphilis—I know it was the opinion of one of my teachers there, Mr. Bader—that syphilitic persons were amongst the most troublesome under chloroform, that they resisted the most; I mean people who had had syphilis comparatively recently.

7945. Now, may I take you to the next stage. During the operative procedure, may you, without recovery of sensation of pain, have any movements?—Yes. When a patient is absolutely unconscious to everything and is as still as possible, when you put the knife on the surface of his skin and begin to make an incision, he may struggle or turn, so that you may even yourself turn round and ask the anæsthetist whether the patient is fully under, and he will say, "Oh, yes, quite," and you know that he is.

7946. Are there any special nerves or muscles which are apt to contract and cause movements when the anæsthesia is still complete?—One of the tests that one often sees employed—it is a rough test—by a teacher showing a witness who is learning the administration, is to touch the conjunctive of the eye with the finger. We surgeons know that a patient may be absolutely insensitive to the touch of the conjunctiva with the finger, and yet give a very considerable writhe directly the operation is about to commence or is commencing round the fundament, round the anus. That probably is the last part of the body, so far as one knows, to answer to the anæsthetic.

7947. I want to take you again to the operation. In In what order does full consciousness come back (consciousness and sensation)? Is there any recognised order?—I could not answer that in a precise way, but the return of consciousness is sometimes very much delayed. Patients resist the touch of the hand long before they are conscious; they resist your moving a limb, or putting it into a certain position.

7948. Yet there is no sensation of pain?—No sensation of pain. The sense of pain will come back perhaps sooner than the sense of consciousness does; a patient will feel a prick of pain sooner than he will be able to hold an intelligent conversation.

7949. But would the power of motion come back before the sensation of pain?—Yes, long before.

7950. Is there any appreciable interval?—It would vary. May I give an illustration? How long a patient will be before he appreciates pain may be judged of by those rare and most unfortunate accidents which have occurred with regard to hot water bottles. A nurse perhaps has put a hot water bottle against the patient's side or against the patient's limb, and he lies against it perhaps for an hour or two, until, when the misfortune is discovered, there is a very considerable burn, which may end in a very considerable slough.

7951. That is after the patient has been put back to bed?—Yes.

7952. So that there is a very considerable interval before full sensation of pain and full power of feeling returns, after a strong and full dose of an anæsthetic?—There is a considerable interval elapsing before the patient will make an effort to move away from a hot water bottle, for instance, which is producing such damage that a very large slough will ensue.

7953. Although the patient is capable of making movements?—Yes.

7954. So far as your experience goes in the administration of an anæsthetic to animals, a full dose has been used?—Yes, I have seen that.

7955. For the reason which you gave, that the worst that can happen is that the animal dies prematurely?—Yes.

7956. Whereas in the case of one of your patients, it is important that he should recover?—Yes.

7957. Have you any opinion as to whether there is any difference in intensity between animal and human sensibility?—I have not.

7958. There are no data perhaps?—I have not any data except one's general experience of the indifference with which dogs and animals will go about wounded, as compared with a human being. A dog will run about, or a horse go walking about, with the whole of its entrails hanging out; and provided that it does not affect their limbs, they will go about and eat with the greatest indifference.

7959. A dog directly after a good fight shakes himself and shows very small signs of pain, as a rule?—Yes, if it is merely a flesh tear, or something of that sort, or even if a cavity is opened.

7960. We had some evidence here the other day of a medical anti-vivisection witness, who suggested that experiments on animals might be deceptive because unconsciously the experimenter hypnotised the animal. Have you anything to say with regard to that?—That hypnotism has taken the place of an anæsthetic, do you mean?

7961. No, she said, as I understood her, that the experimenter got the result from the experiment which he desired because he hypnotised the animal?—I fear that is a subject upon which I could not follow the lady.

7962. At any rate, you never attributed the success of any surgical operation to that force?—No. There have, of course, been things done on certain people by hypnotism.

7963. By Dr. Braid, of Manchester, for instance?—Yes.

7964. As regards amendments of the law, do you think it would be right, in granting certificates, that particular forms of anæsthetics should be prescribed; or do you think that they might be safely left to the operator?—I think it might be safely left as it is under the Act now.

7965. You do not think that any amendment is required there?—I do not think so at all. The Act specifically states that curare must not be used.

7966. As an anæsthetic?—An an anæsthetic, alone.

7967. It is used not as an anæsthetic, but as an adjunct to the operation?—Yes. I do not think that any further restrictions on that head are required.

7968. I suppose it is not likely that local anæsthetics would be used in experiments on animals in any severe experiment?—It depends upon what you mean by local anæsthetics.

7969. I was thinking of cocaine or eucaine?—I think it is possible that there might be cases where an injection of cocaine or urethane into the theca of the spinal chords might take the place of a general anæsthetic.

7970. For the purpose of operations below that place?—I am not sure that it need be limited to below the theca.

7971. Have you yourself in your own practice used local anæsthetics?—No.

7972. You always use a general anæsthetic?—Yes; but there have been many cases of surgical operations of moment reported as having been quite successful under an injection of eucaine and cocaine.

7973. Successful from the point of anæsthetisation?—Yes; so that that is a thing that might easily arrive.

7974. I do not know whether you have seen a pamphlet by Surgeon-General Sir James Thornton?—What is it entitled?

7975. It is "The Principal Claims on Behalf of Vivisection: A Refutation"?—No, I have not seen it.

7976. He has taken in a very convenient form—in alphabetical order—various heads, and has put in a most convenient and concise way the objections to vivisection. Some of those heads are purely surgical. I do not know whether it would be fair to read a short extract and ask you your opinion of it?—I shall be happy to hear it.

7977. On page 1, may I take the opening sentence? "Abdominal surgery had been seriously retarded by experiments on living animals. The following statement was made by Sir Frederick Treves in his 'Address on Some Rudiments of Intestinal Surgery,' delivered before the Midland Medical Society at Birmingham, and reported in the British Medical Journal, November 5th, 1898:—'Many years ago I carried out on the Continent sundry operations upon the intestines of dogs, but such are the differences between the human and canine bowel that when I came to operate upon man I found I was much hampered by

my new experience, that I had everything to unlearn, and that my experiments had done little but unfit me to deal with the human intestines.'" Have you any comment to make on that?—Of course, I am familiar with that sort of quotation, but I do not hesitate to say that my opinion is absolutely opposed to it as a general statement. I hold that abdominal surgery has been enormously advanced by experiments on animals, and I would go so far as to say that there are many operations, such, for instance, as that which I referred to in regard to the extirpation of the kidney, which I do not think any prudent and cautious surgeon would have undertaken, if it had not been for the knowledge which has been gained by experiments on animals.

7978. The next alphabetical head, which deals with surgery, is on page 4 on "Acupressure." I do not know how far you would agree with what he says there. "Lawson Tait, F.R.C.S., who conducted the chief experiments on this subject, writes: 'The conclusions of the experiments were quoted far and wide, were translated into foreign languages, and everything looked as if "acupressure" was to reform the art of surgery. But it did not; it speedily died out, and, I think, has been almost forgotten. The explanation of this lay in the simple fact that the closure of a dog's artery is altogether a different process from that seen in the human vessel, and my experiments were not only needless, but they were absolutely misleading.'" Have you any comment to make upon that paragraph?—With regard to acupressure, I should say that I remember the time when it was introduced and when it was very considerably tried, and I should say that in many instances it was a better method of controlling hæmorrhage with safety than the ligature was. I remember when it was used for amputations of thighs, and so on; and it used to produce an exceedingly good effect. But we have now safer, more certain, and more convenient methods of controlling primary hæmorrhage after amputations, etc., than by means of these acupressure needles. Acupressure is never employed now for that reason.

7979. It has been superseded by a better method?—Yes.

7980. It was an improvement on the old method?—Yes.

7981. But it has now been superseded by newer and better methods?—Yes.

7982. Then he controverts exactly what you said in your *précis* about antiseptic surgery. I do not think I need trouble you with that, because you have given your evidence, and it is in opposition to Sir James Thornton's view. Would you kindly look at page 7—"Blood-pressure Experiments"—and would you say whether you agree with this passage quoted from Dr. Macaulay: "For practical application in medicine the numerous experiments made since the time of Hales are quite useless. The force of the heart, for example, varies in the animals inspected, and under different conditions; and the variations are infinite in different persons, in various conditions of age, strength, and state of health. The general estimates may be interesting as facts for philosophical statement, but are useless with any view of applying such experiments to use in maladies either of the sanguineous or nervous system. More useful information can be obtained by observing the force of the heart as indicated on the delicate dial of a balance-chair than from all the experiments of vivisectors." Will you make any comment on that that you think fit?—I have not had time to read these extracts and think them over in all points, but as I read this through here, I should suppose that the writer of it had very little experience of the experiments and results of experiments which have been made with regard to many drugs.

7983. Do you know the writer by name?—No.

7984. The writer is apparently Dr. Macaulay, in his Prize Essay in 1881. Do you know anything about Dr. Macaulay?—No, I cannot say that I do.

7985. He is not a known authority?—I do not know him.

7986. Sir James Thornton adopts it, I presume?—I was thinking that anyone who wrote it, or adopted it, had probably not had very much experience of experiments and the results of experiments on animals, as I mentioned.

7987. He next deals with brain experiments. Sir Victor Horsley will tell us about them?—Yes.

7988. Now, may I ask you to look at page 17, because that touches your evidence. "Kidney Removal. It has been asserted that the operation for removal of a diseased kidney was devised through experiments on dogs, but this claim cannot be sustained, as the operation in question is very ancient, and was done in the time of Hippocrates. It has long been known to pathologists that a person may live with one kidney practically useless; no experiments, therefore, were required to prove it, especially as there have been rare instances of congenital absence of one kidney." You have already dealt with this?—Yes, I have referred to it in my *précis*.

7989. Would you like to make any comment on that passage?—It is quite true that experiments have been made many years ago. I referred to them in some little detail in some lectures* I gave some years ago. Simon probably was aware of them—it is possible that he was not; but I think that even if he had been aware of them, they were not of such a date and of such a character that he would have felt justified in basing upon them the operation which he did upon the first patient of his.

7990. And since his operation, it has become an operation carried out from time to time?—Yes, nearly every week, I should say.

7991. It is an operation you have done yourself?—Very many times.

7992. Then there is rather a long attack on Pasteur's anti-rabic inoculations. Sir James Thornton suggests an alternative treatment, such as the Buisson baths. Have you any opinion to express upon that?—I have a great opinion of the anti-rabic treatment, but I know nothing about the other.

7993. If a patient of yours was bitten by a dog that you believed to be mad, would you take the responsibility of advising him to undergo Pasteur's treatment?—Yes, certainly.

7994. Without hesitation?—Without hesitation. But that is assuming that it was actually a rabid dog.

7995. I say a dog reasonably believed to be mad?—Yes.

7996. (*Dr. Gaskell.*) I wanted just to ask you whether in conditions of light anæsthesia, such as a dentist uses, and so on, you may not often get movements, and even cries, and the patients afterwards say that they felt no pain?—Yes.

7997. Not only movements, but actual cries at the moment when the operation is done?—That is so.

7998. I have seen a case with Sir George Murray Humphrey at Addenbrookes Hospital, in Cambridge, in which he removed a large fatty tumour. While the operation was going on, the patient came round, and conversed with me, and said he was very comfortable and felt no pain, although he knew exactly what was going on; and after the operation he jumped off the table and walked off to the ward by himself. Have you ever seen anything of that sort during anæsthesia when the patient apparently was entirely conscious and yet felt no pain?—No, I do not recall a parallel case.

7999. But have you not ever heard that the patient has said afterwards, "I knew what you were doing"?—Yes, I have heard that very often.

8000. And that means a certain amount of consciousness?—I suppose the patient would even have been conscious. I might perhaps give you an instance. At the end of an operation, perhaps it occurs to me before putting a dressing on the wound that I had better put another suture, another stitch, into the wound. The patient is now coming round, and gives a little cry; and afterwards, when he has been asked whether he cried because he felt the pain, he has forgotten all about it if he had felt it.

8001. So that really one may take it that pain is one of the first things to go?—Yes.

8002. Even although there is still a certain amount of consciousness left in many cases?—Yes.

8003. Another point that I want to ask you about is with respect to manual skill. The Act says that no one shall be licensed under it for the purpose of acquiring manual skill?—Yes.

* "*The Origin and Progress of Renal Surgery*"—*Hunterian Lectures, 1898.*—*H.M.*

Mr. H. Morris, P.R.C.S.

8 May 1907.

8004. Then I understood you to say that in the case of surgeons it might be advisable sometimes for them to perform an operation on an animal before performing on a human being?—Yes; but I would not say that from the point of view of his acquiring general manual skill, but from the point of view of learning the technique of some particular operation.

8005. But that at present is illegal, is it not; the Act would forbid you to obtain that necessary skill?—Yes.

8006. Would you not, therefore, as representing the Royal College of Surgeons, like to see the Act altered in that respect?—I am not quite sure whether the Act does forbid it.

8007. To obtain the technique, I said?—Yes, not manual skill; that is forbidden. To give an illustration: Supposing you are going to cut a cancer out of the bowel, and are going to remove it altogether; you have to cut the bowel above and below the site of the cancer widely, so as to take it all away; then you have to join those two ends of the bowel up. How are you going to do it? Will you stitch up one end entirely and graft the other end into the bowel above, or will you join them end to end? That is the kind of thing I meant; and you can find it out by experiments on animals; and you might find that out, I think, under the Act, under licence and Certificate B. That is a thing of which it is very desirable that we should be able to learn the technique in that way, or to test it in that way. But that is not what I thought you meant. I think the Act means that experimentation is not to be employed for learning manual skill on the part of a young surgeon.

8008. It does not say anything of the kind; it says manual skill?—I always read it in that way.

8009. One final question with respect to demonstrations. Have you read "The Shambles of Science"?—Oh, yes.

8010. Do you consider that for advanced students who are likely to be teachers of physiology, and are likely to hold teaching positions in some way in connection with medicine, the kind of demonstrations spoken of there ought to be forbidden?—I cannot call to mind all those demonstrations.

8011. They were demonstrations showing comparatively new discoveries in physiology; not demonstrations that you would give to young students first coming in to study physiology, but deliberately given as showing the latest results of experimental work in physiology?—I think that those who are going to be practical teachers of physiology ought to have an opportunity of witnessing many experiments, and perhaps doing experiments, which are not necessary to be shown to ordinary students—if that is your point.

8012. Yes. It has been said that those experiments ought not to be allowed, and hardly are allowed under the Act?—I cannot answer that question, because I do not know what those experiments are.

8013. I mean those experiments which are described in the "Shambles of Science"?—I have not got them sufficiently in mind.

8014. But they are experiments for advanced students showing the newest things in physiology?—I think, as I have said already, that many experiments ought to be permitted to those who are going to teach and practice physiology which ought not to be permitted, or ought not to be shown,, to ordinary general students.

8015. Which would be useless for ordinary students?—Yes.

8016. (*Mr. Tomkinson.*) You spoke of the valuable results of surgical demonstrations in regard to observing the action of the heart and entrails?—Those we call physiological experiments; they are not surgical. They do not come into any part of either surgical teaching or practice.

8017. I wish to ask you, would the action of those organs be affected by the fact of the animal being under anæsthetics?—Not sufficiently to prevent the observer getting a true and accurate idea of what goes on.

8018. With regard to the completeness of anæsthesia and the use of curare, you deprecate the use of curare alone?—Yes, I do; we do not know absolutely for certain everything about it.

8019. Do you think that, administered with chloroform or ether and other real anæsthetics, there may not be a danger of the animal being powerless through the effect of the curare, and yet the anæsthesia would be imperfect at the time?—I do not think so at all. I am aware to what you refer, of course—that the animal being under the influence of curare cannot move even though it may suffer; but I think, from the way in which I have always seen and have always heard that chloroform or ether is administered, there is not the least fear of that.

8020. You are absolutely satisfied in your own mind that complete anæsthesia is always aimed at most carefully and in your opinion secured?—Yes, I do think so; and certainly I have always seen it.

8021. Little has been said about the paragraph in your *précis* on the question of shock. May I ask whether you have seen that pamphlet giving a description of 16 experiments by Dr. George Crile upon animals?—Yes, I have read it a long time ago.

8022. The pamphlet excited a good deal of attention and, I may say, horror among a great many people, from the tremendous severity of the operations to which the dogs were subjected?—Yes.

8023. In your opinion were the results, from any point of view, at all commensurate with the severity of such operations on the observation of shock or the effect of shock?—I hardly feel that I can form a just opinion about that. I did not like many of the experiments that were performed, but I would not like to express a definite opinion.

8024. You hardly feel prepared to justify them altogether?—No; and I would hardly be prepared to say that they were not desirable to have been made once, but I certainly think that such things ought not to be repeated for the sake of repeating them.

8025. Provided that complete anæsthesia can be secured, I suppose you would be opposed and object to any really painful experiment without it?—I would not go quite so far as that; because, if I may again refer to the experiments which were made by Sir Charles Bell many years ago, it is possible that some investigations involving more or less pain might be very desirable; but as a rule I should certainly say so.

8026. Could you express any opinion as to whether from the moral point of view it would be justifiable to inflict really painful experiments for possible results in search of alleviating human suffering or the prevention of it?—That is a matter upon which of course each individual will have his own opinion. As a great lover of dogs, and as a person very desirous of saving animal suffering, I should still say that I thought that from the moral aspect it was quite justifiable.

8027. (*Dr. Wilson.*) There is only one question that I should like to put to you. Are you of opinion that a very severe and prolonged operation can be performed on an animal under narcosis alone, under morphia for example, or paraldehyde or eucaine, an operation involving the opening of the abdomen, the thorax, and so on, to continue, say, for an hour or an hour and a half or longer?—Under the conditions suggested by Sir William Church I certainly think that an operation could be performed under morphia alone without the animal feeling any pain.

8028. You think that the animal could live even through that period if it had a poisonous dose?—I think so.

NINETEENTH DAY.

Wednesday, 15*th May* 1907.

PRESENT:

Sir W. S. Church, Bart., K.C.B., M.D. (*in the Chair*).

Colonel the Right Hon. A. M. Lockwood, C.V.O., M.P.*
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir J. McFadyean, M.B.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. J. Tomkinson, M.P.
Mr. G. Wilson, LL.D., M.D.
Captain C. Bigham, C.M.G. (*Secretary*).

Major Leonard Rogers, M.D., F.R.C.P., I.M.S., called in; and Examined.

Major L. Rogers M.D., F.R.C.P., I.M.S.
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8029. (*Sir William Church.*) You are a Fellow of the Royal College of Physicians and Professor of Pathology at the Medical College, Calcutta?—Yes.

8030. And you are a Major in the Indian Medical Service?—Yes.

8031. You consider that our present knowledge of the physiological action, and of the antidotes of snake poisons, is founded entirely on the results of experiments on animals?—Yes.

8032. Will you tell us on what grounds you base that opinion?—The most important early experiments are those of Sir Joseph Fayrer and Sir Lauder Brunton on cobra venom, which showed that the cobra venom kills by producing paralysis of the respiratory centres, and has subsidiary actions, but that that is the principal action and the primary cause of death. As the result of those experiments, they carried on artificial respiration, and they found that they could keep animals alive for a large number of hours, even up to twenty-four hours after paralysis of respiration had taken place. And subsequently, a further Commission worked in India, and suggested the practical value of this method, and they succeeded in recovering a few animals after many hours of artificial respiration. And there have been a few cases on record, one in Ceylon in particular, where this method appears to have been successful in saving the lives of human beings who had been bitten by cobras; but, unfortunately, it only works if the dose of cobra venom is comparatively small, only just a lethal dose, but if the cobra injects several times a fatal dose it is not possible to recover the patient by that method.

8033. Have further experiments shown that it is probable that intravascular clotting also is part of the cause of death, besides the paralysis of respiration?—That is, in the case of Russell's viper, and viperine poisons. The viperine class of poisons have a totally different action from that of the cobra class, that is the poisonous colubrines. It was first shown by Professor C. J. Martin, now of the Lister Institute, that one of the snakes in Australia caused intravascular clotting, and he suggested that this was probably the physiological action of the viperine class; and it was afterwards shown by Lamb in Bombay, that Russell's viper does cause death in that way. Four years ago I investigated this subject at the physiological laboratory at the London University with Dr. Waller, and I tested vipers, both Russell's viper of India, the rattlesnake of America, and the African puff adder, and I found that, in addition to this intravascular clotting you also got paralysis of the vaso-motor centre, and that that is the most essential cause of death in the case of viperine snake poisons. I also found that it could be counteracted to a considerable extent by the action of drugs, especially adrenalin, which contract the vessels and so counteract the fall of blood pressure. I published the experiments in a paper read before the Royal Society, and illustrated my point with tracings. So that our knowledge of the viperine poison's physiological action is also due entirely to experiments on animals.

8034. You wish also to refer to some experiments of Lille and Sir Thomas Fraser?—Yes. The next point in the action of snake poisons is the question of the action of antivenins, first discovered by Calmette of Lille, and independently by Sir Thomas Fraser. They found, as the result of repeated inoculation of cobra venom into animals such as horses over a period of many months, taking usually eighteen months, that they were able to produce a powerful serum or antidote in the blood of the infected horses, which has a definite specific action against the cobra venom; so that after injection of the cobra venom into an animal it is possible to save that animal's life by the injection into a different part of the body of a proper dose of the antivenin. In some experiments which I have done on this question, too, I found that the results are very much better if the antidote is injected intravenously. If the anti-cobra serum is injected intravenously it has exactly the same action upon the system of the animal that it has if mixed with cobra venom before injection; but if it is injected subcutaneously, it is absorbed more slowly, and does not act so efficiently. There is one difficulty with regard to the practical application of this treatment, which is, that the serum made from cobra venom does not act at all against the venoms of the viperine class, which have a different physiological action; and, further, the antivenin made from cobra venom only has a slight action against other colubrine poisons, such as the krait or the hamadryad.

8035. Therefore, although the antivenin has a decided protective influence, it is not practically of very great use?—No.

8036. Because it is not at hand?—It is not on hand, for one condition; you seldom have both the antivenin and the syringe on the spot; and even if you have it really requires much larger doses than are usually given.

8037. (*Sir Mackenzie Chalmers.*) And a very skilled operator?—Yes.

8038. No village man could do it?—No, I do not think so. There have been a number of cases reported where it has apparently had a good effect.

* *Col. Lockwood came in late during the sitting, and Sir W. Church therefore remained in the Chair.*

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8039. Where a doctor happens to have been near?—Yes. The best example was the case of Captain Lamb. He was investigating snake poison in Bombay and he was bitten by a cobra in the thumb, one fang entering. They were testing antivenin at the time, and the antivenin which they had they found was not strong enough to be of much use, so that they had to send for some more. It was an hour before they got an efficient serum, and by that time Captain Lamb was beginning to show symptoms of drowsiness and early symptoms of cobra poisoning. After an injection of the antivenin he recovered perfectly well with some local sloughing, which you always get after such poisoning. That is a definite case where antivenin appears to have saved life.

8040. (*Sir William Church.*) How long ago did you take up the subject?—I was working first at the sea snake poisons in Calcutta, in 1901, when I found that they were very poisonous, and that their venom had the same action as the cobra venom. Then when I was in London in 1903 I was working at the physiological action of the sea snakes and the viperine poisons which I have mentioned; and then Sir Lauder Brunton suggested to me the carrying out of experiments to test the value of the method he had previously suggested of a local application of permanganate of potash by incising the seat of the bite, or the injection of venom and rubbing in the crystals.

8041. Sir Lauder Brunton and Sir Joseph Fayrer, I think, found that mixing a solution of permanganate of potash with cobra venom destroyed its activity?—Yes.

8042. And then did he request you to continue the experiments with permanganate of potash?—He requested me to continue the experiments. He had previously published the suggestion of making the snake lancet, and it had actually been made; but as it had not been thoroughly tested and proved, it had not come into practice; and he asked me to take the matter up at that point. The first thing I did was to test the action of permanganate of potash on a number of different venoms, as it was previously only known that it destroyed cobra venom. I tested it against all the Indian colubrine poisons, the krait venom, the hamadryad and the banded krait, and the sea snakes, and also against Russell's viper of India, the rattlesnake of America, and the African puff adder; and I found that mixing a strong solution of permanganate of potash with any of these venoms would destroy the venoms in almost the same weight as the permanganate of potash; so that it has a very powerful action. It appears to act by oxydising these substances, which are all albumoses, which are an unstable form of albumen, into a harmless substance.

8043. Do you know at all why Sir Lauder Brunton and Sir Joseph Fayrer did not carry on their experiments with permanganate of potash?—I do not know the details of it. Sir Lauder Brunton told me that it was owing to the passing of the Anti-vivisection Act that his experiments were stopped—he was unable to go on. At that time he was trying injections of a solution of permanganate of potash as a form of treatment; but that apparently is not an efficient method of using it. What he told me was that his experiments were stopped by the passing of the Act. Then next I started carrying out experiments on animals as nearly as possible under conditions in which the permanganate of potash would be used in actual practice. I used rabbits and cats.

8044. You say that you carried out the experiments on animals as nearly as possible under the conditions in which the permanganate of potash would be used; but these animals, I gather, were under an anæsthetic?—They were chloroformed. There was a difference in that point. The animals were all chloroformed, then a measured dose of cobra venom was given in accordance with the weight of the animal, so many lethal doses, or, rather, so many times a fatal dose. The injections were made a little above the foot. Ordinarily snake bites are on the hand or foot, as a rule. After the measured time had elapsed, varying with the different experiments, a ligature was applied above the seat of the injection, in order to stop the circulation. An incision was then made at the seat of the injection with a small lancet—the actual lancet which is now being sold in India, and at the seat of the injection within 30 seconds I found that there was always a small blood-stained effusion, which is of practical importance, at any rate, as it shows the exact seat of the venom; a few permanganate crystals were then poured into the wound and rubbed in until the wound was completely blackened. This took one or two minutes. The ligature was then immediately removed on the supposition that the poison had been destroyed by the application of the permanganate of potash. The cats were then allowed to come round—I had reason, of course, for doing that—and kept from five days to a week. They appeared to suffer no pain; in fact, the only difficulty I had with them was that they all wanted to be petted at the same time if I went to see them. As the result of the experiments, I found that after the injection of ten times a fatal dose of cobra venom I could save the animal by operating 30 seconds afterwards. That is about the time it takes to apply the ligature if a person is bitten in a limb. I lost one animal after the injection of five times a fatal dose (it was the first animal done), the application not having been efficiently carried out as in the latter case; but its life was very greatly prolonged as compared with the control animal injected with the same dose without the treatment. Then I used decreasing doses of venom at longer intervals, and I have now worked up to getting a recovery in the case of a cat not treated until half an hour after the injection of a double lethal dose.

8045. (*Sir Mackenzie Chalmers.*) By treating, do you mean the ligature?—The ligature and the permanganate treatment.

8046. They are contemporaneous practically?—Yes.

8047. When you talked of 30 seconds, what did that mean?—That means the application of the ligature.

8048. The other would be within half a minute?—Yes, as soon as possible. The time limit is the time when the ligature is applied which gives you time during which there has been a chance for the absorption of the venom.

8049. In the case of a human being, could you apply a ligature?—Yes, as a matter of fact, the natives almost invariably do it. Similarly I tested with Russell's viper as a type of the viperine class, and there I have got recoveries up to five times a fatal dose, which is more than any viper I know of can inject, judging from the amounts I have obtained from these snakes, in proportion to the weight of a man. That is the experimental basis of the method.

8050. (*Sir William Church.*) And this method has been successfully applied to human being?—Yes. Now with the printed instructions giving directions for the use of the lancet, supplied with each lancet, I ask to have reports sent to me; and I have received a number of reports during the last three years, which I found it necessary to classify in two different classes. In judging of the action of any remedy in the case of snake bite there are a great many fallacies to be avoided. In the first place, the patient may not have been bitten by a poisonous snake; and in the second place, he may not have received a fatal dose; consequently it is owing to this that so many remedies have been brought forward as cures for snake bite which have been found experimentally not to be efficient. In order to try and get absolutely accurate figures I have taken in my first class only the cases in which the snake has been killed and identified as a poisonous one, and in which there were definite fang marks where undoubtedly the patient had been bitten by a poisonous snake.

8051. May I ask in what way the snake has been identified as a poisonous one?—The snake has been identified as a poisonous one by the hospital assistant, in some cases by qualified medical men. Many years ago as the result of Fayrer's work the Government of India provided coloured plates of different poisonous snakes to all the dispensaries in India; so that these men are trained to recognise these snakes, and I think with the great majority of these cases certainly the identification has been undoubtedly accurate. I cannot vouch for it personally, but that is as near as I can get.

8052. (*Sir Mackenzie Chalmers.*) You mean that the snake is killed and brought to the dispensary?—The snake is killed and brought to the dispensary and identified by the assistant surgeon, who has been trained and has these coloured plates at his disposal.

8053. (*Sir William Church.*) The identification of a poisonous snake is not a difficult matter, is it?—No, not to these men who have the coloured plates, and have been trained to it.

8054. No non-poisonous snake has got the character

istic fangs, has it?—No, the fangs are quite definite, and then, again, in nearly all the cases I have got a definite note of there were two definite fang marks, punctures which would not be produced by anything but a poisonous snake. Among these 14 cases where the snake was killed and identified, nine were cobras, three Russell's vipers, and two kraits, these being the commonest very poisonous snakes in India. The large number of cobra cases is important, because in measuring the amount of cobra venom that one can obtain from a freshly-caught cobra, the amount averages ten times a fatal dose for a man; so that it is exceedingly unlikely that many of these cobras can have injected less than one fatal dose, and it is very unlikely that more than at any rate one or two of these patients had not received a fatal dose of venom. Among these 14 cases there was one death, and this was a woman, who was brought to the hospital eleven hours after the infliction of the bite, when she was quite moribund. She had been bitten at 3 o'clock in the morning, and was brought to the hospital in the middle of the next day; and in that case, of course, no treatment could be expected to have any effect. On the other hand, there was one case of a cobra bite in which the patient was not treated until four hours afterwards, when definite symptoms had set in, and from my experience of experimental work I know that any animal which shows symptoms within four hours of injection of cobra venom will always die; so that I have no doubt that this man's life was saved by the treatment. It is also well known that an animal may recover after well-marked symptoms have set in; if you give just a fatal dose, or rather a dose which is just not fatal, the animal may actually remain paralysed for 24 or even 48 hours, and still recover. So that it does not follow that after symptoms have set in you may not save your patient; it entirely depends upon whether a fatal dose has already entered the circulation; if not, by destroying all venom locally which is not absorbed, you prevent a fatal dose being absorbed, and so may save your patient after symptoms have set in.

8055. (*Mr. Ram.*) In that case, of course, the symptoms showed, I understand, that beyond all doubt the cobra was the cause of the bite?—I think so, from the description.

8056. There is nothing to show, I suppose, whether the dose was a fatal dose or not?—Nothing more than the fact that the symptoms had set in within four hours; and I know that in animals if symptoms set in within four hours they never recover. If you give a non-fatal dose you often get no symptoms in less than 12 hours.

8057. (*Sir Mackenzie Chalmers.*) What is the ordinary period within which symptoms set in in the case of the cobra?—It depends entirely on the dose. If the cobra injects its full quantity of ten times a fatal dose, you get symptoms within half an hour, not, I think, much earlier; so that does give you ample time.

8058. You have half an hour's grace?—Yes.

8059. (*Mr. Ram.*) In the worst cases?—Yes.

8060. (*Dr. Wilson.*) Had the natives applied the ligature in these cases before they were brought to you?—Yes, in a number of them they had. In a good many of these cases the ligature had been applied.

8061. Do you know in what proportion?—I have notes of some; I have not been able to get full details of all the cases. I have corresponded with regard to them so far as I could. I have four cases here where the ligature was applied at once.

8062. Before they came to you?—Yes, before they were brought for treatment, and that, of course, makes it much more favourable.

8063. (*Sir William Church.*) In the 34 cases in which the snake was not killed and identified, was there evidence of any of those being bitten by a poisonous snake?—In a number of those cases it was reported that the snake was actually named as a cobra, but the snake was not killed; so that I thought it better to exclude them.

8064. (*Sir Mackenzie Chalmers.*) The natives know the cobra very well?—Yes, but I wanted to be on the safe side.

8065. It is not an easy snake to mistake for any other?—No. One difficulty is that even after a cobra has bitten a native they will not kill it very often; so that a number of cases in that 34 were certainly bitten by cobras, but as the snake was not killed I put them in the other class.

8066. (*Sir William Church.*) Six of those cases were fatal?—Yes. One was not treated till nine hours afterwards, and three others from four to six hours after; but one observer reported three cases treated two hours after which died. That is the only record of any case which has failed that I know of; but the same observer reported 20 successful cases.

8067. You go on to say that it is unlikely that such a large number would have been saved—that is, 24 out of 30—without the use of the permanganate locally applied; and then you go on to say that in some of these cases it was successfully applied by persons without any medical training?—That ought also to have applied, of course, to the first series. It applies to both series.

8068. It applies to all?—It applies to all. There was one case of a Russell's viper treated by a European some thousand miles away in the north of Siam.

8069. You do not think that without your experiment on animals it would have occurred to anyone to use this treatment?—Sir Lauder Brunton had actually had the lancet made, and had described it in his book on "Therapeutics."

8070. He arrived at it in seeing the effect of mixing permanganate of potash with cobra venom before he injected it into the animal?—Yes. There have been thousands of remedies brought forward supposed to have been used in cases, but that evidence is so absolutely unreliable that without the experiments, I think, it would have been impossible to establish any form of treatment; and this treatment was undoubtedly established as the result of experimental work on animals.

8071. Could you inform the Commission at all what the mortality from snake bite in India is?—About 20,000 cases are reported every year in India, and 10,000 of these, just about half, occur in Bengal alone.

8072. (*Mr. Tomkinson.*) Fatal cases?—Yes, those are really fatal cases. Of course, you cannot look upon these as absolutely reliable statistics, but I think they give a very fair idea.

8073. (*Sir Mackenzie Chalmers.*) And year by year they are very much the same?—Year by year they are very much the same.

8074. (*Sir William Church.*) You do not think that a large number of deaths not due to snake poison are put down in the returns to snake poison?—I do not think so. I should think it is more likely the other way, because I know from experience that over 90 per cent. of deaths are returned as fever, and I have known any disease returned as fever. The ordinary tendency for the village man is to return everything as fever; in fact, I have myself met with cases of snake bite recorded as fever.

8075. So that a sudden and unexpected or suspicious death is not put down to snake bite, which has often been stated to be the case?—That is a conceivable thing; but I think, taking the whole cases, it is more likely that they are underestimated than overestimated.

8076. (*Mr. Ram.*) Who makes the return?—The return is made by the Headman of the village in different parts of India.

8077. Is he bound to make a return?—Yes.

8078. Of every death?—Yes. He is not a medical man.

8079. (*Sir Mackenzie Chalmers.*) He is a registrar?—Yes, in different parts of India he is sometimes called the Chokidor.

8080. (*Sir William Church.*) Can you give the Commission any idea of the amount of loss of life caused by snake bite?—I do not know the exact figures, but I know that there are many thousands a year returned as due to snake bite.

8081. Perhaps some members of the Commission would like to ask you some questions with regard to snake bite before we go on to the second part of your *précis*?—If you please.

8082. (*Sir William Collins.*) Do I rightly understand that the nature of the poison in these cases of snake bite is a chemical one?—What do you mean exactly by chemical?

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8083. Is it due to an organism?—No; it is chemical in that sense.

8084. And I think you said that it was due to albumoses?—Yes.

8085. Are they substances which are destroyed by oxidation?—Yes.

8086. And is the principle of the utilisation of the permanganate of potash to act as an oxydising agent upon the chemical poison?—The permanganate of potash was first used before the nature of the poisons was known. In Sir Lauder Brunton's time the composition of snake poison was not generally known. That is my suggestion, that it acts as an oxydiser.

8087. It is the essential principle of Condy's Fluid, is it not?—Yes.

8088. Are all cases of snake bite by poisonous snakes in the human subject fatal?—No, by no means.

8089. Can you give us any idea what proportion of cases are fatal?—It is very difficult to get evidence of that.

8090. Assuming a bite, by an admittedly poisonous snake, in a human subject, what proportion of cases would be fatal?—I should say a very large proportion, certainly 75 per cent.

8091. Do you think we might say that 25 per cent. would recover?—I do not think necessarily so. I say that at least 75 per cent. die, and probably a great many more. I am very anxious not to exaggerate.

8092. And even if symptoms have supervened may recovery occur?—Yes.

8093. Did I rightly understand you to say that a ligature was the regular mode of treatment?—The natives use a ligature almost invariably—very often indeed.

8094. Do you regard the ligature as a successful mode of treatment?—Not by itself; it would delay death, but would not save a man's life.

8095. Do you think that in any case it would preclude a fatal result?—I do not think so. I tried an experiment on it once by ligaturing the wound and opening it again after a time, and letting a little poison in at a time.

8096. Did you try a series of control experiments with the ligature without injection, and a series with the ligature with injection?—Yes; it delays death, but I never got any result of preventing death.

8097. Have you published those results?—No; they were done at Rawal Pindi the first year I was out. I did not get any practical results, so that I did not publish them.

8098. You are in the Indian Medical Service?—I am.

8099. Did you make reports annually to the Government?—Those were entirely private experiments of my own; I was in military employ, and I did them in my spare time.

8100. Have you in the course of any official report made a reference to these experiments?—No; all my papers have been published either by the Royal Society or in the medical journals. Most of my work was done in my spare time, mostly when I was on leave.

8101. How many cases of bite from a poisonous snake have you seen yourself in human beings?—One.

8102. Was the remedy applied in that case?—The remedy was applied by me in a few minutes, and the patients had no bad systems—that was in Calcutta.

8103. And in the cases which you have brought before us—this series of fourteen cases with one death—was the poison from the snake tested upon animals?—No, certainly not. How could it have been? There was no opportunity of doing so. These cases have all been reported to me by different medical men in different parts of the country, and there was no reason for doing it. It is all a question of how much the snake injected. If you make it bite another animal, it may have exhausted its venom, and you will prove nothing.

8104. You think it would be superfluous?—Absolutely. If a snake has bitten an animal and has exhaused its venom, if you made it bite a dog, and it did not kill it, that would not prove that it had not killed a man in the first instance.

8105. I should have thought that the experiments might have been considered of value; but I am quite prepared to take your opinion?—If I had been on the spot to do it, I should have been very glad; but there is no doubt how very uniform the strength of cobra venom is.

8106. Do I correctly understand that in your second series of cases there were 34 cases and six deaths, as against 14 cases and one death in your first series?—Yes, so it happens.

8107. To what do you attribute the excess of mortality in the second series as against the first?—Because in the second series there were three cases which were not treated for four hours or more after the bite; and but one case occurred which was treated at a later time than that in the first series.

8108. But the distinction between the two classes is, that in the first the snake was identified, and in the second it was not?—That is one distinction; but there is the further distinction which I have mentioned, that in two fatal cases in the second series the patient did not come under treatment till much later than those which recovered in the first series, which makes an essential difference in the mortality.

8109. Has the Government of India adopted the method of using the permanganate of potash?—When the method was first brought out they declined to adopt it on Sir Joseph Fayrer's recommendation; but since these results have been published they are now widely distributing the lancet in every village in several provinces, having been convinced of the utility of the method.

8110. At the public expense?—Yes.

8111. Is it being sold as well as supplied?—Yes, over 7,000 lancets are sold by one firm in Calcutta. Every European going shooting, and that sort of thing, practically invariably takes one.

8112. How long has that distribution been going on?—It is only three years ago since those results were published, and it is only last year that the Government have taken it up extensively. The previous distribution was entirely by sale through chemists.

8113. Is it too early yet to notice whether it has had any effect upon the mortality returns?—Certainly it is much to early.

8114. Do you institute any comparison between the method of Calmette and this method of permanganate?—The two should be combined. This is practically a method which applies to every kind of snake. Calmette's method does not, because the antivenin which is made for cobra is only useful for cobra. An antivenin has been made for Russell's viper recently, but it will only work for Russell's viper; and, further, the amount of sera required is very large, larger than is often practicable; but if the permanganate treatment is being used to destroy all the venom remaining locally at the seat of injection, then the use of antivenin becomes much more likely to save life, as it would have only to act against a much smaller amount of venom; so that if you happen to get a patient who has just absorbed a fatal dose and so that the permanganate treatment would be too late to save life, by destroying all the venom remaining locally by the permanganate method you could then save that patient's life by a comparatively small dose of the antivenin. So that the permangante mode really makes the antivenin one much more practicable.

8115. Then by the combined method, the local use of permangante crystals and subsequent injection of Calmette's serum, would you be able to say within a certain number of hours subsequent to the onset of symptoms, that you might deal with perfect success with a case of snake bite?—I think you could if you identified your snake, so that you knew what serum to use, and if you had got that particular serum available, which unfortunately is seldom the case.

8116. There is no polyvalent serum?—Not yet; they are making one for both cobra and Russell's viper at the Pasteur Institute at Kasauli.

8117. I suppose that the production of these sera is the result of experiments on animals?—Entirely.

8118. Seeing that snake poison is not due to an organism, by what mode is the antitoxin or vaccine or serum prepared?—It is prepared by injecting the measured doses of the venom which is obtained from the snakes and then dried, and in the dried condition it can be kept for 30 years or more without losing its strength; then measured quantities of this are injected, beginning with much less than a fatal dose and gradually increasing it until many times a fatal dose can

be injected into the animal; and this causes the production of the antidote in the horse's blood, just as after the injection of the diphtheria toxin in the case of the antidiphtheritic serum.

8119. Then is the antitoxin obtained from a horse by injection of an attenuated dose of the original poison?—Beginning with an attenuated dose, going on to very large doses.

8120. (*Sir Mackenzie Chalmers.*) The animal becomes tolerant as you go on?—Yes, and you may give a hundred times a fatal dose.

8121. (*Sir William Collins.*) It has been stated in the course of evidence before the Commission that the injection of a solution of permanganate of potash was not efficient?—It has not been found so.

8122. Whose experiments were those?—Sir James Fayrer and Sir Lauder Brunton's experiments were the original ones, and some have been done by other people as well. There was a paper published by Brieger a short time ago, but he used a very weak solution.

8123. Do you mean that the animal died in spite of the use of the injection?—Yes. Another great disadvantage of the injection is that you must have a syringe, which is not available under ordinary conditions; whereas the snake lancet is an inch long and can be carried in the pocket.

8124. But apart from practicability, I understand you to state that the use of the solution by injection has been a failure, whereas the use of the crystal is found sufficient?—We have not got many successful cases from the use of solutions, but I think that is largely due to their not having been used strong enough. I think that possibly a stronger solution might be effective, but it is much less practicable.

8125. An injection of Condy's Fluid?—Yes; it is certainly much more difficult to destroy the venom in that way. By rubbing in the crystals you get much more chemical action; by injecting subcutaneously it is liable to be absorbed.

8126. I take it that it must be a solution of the crystal in the blood or the lymph?—Yes, but it is a much more strong solution.

8127. But you do not attribute virtue to the dry crystals?—No, it has to be dissolved in the serum. I think that a very strong solution would be effective; but it is nothing like as practicable. It must act in solution, of course. I think it is purely a matter of dose.

8128. In a little paper which Sir Lauder Brunton handed in, which goes with Ferris and Company's First Aid Snake-bite Outfit, it is stated "The best remedy that can be carried for this purpose is permanganate of potash, the efficacy of which as an antidote to the poison, not only of the cobra, but of other venomous snakes, when properly applied, has been conclusively demonstrated." Do you think that statement is justified?—What is the date of that?

8129. It was handed in the other day?—Yes. The only two large classes, namely the cobra class and the viperine class, have two absolutely distinct actions. There are a few which do have a combined action to a certain extent, but the permanganate has been tested now against such a large number and found uniformly active, that I think we may take it as practically certain that it will act against any kind of snake poison. It has not been tested, of course, against every variety in the world yet.

8130. If that be so, in view of the heavy mortality in India, a heavy responsibility would rest upon the public authorities if they did not supply this apparatus?—You have to take into account the conditions in India. If you take, say, Eastern Bengal, where so many snake bites occur and where almost every house is separate, with a large number of rice fields between, there is practically no such thing as a village. When you come to hundreds of thousands of square miles of country like that, you can see how difficult it is to have the remedy always on the spot. But the Government are doing now a great deal towards distributing it, and I expect that we shall get good results.

8131. You say that the fact has been known for 30 years that this permanganate of potash is an antidote?—If mixed with cobra venom outside the body, it was known 30 years ago that it would destroy the venom.

8132. And presuming that the application was made in time, many thousands of lives might have been saved if what you tell us is correct?—It was purely a matter of getting a practical method of using it, which we now have but which we had not at that time.

8133. (*Mr. Ram.*) First with regard to viperine poison, you say that, if treated, it is counteracted by the use of adrenalin?—It counteracts the fall of pressure to some extent.

8134. In that particular case of treating viperine poison, experiments on animals would be shown to be doubly useful, would they not?—Yes, I think so.

8135. In the first place, you ascertain, I understand, by experiments on animals, exactly the mischief caused by viperine poison, namely, intravascular clotting or paralysis of the vaso-motor centre?—Yes.

8136. In the second place, having discovered the exact injury inflicted by the poison, you treat it with adrenalin?—Yes.

8137. Adrenalin also was discovered, was it not, in consequence of experiments on animals?—Undoubtedly.

8138. With regard to the snake bites, had they been investigated clinically for a great many years?—Yes.

8139. As I understand, as long ago as 30 years, by means of clinical research, the use of permanganate of potash was discovered as being advantageous?—Not by clinical work.

8140. Was it ascertained even as long ago as 30 years that the use of permanganate of potash was advantageous by experiments on animals?—It was discovered by experiments in order to try and find some antidote. They were mixing different chemicals with snake poison in order to see if they could find some antidote, and it was found in that way.

8141. Do you know what it was at the passing of the Act which hindered Sir Lauder Brunton from carrying on further experiments?—I do not know the details of it.

8142. You do not know what bit of the Act, so to speak, pinched him?—No, I have only his statement that that was the fact.

8143. I understand that horses, for instance, have been made immune by the injection of snake poison?—Yes.

8144. That was ascertained by experiments on animals, I suppose?—Yes.

8145. Solely and entirely?—Entirely.

8146. With regard to the use of permanganate of potash, you told Sir William Collins that it does not have to operate against mischief produced by an organism?—No, it has an antiseptic effect as well, which is useful; because there is a great tendency for septic changes to follow on snake bites, especially viperine ones. But that is not its essential action.

8147. But the mischief which it is intended to obviate and does obviate is caused not by an organism, but chemically?—Yes.

8148. Objection has been taken, as you no doubt are aware, by a good many witnesses to what they term the injection of any matter from any animal into a human creature?—Yes.

8149. This treatment of permanganate of potash on snake bite would not be open to that objection?—No.

8150. With regard to immunising horses by serum, is this the only case in which an antitoxin can be shown to be valuable against an evil which is not caused by an organism?—There are other chemicals, such as ricin and abrin, of which it has been said, that by injecting these into animals you can produce a serum which will even prevent the action of these chemicals, although they are of vegetable origin; but not against such substances as morphia.

8151. (*Dr. Gaskell.*) Those substances are also albumoses?—I think they are very closely allied.

8152. They are vegetable albumoses?—Yes.

8153. (*Mr. Ram.*) Then, except with regard to vegetable albumoses, the evil which the antitoxin can prevent in all other cases is produced by organisms. Is that so, so far as you know?—I believe so, so far as I know. I do not know the details of all those things.

8154. (*Sir Mackenzie Chalmers.*) Have you made any experiments with duboyi poison?—That is Russell's viper.

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8155. What is that big snake that is so exceedingly poisonous?—The hamadryad.

8156. But there is another name for it?—The king cobra.

8157. Have you ever experimented on that?—Yes. I tested all these to see whether they had the same action as cobra venom, and whether Calmette's antivenin made from cobra venom would act against the other colubrine snakes, the hamadryad, the krait, and the banded krait. I found that they do have some action, but not enough to be of very practical use.

8158. The reason being that the physiological action of the two poisons is different?—No, the physiological action is the same. I tested them all first, and they all had the same physiological action in paralysing the respiratory centres—that is to say, all the colubrine class and the sea snakes. The cobra antivenin does have some effect against all those, but it is very much less than the effect against the cobra itself.

8159. And it has no effect when you get it against viperine poison?—It has no practical effect really against any but the cobra.

8160. But the permanganate of potash if applied instantaneously operates, so far as you know, on all snake poisons that you have tried?—Yes.

8161. Did not Dr. Douglas Cunningham try some experiments for the Government of India?—Yes.

8162. They were negative, were they not, in their results?—He tested a large number of reputed antidotes, but the results were all negative.

8163. And also he tried Calmette's and Fraser's antivenin?—Yes; he showed that it had an effect, but the effect was not very strong; it required to be much stronger.

8164. Was that on account of the small doses?—Yes; as a matter of fact, if you take a full quantity of cobra venom and you inject an average quantity, you will want 400 c.c. of Calmette's serum of the usual strength to neutralise it; that is why it is not very practicable; but, of course, a snake may often inject a small quantity, and then it may be efficient, but it cannot be relied upon by itself in every case.

8165. As regards the ligature, I suppose that the object of the ligature is to prevent the poison spreading into the system?—Yes.

8166. But if you could not apply the permanganate of potash for some time, do I rightly understand that in spite of any ligature the poison does get into the system?—I think that the poison would diffuse to a considerable extent in the limb, and make it much more difficult to neutralise it locally. When some time has elapsed I advocate making a much longer excision and applying the permanganate of potash more thoroughly.

8167. Can you catch up the poison then?—Not altogether.

8168. To some extent the permanganate gets into the circulation and catches it up?—The permanganate does not get into the circulation. I have tried that, and I find that you cannot do it, for this reason: If you use the potassium salt permanganate, it is too poisonous to inject in sufficiently large doses. I tried to use the sodium permanganate, but I found that upon sterilising it by boiling first a lot of it gets precipitated and it is very difficult to use—it is more unstable. I have not been successful with it.

8169. If you cannot apply the permanganate of potash at once and to some extent the symptoms have set in, I suppose you apply other remedies as well in addition to it?—Yes, certainly; you apply any remedy that you think may be of value in addition.

8170. How long after a bite is it of any use whatever to apply permanganate?—The longest case I have got was four hours.

8171. Does some part of the poison remain locally as long as that?—Yes, you can show it in this way: If you take a dose which is just fatal to an animal, if you inject it intravenously it will kill the animal in 20 minutes, but if you inject the same dose subcutaneously it takes 24 and often 48 hours to kill the animal; showing that it is absorbed comparatively slowly. I think the reason is the point which I have mentioned, that when you make an incision at the seat of injection you always find a bloodstained effusion of serum in 30 seconds. I think that is a protective agency to lessen the rapidity of absorption, as the absorption of a small dose is slow. So that if a very large dose is injected, a fatal dose may reach the circulation very rapidly before there has been much effusion to limit the absorption.

8172. Practically, then, the mortality of snake bite depends upon the amount of venom which happens to be injected?—Yes.

8173. And therefore all figures would be more or less fallacious, because it all depends upon the dose?—Undoubtedly.

8174. And to some extent, I suppose, it depends upon individual idiosyncrasy?—I doubt it. From experiments on animals I find no tendency of that kind. I should say that there is very little individual idiosyncrasy.

8175. It is almost impossible, I suppose, to get an intravenous injection by means of ordinary snake bite?—It might occur; in that case it would be fatal very rapidly.

8176. On the chance of its being intravenously injected, how soon would it be fatal—in a couple of hours?—If intravenously it would cause death quicker than that; it would not be likely to take more than 10 to 15 minutes, but even less.

8176A. In the ordinary case, when a fatal dose is not received, I suppose it is where clothing intervenes?—Yes, that is an important point. In this series I think that every one of the cases was inoculated not through clothes, but on the hand or foot.

8177. On the bare hand?—Yes, that is an important point in the series where the snake was identified.

8178. Because if a man was bitten through the trousers——?—He might probably escape altogether.

8179. Have you tried the effect of permanganate of potash on scorpion poisoning?—I have not done so myself, but it has been tried in Egypt, I am told, and it has been found to be equally effective. It is used now in Egypt, I am told, for scorpion poisoning.

8180. Rubbed in in the same way?—Yes, either rubbed in or injected.

8181. A great many natives in India are bitten by scorpions, are they not?—We do not hear much of it.

8182. Because the cases are not usually fatal?—Yes.

8183. Although they are very painful?—Yes, they are common enough. I have used the permanganate of potash in another way. I have found it very useful in preventing tetanus infection in wounds.

8184. Will you tell us anything that you know about that?—In Calcutta tetanus is an exceedingly common disease.

8185. Because the whole of the road dust is full of it?—Yes; so I carried out some experiments by putting a little dust from the Calcutta road under the skin of a rat, and I found that five out of six died of tetanus; but if I added a few crystals of permanganate of potash to the dust, then I only lost one animal, and that was not from tetanus. I then carried out a further series of experiments by scarifying the skin, rubbing in the dust, and using strong permanganate of potash solutions at different intervals, and I found by that measure also, even up to an hour afterwards, it was nearly always possible to prevent tetanus infection, although the control animals developed the disease. But permanganate of potash has also been used at my suggestion in cases of leopard bites. When persons are shooting in the wilds and there is no doctor near, permanganate crystals instilled into leopard bites, which are usually very fatal, in severe cases have prevented serious results from blood poisoning.

8186. The serious results arising simply from the dirtiness of the leopard's teeth?—Yes. I also suggested that it was worth trying in hydrophobia, and one of the medical officers in the South of India has used it in a number of cases of dogs which have been bitten by mad dogs, and a number of them have been saved apparently, and he is very enthusiastic about it.

8187. Using it according to your method of making an incision and rubbing in the actual crystal?—Yes, so that evidently of late it appears to be of very wide application.

8188. Is there a considerable death rate from tetanus at Calcutta at the present time?—Yes.

8189. And when a man scratches his hand on a road in Calcutta is this treatment applied?—They are

using the tetanus serum in Calcutta as a prophylactic. It is of very little use after symptoms have developed, but it is now used at the Medical College Hospital in all wounds that are thought to be likely to be infected with tetanus; and it has very materially reduced the number of cases of tetanus at the Medical College Hospital in the last few years.

8190. What is the incubation period of tetanus?—It varies very much, from seven to fourteen days. In the early days incubation it is much more fatal.

8191. In fact, the larger the dose the quicker the disease comes on?—Yes.

8192. When you have used the permanganate of potash have you also used the antitetanus serum?—In these cases in the Calcutta Hospital they only use the serum.

8193. They have not used the permanganate?—No.

8194. In your opinion, would it be desirable, say, first to use the permanganate?—Personally, I should do so.

8195. You would use the double treatment?—Yes.

8196. You would first try the permanganate?—Yes, and then get the serum as well, as a prophylactic. I have not had cases of my own to treat.

8197. I wanted just to get clear whether there had been any cases in which you used the permanganate of potash alone and where the treatment has been successful, other than experiments on animals?—Only in experiments on animals, so far as I know. I have not had any actual cases yet. I have not had any opportunity of using it myself, and I do not know how far it has been used.

8198. It would not be safe to use only the permanganate on man without trying the antitetanic serum as well?—I should use both where they were available; but the permanganate is much more likely to be available.

8199. Is there any danger from the antitetanic serum itself as apart from the disease?—I have never seen any.

8200. What symptoms does it produce?—A slight rash or edema, just as in diphtheria, but I have never seen any serious symptoms. It has even been used for injecting into the spinal cord recently in cases of tetanus, without any harm at any rate, and one case has recovered; but tetanus is a disease from which a great many cases recover, and you want a large series of cases to test it.

8201. Tetanus is not an absolutely fatal disease; there may be recovery with ordinary treatment?—Yes, especially under the chloral treatment.

8202. (*Dr. Gaskell.*) Do you know anything about the treatment for snake bites by swallowing the poison glands?—That was very carefully tested by Professor Cunningham. There was an idea in India that snake charmers produced immunity by swallowing snake venom; but Professor Cunningham fed a goat for, I think, two or three years on large quantities of venom, he gave some very large quantities, and in addition he gave all the remaining portions of any that he had used in his experiments, and he subsequently tested that goat with just enough to kill an ordinary goat, and it died in the ordinary time; so that I think we may take it as quite certain that it does not produce any immunity in that way. And that is what one would expect, because one would expect an albumose of that nature to be destroyed in the digestion and not absorbed as such.

8203. With respect to what you answered just now about antialbumoses, you know that if the serum of an animal is injected you can get an antiserum?—Yes.

8204. So that it is not only a question of albumoses?—No

8205. It applies to the various proteid bodies?—Yes, I was thinking at the time of the practical application.

8206. Do you think that snake charmers immunise themselves by small inoculations?—I think they do. I think that they often get little scratches, and get a little venom injected in that way. We had a man for many years at Calcutta who certainly had been scratched in that way repeatedly, and he was perfectly convinced himself that he had got immunity; and I know that Professor Cunningham thought so, too.

8207. (*Dr. Wilson.*) You said that in experimenting with this antivenin you found that it acted much more efficiently and rapidly if you injected it intravenously and not subcutaneously?—Yes, much more so.

8208. That, of course, would apply to the use of all sera?—I think so.

8209. In human beings as well as in animals?—Yes.

8210. So that to get the most efficient use of a serum, you would say from these experiments of yours, that instead of injecting it subcutaneously it ought always to be used intravenously?—Certainly in the case of snake poison where you want to get an immediate rapid effect, and, I think, also in many other cases possibly.

8211. But, of course, you admit that owing to the many difficulties which you have described, these sera, having to be manufactured for different snake poisons, until you get a polyvalent serum they are of little practical use?—That is my personal opinion.

8212. And I think you admitted also that in almost all cases which were brought to you, in the first series at all events, a ligature had been applied?—They were reported to me. I have not got full details of all, but I find there were four cases where a ligature was applied at once.

8213. And in the second series was a ligature also applied there; it would be, I suppose?—In regard to them I have not been able to get full details. I have corresponded with the people.

8214. I ask the question, because in China, according to a statement made by Mr. Cantlee, the well-known surgeon who has practised, as you know, out in China for a great many years, the Chinese immediately adopt the ligature; and he also says that it is a fact that very few Chinese die of snake bite. That is his positive statement?—I should like to know what the snakes are, and also how much venom they ordinarily emit, how many times what would be a fatal dose to man. You want to know all those factors. That is why I lay so much stress on our cobra cases, because I know that a cobra does eject on the average ten times a fatal dose.

8215. But, on the other hand, does it not appear that some considerable value might be attached to the use of the ligature?—I hoped so; but I may say that I did some experiments, but was not able to find it. It is a thing that wants further working out.

8216. Is it made part of the instructions which are issued with this lancet, that the ligature is to be applied immediately?—Yes. In my experiments with permanganate treatment ligatures were applied in nearly all the control animals which were injected with snake venom, but not treated with permanganate, but they all died in about the usual times, so that the ligatures alone completely failed to save any of them, even when only one fatal dose was injected.

8217. Now, with regard to permanganate of potash, are there other agencies, do you think, which would act equally well—for example, caustic potash?—Chloride of lime and gold chloride were found by Professor Cunningham to destroy snake venoms, but they are not nearly so practicable apparently as the permanganate; the gold chloride is exceedingly expensive while the chloride of lime is unstable.

8218. Have carbolic crystals been used?—I can not tell you absolutely. I know that Sir Joseph Fayrer and Sir Lauder Brunton tested a large number of things. I think that it was tested, but I cannot be quite certain.

8219. In early days, of course, permanganate of potash was a favourite antiseptic agent?—Yes.

8220. And in using it for tetanus, of course, in all the cases that go to you the wounds will be washed and cleaned, and so on?—Yes, but it is a curious thing with tetanus that ordinary antiseptics such as carbolic are certainly not as efficient as permanganate of potash. I remember a case of my own where the patient had been dragged along the road in London, and dirt had been grounded into the bone. I scraped it out most thoroughly, thinking of tetanus, and I used strong carbolic; but the patient developed tetanus, and Mr. Collier, the surgeon, informed me the other day that he had had similar results; so that permanganate of potash does appear to be specially efficient as an antiseptic against tetanus organisms, possibly. I think this is owing to its strong oxydising action against the tetanus bacillus, which is an

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anærobic organism which cannot live in the presence of oxygen.

8221. But the serum now is only used as a prophylactic; it has failed as a cure?—It has not been successful as a cure so far as I know in my experience.

8222. (*Sir William Church.*) Now you are going to be good enough to give us some information on the subject of rinderpest. It appears that rinderpest had attracted the attention of the Indian Government for many years?—Yes, as far back as 1881, I think.

8223. And they have a bacteriological laboratory under their own supervision?—Yes, entirely for studying diseases of animals.

8224. Especially for studying rinderpest and other diseases of animals?—Yes.

8225. And in 1899 you were in charge of the laboratory?—Yes; it was in November, 1898, that I started, and I was in charge all through 1899.

8226. Would you tell the Commission what the work of your laboratory then was?—At that time I was appointed to act for Dr. Lindgard, who had gone on sick leave. Professor Koch had been working in South Africa, and he had also visited India shortly before. The whole question of inoculation against rinderpest was being worked at in South Africa, and the Government of India wished the methods there used to be tested, to see how far they were applicable to the conditions in India. There were two chief methods. One was the bile method of Professor Koch, which had done great service in the early part of the outbreak in South Africa, but which was at that time being discarded in favour of the serum method. The serum method was that of Turner and Kolle. It consisted in preparing an antidotal serum by injecting large quantities of virulent blood obtained from an animal suffering from rinderpest, because the organism had not been discovered, but the blood is known to contain the infection; and by repeated injections in this way into an animal which had recovered from rinderpest and so become immune, it was possible to produce in the blood of cattle a serum which had a very strong antidotal action against rinderpest. This serum could be used in two different ways; it could be used as a prophylactic injection by injecting the serum alone into an animal which was exposed to infection as in an outbreak. It was found that injection of serum alone would protect the animal for some weeks or months, and in Indian conditions would tide the animal usually over the particular outbreak in face of which it was injected. But that method of injecting serum alone only produces a temporary immunity of comparatively short duration. The second method of using the serum, the serum simultaneous method of Kolle and Turner, was to get permanent immunity for life by producing a slight attack of rinderpest in the animal injected; and this is done by injecting a small quantity of virulent blood containing the infection in one side of the animal, and at the same time injecting in a separate place a measured quantity of the anti-rinderpest serum, the strength of which has been previously ascertained. By this method it is possible to produce a mild attack of the disease in the animal, which will protect it for the rest of its life. This method was very successful in South Africa, but I found that it did not work very successfully in India. The reason for the difference was this. In South Africa the disease was a new one, so that all animals were equally susceptible, and consequently it was possible to regulate the dose of serum so accurately as to produce a mild attack of the disease in 90 per cent. of the animals, and yet not to have more than a half per cent. of fatal cases. In India, on the other hand, I found that owing to rinderpest having been present for very many years, the susceptibility of animals varied very greatly, so that it was impossible to so regulate the dose of serum as to get the results which were obtained in South Africa. If a large dose of serum was used so as to be sure of not getting many fatalities, then only a small number of animals, from 30 to 50 per cent., actually developed an attack of the disease, and so only that number obtained permanent immunity. If, on the other hand, the dose of serum was reduced so as to get a larger number of attacks, then the mortality was much higher than they got in South Africa. I found, however, that I could get over this difficulty, after long experiments, by using a large dose of serum to start with at the same time that the blood was injected, so as not to lose any animals, and then to any animals which did not react, which got no temperature or fever, or any signs of the disease at the end of one week, I found that I could then safely give a second dose of virulent blood, and then I nearly always got a reaction with permanent immunity; the reason being that those animals which did not react at the first inoculation were less susceptible than the others, and the serum was too strong to allow the virus to develop, but at the end of a week some of the strength of the serum had passed off, and then you could get the reaction safely. And this method I called the re-inoculation method. This method has been tested in the Philippine Islands, and is now being recommended in the Philippine Islands as preferable to all other methods of obtaining permanent immunity; but in India the method of simply injecting the serum alone is most generally used, because the more complicated re-inoculation method requires more skilful observers than are generally available. I may say that at the end of my year's work I made arrangements for turning out 100,000 doses of serum a year. I also inoculated a large number of animals myself in actual outbreaks. In two districts I inoculated over 500 animals that were actually in the same sheds with other infected animals, and at the same time I had notes of all the other animals similarly exposed to infection which were not inoculated because the villagers did not agree to it. These were all subsequently reinvestigated at the end of two months, and it was found that no one of the 500 animals which had been inoculated had developed rinderpest later than four days after the inoculation—that is to say, within the incubation period of the disease; so that those who developed the disease within four days must have been infected before the inoculation. We may say, therefore, that no animal which was not already infected subsequently developed the disease after inoculation in that outbreak. On the other hand, of about an equal number of animals similarly exposed to infection and not inoculated, one half developed the disease and half of those died, making a mortality of 25 per cent. of the uninoculated animals. These results were so conslusive that the Government of India arranged to turn out 100,000 doses of serum a year. Many thousands of inoculations have been done year by year since, and, I think it was last year, a second laboratory was sanctioned in the South of India because the one laboratory is not sufficient to make enough serum—the results have been so satisfactory.

8227. In what way do you deduce the variable susceptibility to the disease of cattle in India. What is the cause of the presence of rinderpest for so many years in the country?—Rinderpest is always present in India, and a very large number of districts are affected every year. You will find that one district will get rid of a lot of disease in one year, and after that there will be comparatively little for a few years and then you get another outbreak again; and when you come to an actual outbreak there will be a certain number of animals which perhaps have had the disease in a previous outbreak several years before, and those will be permanently immune

8228. Then an epidemic of rinderpest in India shows itself in the same locality during the life of the same animals?—It will not affect animals which have had it before, but there certainly is a very variable susceptibility.

8229. The interval between epidemics in localities in India is a short one?—Yes; it varies very much in different parts.

8230. But frequently it is short?—Yes. I may say that the natives are so fully aware of the protective action of an attack of rinderpest, that in the hill parts where the rinderpest is comparaively rare, and when it occurs it carries off 90 per cent. of the animals, an animal which has had rinderpest is worth double the value of one that has not. That is a very definite fact, and you can produce equal immunity by inoculation by the simultaneous or re-inoculation methods.

8231. That is your explanation of the varying susceptibility of Indian cattle?—Yes, that is so far as I can understand it. It is very difficult to explain, but it is a fact at any rate.

8232. Has the value of re-inoculation of cattle in India been recognised by the natives?—The re-inoculation is used very little in India, because, as I say, it is more difficult to carry out.

8233. But the value of your serum treatment has been recognised by the natives?—Undoubtedly. To give my own experience, after doing inoculations in three districts, the results of which I have just given you, I had applications from the natives to go back and do more in each place, although at first many of them had refused to have it done.

8234. From what class of natives would that be?—All the poorer village class; the ordinary agricultural labourer.

8235. (*Sir Mackenzie Chalmers.*) In what districts?—In Alleghur and Bulundhahar.

8236. In the north-west?—Yes.

8237. (*Sir William Church.*) Would they be Hindoos?—Mostly Hindoos, but a good many Mahomedans as well. I found when I got to a village that if the disease was still very prevalent and they were still losing animals, I had no difficulty in getting a good many done. I simply explained that it was just like vaccination, that as by vaccinating their children they were prevented from getting small-pox, so by vaccinating these animals we could prevent the animals getting rinderpest, and they understood it at once.

8238. So that these uninstructed and perfectly impartial natives were rapidly convinced of the value of it?—Yes; it touched their pockets, too, I may say. I found that some men who were selling cattle, when we wanted to have them inoculated said that they would not have it done because they wanted to sell their cattle, and they did not mind if the cattle got it after they were purchased; but when they belonged to the villagers themselves they were very keen about it.

8239. Do the animals suffer very much from which you get the blood—those in which you artificially produce rinderpest?—They certainly must suffer. The disease is more nearly like typhoid than any other human disease; you get ulceration of the intestines, and you get a high temperature, 104° or so, and the animals soon became drowsy rather like a typhoid patient; and when they get to that stage I do not think they suffer much, but they do get violent diarrhœa and running from the nose. They must suffer to some extent.

8240. Do you keep these animals until they die, or are they killed?—As soon as they have fully developed the disease they are bled to death. In that way you get your blood for injecting, and the animal is put out of pain.

8241. Have you any figures that you can bring forward to show the loss of life among animals during the last few years?—No, I have not. The disease varies very much. You get some epidemic years when they lose 100,000 animals in one district. In other years the disease is much more scattered.

8242. But if there is any value in this treatment, the epidemic ought to get less and less severe so far as mortality goes?—It is only the last six years that it has been introduced, and I have not been able to get at the Veterinary Reports. The figures have been published in Government Reports—probably I can get them at the India Office—showing the results of inoculations in the last few years.

8243. You think that figures will be available from the India Office?—I think I can get you figures as to the actual effect of inoculations in outbreaks.

8244. (*Sir Mackenzie Chalmers.*) It is only a very minute portion of the whole cattle in India that are treated?—There are several million cattle in a single district, so that you could not expect at present a marked effect on the whole of the cattle in India; but there are figures showing a very material reduction of mortality in particular localities; I think I can get you some.

8245. (*Sir William Collins.*) Is cattle plague distributed equally all over India?—Yes, I think pretty well all over India.

8246. Are there very large tracts of India which are free from it?—Not that I know. The hill tracts only suffer occasionally. The hill tracts may be free for several years at a time, but then you get very bad outbreaks afterwards. But in the plains it is practically always present, so far as I know, scattered about.

8247. What is the recognised administrative method of dealing with cattle plague in India?—It is practically impossible to do anything.

8248. Is slaughter of infected cattle carried out?—No, the natives will not allow it.

8249. Is there no attempt at the so-called stamping-out system?—It is quite impossible. You have to remember that the veterinary staff in India is exceedingly small. There is one veterinary officer for a whole province. They are being increased, but require to be increased more.

8250. (*Sir Mackenzie Chalmers.*) A province having from 30 to 70 millions now?—Yes, you have 50 millions in Bengal.

8251. There used to be 70 millions; now, since the partition, there are 50 millions?—Yes.

8252. (*Sir William Collins.*) Is the combined method of which you have spoken used now with Government sanction?—It is used where you can get the favourable conditions. It has been used in Assam among planters and people where it can be thoroughly carried out and where they understand it, but it is not used much by villagers; they prefer the simple serum method, because it tides them over their difficulties, and also because it does not make the animals ill. In several villages where the disease was not very prevalent I found that they would not allow the inoculation to be done. I had to tell them that by the serum simultaneous method the animals might be ill for two or three weeks, and they would not allow it. By the serum alone you get over the difficulty.

8253. Has the Government discouraged the use of infected blood because of the danger of communication of the disease to the animals?—No, I think not. Wherever it can be done it is done, but it is quite exceptional to find a place where the conditions are favourable in India.

8254. Do you think that there is a danger of propagating the infection by the use of this combined method, inasmuch as you are using the actual infected blood?—You only use it in an actual outbreak, where there is an enormous amount of infected cattle.

8255. But where there is an enormous amount also of uninfected cattle, I suppose?—But by this method the symptoms are very slight—nothing but a little fever; you do not get running from the nose and discharges which carry the infection, so that I very much doubt whether there is any material infection from animals with these very slight attacks as the result of inoculation.

8256. There have been previous attempts at vaccination against cattle plague, have there not?—Before South Africa?

8257-8. Yes. Have you heard of McPherson's experiments in Bengal a good many years ago?—Yes; he did some.

8259. He tried communicating the infection of cattle plague to the human subject, did he not?—I do not know.

8260. Have you heard of Furnell's experiments in Assam?—What date was that?

8261. I think it must have been in the thirties?—I do not know about that.

8262. Vaccination was tried against cattle plague, was it not—the use of cow-pox in this country in 1866?—I know nothing about the outbreak in England at that time.

8263. Are you able to institute a comparison between the so-called stamping-out method, if that were practicable, and the method of preventive inoculation?—Preventive inoculation I should say would be a thousandfold preferable to stamping out, as saving such an enormous number of animals which you otherwise would have to sacrifice.

8264. You spoke of a particular outbreak in which the inoculated animals did not suffer, or at any rate did not suffer anything like so severely as the uninoculated. Where was that?—In the United Provinces, in Alleghur and Bulundhahar.

8265. Has that been the subject of official report?—Yes, that is all in my Report to the Government.

8266. What is the date of the Report?—It was published in April, 1900.

8267. In what volume would that be?—It is a Special Report on Inoculation against Rinderpest—Report of the Laboratory.

8268. To the Indian Government?—Yes.

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8269. Is it published in this country?—It is available in this country; it gives you the details and all my experimental results.

8270. (*Sir Mackenzie Chalmers.*) Of course as regards the stamping-out method by killing the cattle there is a religious objection in India?—Absolutely.

8271. It would be impossible in any Hindoo community?—Absolutely impossible; they would not consider it for a moment.

8272. So that, apart from the relative values, it would be an impossible thing for the Government to undertake?—Yes, absolutely impossible.

8273. There is one question that I omitted to ask you about snake poisons; I suppose it would be a very long time before the ordinary cultivator could ever be induced to carry about the permanganate of potash and the lancet?—Yes, the ordinary cultivator would not do it. The idea is to distribute it to the head men in the villages.

8274. In the first place, the ordinary cultivator has no clothes—no pocket, at any rate?—That is so.

8275. And no clothes in which he can carry it?—No.

8276. And it takes a long time to get an idea into his head about the treatment?—Yes, they tie up their pice in the corner of their dhoti; they might do it in time.

8277. (*Mr. Tomkinson.*) You are aware, of course, that there was a tremendous outbreak of rinderpest in this country some 40 years ago?—Yes.

8278. And no cure or preventive was known at that time?—No.

8279. They had to fall back upon the pole-axe altogether?—Yes.

8280. Is this discovery of yours protective only, not curative?—It is not curative. It has been used in that way, but it is not very efficient. I cannot tell you whether in the last few years they may have improved it.

8281. Then, so far, no cure has been discovered for an animal that is actually and virulently attacked?—I think not.

8282. Do I rightly understand that the instance about which you were asked by Sir Willian Church, as to the suffering of animals, was in the case of an animal being experimented on and inoculated with the virus of a dead animal?—No; I said that in preparing the serum, as there is no organism of the disease known, you have to take the blood of an animal that is suffering from the disease and inoculate it into the animal in which you produce the serum so that for that purpose you have to inoculate a certain number of animals with the disease.

8283. With the blood of the animal that has been attacked, naturally?—Yes; you have an animal which you inoculate with the disease in order to produce the disease, then you bleed that animal and use its blood to inoculate into the animal in which you produce the serum.

8284. From that second animal the serum is produced?—Yes, that animal is not killed.

8285. How much does that animal suffer?—That animal does not appear to suffer at all. You have to use an animal that has had an attack, or one which you have inoculated yourself; after that they get practically no reaction.

8286. Then they develop the serum which is required for a prophylactic?—Yes.

8287. Then in what way is that serum drawn from that animal's body?—It is drawn off from a vein in the neck, taking sufficient blood not to injure the animal, and then after a certain time you can take some more blood.

8288. Is it blood or a secretion?—It is the blood itself from which you separate the serum by a centrifugal machine, and then you add a little carbolic or lysol antiseptic and bottle it.

8289. Have you any idea how the disease originates otherwise than by infection or contagion?—It originates by contagion as a matter of fact. Unfortunately in India it is sometimes spread by the Chamars, the leather workers, who actually spread rinderpest in order to get the hides of dead cattle. It is a very difficult thing to stop.

8290. But any origin except that of infection or contagion has not been discovered?—No.

8291. (*Dr. Wilson.*) I suppose it is the horse that is used for rinderpest serum?—No; cattle are used. Horses are not susceptible to rinderpest.

8292. Would the serum, then, of an animal, which had an attack of the disease naturally, be looked upon as a prophylactic serum?—It would have some antidotal action, but not nearly strong enough to be of value; you would have to heighten it by further injections.

8293. Then the serum is used alone. You do not get the dual system?—No, we use chiefly, almost entirely, the serum alone in India.

8294. If a mild attack of the disease is induced by injecting the blood of a diseased animal it is merely a repetition of the old process of inoculation against small-pox; I mean inoculation from mild cases, as it were?—You cannot produce a mild case of rinderpest by inoculating from a mild case.

8295. It was believed in old days that you could produce small-pox in that way?—I do not think that is so with rinderpest. If you take a mild case it will produce a very violent case in an animal that is fully susceptible.

8296. By infusing the blood, you say, that a mild attack of the disease is induced, and, of course, that would produce immunity?—Permanent immunity is produced by injection of both virulent blood and the protective serum in a proper dose.

8297. But by the serum treatment it is temporary immunity?—It is temporary immunity which tides over that particular outbreak, but in a year's time the immunity will probably have vanished.

8298. So that you will have to go on?—If you get another outbreak you will have to inoculate other animals.

8299. (*Sir William Church.*) Could you tell the Commission anything about the working of the Kasauli Institute; do you know anything about it?—I know something about it. I have not worked there myself. Originally private subscriptions were raised in order to start a Pasteur Institute so that the treatment of hydrophobia could be carried out in India and the very serious delay of people going off to Paris could be avoided. When I went out to India first in 1893, subscriptions were being raised, and a year or so later one of the Anti-Vivisection Societies, I do not know which, sent out appeals to all the native Rajahs and people who had subscribed to this Pasteur Institute in India or had promised to subscribe to induce them to withdraw their subscriptions. However, Surgeon-General Harvey, at the Indian Medical Congress in 1894, gave an address on this question of anti-vivisection which neutralised that effort. Later on it was found that sufficient subscriptions could not be raised in that way, or, at least, before sufficient subscriptions were raised to do it in a private manner, the Government took it up, and I believe they gave a contribution equivalent to the amount which they had been spending on sending Europeans to Paris to be treated, and in that way the Kasauli Institute was started. The results are considered to have been so valuable that within the last year a second Pasteur Institute has been started in the south of India on account of the enormous area which has to be covered.

8300. (*Sir Mackenzie Chalmers.*) Do you know the results of the treatment at the Institute?—The figures are practically identical with those of Paris itself; the results have been very favourable.

8301. (*Sir William Church.*) Hydrophobia is not an uncommon disease in India, is it?—It is quite common.

8302. Has the population recognised the benefit of the Institution, or is there any difficulty in getting it recognised?—A large number of natives attend the Institute at the present time.

8303. Without pressure being placed upon them?—I think so.

8304. They recognise its value?—I believe so. I have not worked there myself.

8305. (*Sir Mackenzie Chalmers.*) Where can we get the exact figures?—I will look them up for you. I think I can find them for you.

8306. (*Sir William Collins.*) Is the Laboratory supported partly by Government aid and partly by volun-

tary contributions at the present time?—Yes, the original subscriptions were all funded. The Government pay most of the expenses, I think, now.

8307. (*Sir William Church.*) Is there anyone in this country at the present time who can give us information about it?—I do not know that there is anyone in the country who has worked there, but I shall be very pleased to look up the figures.

8308. I suppose the India Office would have them? Yes.

8309. (*Dr. Wilson.*) Is the Pasteur system of treatment carried out there?—Yes.

8310. But is not this new treatment, the permanganate of potash treatment, likely to supplant the Pasteur system?—We should not like to rely on it entirely by itself. I think it is a most useful thing to start with to save any loss of time.

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8311. Is there a tendency to combine both systems?—In the south of India the permanganate system has been used more particularly in dogs in hydrophobia cases. How far it has been used in the human subject I do not know. It is only last year that these results were published. I do not think the system is very generally known yet.

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Rev. John Page Hopps, called in; and Examined.

8312. (*Sir William Church.*) You appear as a witness on behalf of the Social Purity Alliance?—I think so; that is what I understand.

8313. You are a minister of religion in London?—Yes; Little Portland Street Chapel.

8314. Of what denomination?—Unitarian.

8315. You wish to give your opinion as to the ethical bearing of vivisection upon public safety and public morals?—Entirely that. I should like to say first that I am very glad to hear the question asked me just now whether I am prepared to speak on the ethical side of the matter connected with vivisection, because when I was asked to give evidence, I thought it would scarcely be admissible, and I thought I should almost have to apologise for consenting to be presented as a witness, seeing that I am not in any way an authority on the subject of vivisection, but I have been informed by those who asked me to attend, that this Commission is open to receive personal impressions and opinions as to the ethical bearings of vivisection upon public safety and public morals, and, if I may venture to say so, upon those who practice vivisection. Moreover, on the broad ground that the law should be mindful of morals, an advocate for applying the claims of ethics to vivisection, may perhaps justly have a *locus standi* here. As to matters of fact, I have therefore very little to say; mine are only inferences based upon facts, and I must not be personal. One possible justification for the presence of such a witness as I am may be found in an inference which very seriously bears upon what I have called 'public safety." The practice of vivisection, I suppose it is granted on all hands, undoubtedly produces a fascination for it which, in some cases, has confessedly risen to joy, and even to the perception of supposed religious sanctions.

8316. I would like to ask you at that point what evidence you can bring forward to support that statement?—I am proceeding to that. This may explain the dangerous suggestion made by one practitioner, that the hospitals ought to be supported, because "their inmates are made use of otherwise than for treatment"—a highly significant and sinister suggestion, which has very properly called forth from the other side the suggestion that the vivisector "is never safe to be trusted with a hospital patient on an operating table, and a tray of knives at his side." I do not say that is my opinion; I only say that I noticed both those statements.

8317. (*Sir John MacFadyean.*) Might we have references to the statements?—I should be very glad to supply them.

8318. Might we have them now. Who made the statements?—I have seen them stated. I will be very glad to supply the references later on. I am simply giving my own opinion in what I say. With regard to statements made by others I simply pledge my honour that I have seen them.

8319. (*Sir William Church.*) We do not for a moment doubt that you have seen them, but it is really quite necessary for us to know where you have seen them in order that we might see the nature of the context in which the statements have been made?—I think with regard to all other statements I do provide the authorities. An experimenter quite naturally desires subjects, and the temptation to seek them is great. There is a public peril here, because, unfortunately, in such a matter as this we must take the practitioners at their worst—in this case also the chain being no stronger anywhere than at its weakest part. Of course, it is well known that in Continental hospitals experimental vivisections of patients is common. The revelations of the "Deutsches Volksblatt" concerning the conducting of dangerous experiments purely for practice, and the inoculation with contagious diseases of young children and pregnant women, are not likely to be soon forgotten. One distinguished surgeon in Vienna frankly told the correspondent of the "Morning Leader" that in most free hospitals "patients are made use of in the cause of science," adding, "Tests must be made on somebody, and patients in charity hospitals are generally the only available subjects."

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8320. I am afraid I must trouble you for the references. You have only given us the "Morning Leader." We wish to know who the Austrian surgeons are who have stated those views?—I am quoting from the "Star" of January 27th, 1899.

8321. We cannot accept the "Star" as an authority for what they state that people said in Austria?—Then I give it for what it is worth.

8322. (*Sir John MacFadyean.*) I submit another point. This evidence relates to Vienna—which is not germane to our inquiry?—I give it for what it is worth. I quote the "Star" of 27th January, 1899.

8323. (*Colonel Lockwood.*) It only refers to foreign hospitals?—We are all human beings, and what is human nature is human nature in Austria or in this country.

8324. (*Sir William Church.*) But you surely must realise that papers like the "Star" and many other papers are not very particular as to the truth of statements which they make. They will extract from a person's speech or writings a sentence which may bear a very different meaning from that which it bore in the original statement of the speaker or author?—I am very well aware of that.

8325. Therefore we must be referred, if you please, to the original published book or speech?—It was a statement made, according to the "Morning Leader" and the "Star" to an interviewer.

8326. It applies to foreign hospitals out of this country, where, of course, our law does not apply?—I only give it, as I think I said, as showing the natural effect upon a man who takes a very great interest in the subject—that is human nature; a man would like—I think I should myself, if I were a vivisector and thoroughly believed in it, and it was my profession—be very glad to get subjects in any direction, and if I was shut up in one direction I should try another and should pursue it to the very bitter end.

8327. (*Sir John MacFadyean.*) Might I ask whether there is any evidence that the medical man in Vienna who made these statements had ever practiced vivisection?—I know no more than I quote.

8328. Then it is not stated that he ever practiced vivisection. What bearing his inclinations might have upon the practice of vivisection does not appear?

8329. (*Sir William Church.*) We will not interrupt you, Sir, any more, but we will ask you perhaps after your statement has been made some further questions on it?—As to the ethical effects of vivisection upon the community, it is surely reasonable to suppose that the example set by such highly-placed men as adorn the medical profession will be followed by others, not so highly placed (I am speaking now of English practitioners) and perhaps not inspired by such beneficent intentions. The old jest, descriptive of the typical Englishman: "It is a fine day, let us go out and kill something," has its extremely serious side, and is connected with all the foul associations of what is called "sport." Of course it will be said that vivisection is justified because it has its uses in relation to the treatment of human beings. As to that I say nothing, except to remark that the medical profession is by no means unanimous as to it, and to add this doubt of my own—whether observations made during the special and artificially produced agony or distress of a dog

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on a dissecting table may not mislead when applied to a man in his bed. The expert ought to know best, but the common sense of it seems to be that every function and act of the organism would be affected, and, in some cases, violently affected, perhaps reversed, under torture. We know that secretions of every kind are so affected by fear, resentment, anger, shock or violent pain. What then would be the value of the observed action of drugs with a subject so affected? One can quite readily see that opinions arrived at in such circumstances might be dangerously wrong. Can any thoroughly reliable inferences be drawn concerning an acutely conscious man from the behaviour of a conscious, an unconscious, or a horribly tortured dog? Surely the torture is a *plus* which may mean a great deal. We know that anger can poison a mother's milk, and that a furious temper or filthy thoughts can produce vile tastes in the mouth, and play havoc with the heart, or, indeed, with every bodily function. Is it not highly probable that similar emotions *plus* agony may induce conditions and consequences which will go far to invalidate any conclusions that may be arrived at respecting the action of drugs? May it not be even dangerous to infer that what happens to an outraged—even if unconscious—dog will happen to a tenderly cared for child or man? But to return to my own province, the ethical consequences of vivisection, and may I venture to say, upon the practitioner himself. Here I really must apologise. I must take the preacher's privilege, for here nearly all is inference or moral probability. What, for instance, must be the ethical effect upon a man who revels in the disgusting work—who calls the ideal vivisector an "artist," and says, "The vivisector must approach a difficult vivisection with joyful excitement. He who shrinks from cutting into a living animal, or approaches an experiment as a disagreeable necessity, will never be an artist in vivisection!" The dreadful character of the vivisector's work will doubtless be described by others, and it may at all events be said of it that if a necessity, it is a painful necessity, and that the natural inference of it is the blunting of the fine moral sense. It may be said that this is the vivisector's business and that it is his own affair if he likes to take his chance. That is not so. The operator may be a physician, or a maker of physicians. He helps to create the atmosphere in which a powerful and beneficent, or possibly a formidable and maleficent, profession works. There is no need to be personal over this matter. There would be no end to that. It will suffice if we consider probabilities. What is likely to be the effect of practising vivisection upon men who so greatly need all the refinements of sympathy, if not its emotions? We seem to be living in a crisis of nervous maladies requiring not so much drugs as direction. The remedy for most of our maladies is not to be found in the carcase of a cat but in the soul of a man; and it is the more or less understood consciousness of this that has led to the great revival of "Confession" and the guidance of the priest in England. The question that trembles on the lips of thousands of sensitive spirits to-day is: "Can'st thou not minister to a mind diseased?" Whether the remedy sought will help or not in the end seems doubtful, but what is certain is that the Father Confessor, if he is to be of any use, must be skilled, not in the behaviour of muscles and glands and membranes, but in the subtle windings of passion, the treacheries of motive, and the dens or Elysian fields of love. The torture of a dog, the dabbling in blood, the abstraction of attention from agony, would spoil him "for the cure of souls"; and is not "the good physician's" vocation near enough to that of the true priest's to make some analogy, both of duty and fitness, useful here? Might it not even be said that the functions are often practically the same? If so, the paths of preparation for the one must be those of the other also. What tends to make the one gentle and keen, sensitive and pitiful, strong and wise, is needed by the other. But bloodshed blunts; and we are specially assured by zealous vivisectors that this is so, and ought to be so. How can it be otherwise? How could a man bear to carry through the tortures we have indicated, unless he acquired the necessary insensibility to, or forgetfulness of, the inflicted pain? And yet it is difficult to see how this could be acquired without deterioration. A man cannot win character and qualities all round, for all classes of work; and, though he may not be conscious of it, his creed is shaped by his actions, and not his actions by his creed. The great Dr. Abernethy's testimony has not been invalidated by time, "Vivisection has the direct effect of deteriorating the moral sense, and, once the moral sense is destroyed, it is impossible to foresee the consequences." I am willing to believe—I am anxious to believe—that in the great majority of cases vivisectors have honestly persuaded themselves that they are doing man service, and are suffering no harm in doing it. But it does not follow that the harm is not done because he who suffers it is unconscious of it. The unconsciousness may be a part of the harm, in the development of a certain narrowing callousness which may be almost like the closing of the Holy of Holies in the inner self. Can a man ever inflict pain, except for the actual sufferer's good, without deterioration of affection and pity? But affection and pity belong to the sphere in which a living man can alone become "a living soul." Knowledge and insight in that divine sphere are worth infinitely more than any knowledge and insight that can be gained in the vivisector's laboratory; and knowledge and insight gained there may be paid for at a fearful rate. The operator may be so absorbed in his study as to be, in a way, oblivious of the disgusting details, but that makes no difference, except as a grave suggestion as to what is happening to the operator. It has been very properly decided in this country that cruelty to animals is an indictable offence. Why? Surely because it is an offence against public decency and public morals to inflict torture. So much has this been felt that even in regard to criminals, we have steadily and of set purpose drifted away from torture as ethically bad. Cruelty, then, in any form, has upon it the brand of social and legal wrong-doing, and the burden of proof is heavy upon him who wishes to have it licensed by law. Suggestions as to the ethically bad effects of vivisection upon the operator are only inferences, but, of recent years, there have been sufficient facts to warrant them. Only last year, a professor—Professor Metchnikoff, at King's College, May, 1906—reported in the Journal of Preventive Medicine, August, 1906, lecturing at King's College, London, asserted that "in questions of health, morality should not attempt to lead hygiene, but should rather follow her," again a very sinister suggestion, which goes a long way towards justifying the statement that these questionable pursuits lead to questionable doctrines, if not questionable ethical practices, and, in the case of this professor, one is hardly surprised at finding him highly interested in syphilitic experiments with chimpanzees and baboons, with his eyes upon "the beginners in prostitution," and a commendation of the application to them—that is, the beginners in prostitution—of the chimpanzee and baboon experiments, as "an advantage to them as well as for the men having intercourse with them." I need not follow this up, in order to point the immoral moral of this odious suggestion. But, apart from this directly ethical view, there is the confessed selfishness of the whole thing, and with that I practically conclude; that enters into my subject of the ethical side of it. The justification of vivisection is based upon sheer selfishness. When all is said on the score of results, and the "artist in vivisection" has done his best to convince us that he is the apostle of mercy, many grave considerations "give us pause." For instance, what law of God or Nature justifies this treatment of our poor relations? Who gave to this amazing enthusiast the right to say offhand that he was at liberty to exploit the lower animals for experiments? What right has he to assume that these creatures are at his disposal for torture? If he proposed to kidnap or purchase cheap babies for laboratory purposes, his spectacles would not be safe in the streets; and if he put his proposal into effect, and got his babies on the dissecting table, the very costermongers would raid his laboratory, and even medical students might help them. Why? The cutting up of one baby might save the lives of thousands of other babies, and the torture of one for an hour might save thousands from suffering for years. But that would be no answer that society would accept. The killing of the cheapest baby would be murder, and its deliberate slaughter for any reason would be voted damnable. Why? Simply because of the egotism of the human animal, which has chosen to come to the conclusion that he is lord of all, with rights and sanctities inherent, which no other animal possesses. What ground is there for that? May not this conclusion of his be only the culmination of an insolent selfishness which, in action, may very easily make him the "inferior animal" after all? Not so far off, as some may imagine, this right to vivisect is related to the right of the odious vulgarian who desecrates the seashore and the beautiful cliffs for the purpose of shoot-

ing seagulls, whose bodies he is not over-anxious to possess; or that other vulgarian who thinks it sport to practice on pigeons, and other birds, preserved for the delectation of mere slaughterers. What right has this human animal to invade the right of his poor relations, and to rob them of their freedom and their life, for finery or for "sport"? Admitting that these are "inferior creatures," as well as "poor relations," what clause of any conceivable moral law hands them over to us for deliberate torture? Does not the whole thing turn, after all, upon the animal instincts which, at all the lower stages, suggest mere crass selfishness? But surely one of the notes of advance, one of the signs of civilisation, is the desire and the ability to win the higher and broader sympathies, and "let the ape and tiger die." And yet, after all, are we so absolutely sure that we know what birds and animals really are? Some of these not only astonish us with their faculties and affections, but entirely baffle and confound us before their mysterious performances. Is the carrier pigeon an inferior creature? Is the skylark inferior to the callous brute who would shoot it, or even eat it? Is the shepherd dog an inferior creature to the shepherd, who may get drunk every fortnight on bad whisky or raw rum? One need not be over fond of birds and "the lower animals" in order to be almost awed, at times, before their courage, their skill, their patience, their sensitiveness and good taste, their endurance and their love. There is a mystery here that we have not yet fathomed; and, before we talk so freely of "the lower animals," it might be well for us to consider whether we are in a position to judge as to what our relative places in Nature really are. Man's arrogance and masterfulness, always selfish and usually cruel, want repressing, not encouraging; and, in that repressing, we must be willing to pay the price, and to forego knowledge gained through the cowardly exploiting of weaker creatures. We must bid for the ideal man; and for this reason, that

> "When the Ideal Man shall reign
> In this world of tears and pain,
> God and man in him shall meet—
> Heaven his every sense will greet.
>
> He will be a vital part
> Of Great Nature's Mother-Heart.
> All that live, and all that feel,
> Will to his pure heart appeal.
>
> His great sympathy shall flow
> Through all forms of life below;
> Flowers and birds will talk to him;
> And the stars, like Seraphim,
> Through their Heaven-revealing eyes,
> Utter speech of Paradise.
>
> He shall be great Nature's heir,
> Lord of all things everywhere—
> Like a benediction dwell,
> Doing wisely all things well."

8330. Are you acquainted with the law of this country at the present time with regard to experimentation on animals?—I have read a very great deal about it on both sides.

8331. I beg your pardon, there cannot be two sides to the law?—I mean to say the law as interpreted by both sides, for there is a doubt, surely, about the interpretation of the law.

8332. I meant the rules and regulations laid down by the law with regard to experimentation on animals—the Act of Parliament—are you acquainted with that?—I think that, on the whole, I know it. I will not say that I am a legal expert, but I have naturally studied the subject.

8333. I only gathered from your statement that you were not acquainted with it, but I was mistaken, no doubt, in that. Would you tell the Commission can anyone perform these tortures upon animals?—No, I know that quite well. I am perfectly familiar with the fact that there are regulations, and that certain persons are authorised to perform them under certain conditions, but, surely, it is always an open question whether those conditions are complied with.

8334. Do you know what the conditions are that the law requires?—I cannot say that I could recite them; I know them in a general way.

8335. Would you tell me the general effect of them?—I should say in general, for instance, that, broadly speaking, the law requires that the animal to be experimented upon shall be made unconscious. That is what the law, I suppose, requires. I doubt whether it is done.

8336. Does it require anything further?—Will you ask me on any specific point?

8337. With regard to the unconsciousness of the animal?—I have said that the law requires, I suppose—I am not sure in all cases—but I suppose in general the law requires that the animal shall be made unconscious.

8338. What else does the law require?—From my reading of cases I should extremely doubt whether that requirement is always complied with.

8339. From your reading of cases you doubt whether that is complied with. I dare say you will be asked some questions upon it; but what further does the law require besides the animal being rendered unconscious at the time when it is operated upon. Were you not aware that it requires that the animal shall be killed before it recovers consciousness?—I am not aware that in all cases the law requires the animal to be killed: if so, I think that would rather strengthen some parts of my statement. I think it strengthens it in two ways: it strengthens it, in the first place, by the suggestion that the animal is terribly injured beyond hope of recovery; and it strengthens it in this direction, that the creature is entirely deprived of its existence. I do not say that that is a deplorable thing, but I only say that it enters into the case if the animal is rendered worthless and has to be destroyed, through being put under most terrible conditions of distress.

8340. But it is unconscious the whole time?—I doubt it.

8341. What are your reasons for doubting it?—I have read thousands and thousands of pages with regard to that, and I have come in general to that conclusion. I am not here to enter into that side of it. I am here to strongly speak on the ethical side, but I should have been prepared to enter largely into that side, for I have written upon it, and I have extreme doubts whether in very many cases the creature is rendered unconscious.

8342. You also say that the example set by highly-placed and conscientious men may be followed by men not so trustworthy and not inspired by such beneficent intentions. Do you know the terms of the law for obtaining these licences?—I know that the law is often evaded in all kinds of ways. We are living in the midst of law-breakers.

8343. Do you know how a licence for performing an experiment on an animal is obtained?—I do not know the details offhand.

8344. Were you aware at the time you wrote that statement that there is very elaborate machinery for guarding against a licence falling into improper hands?—I suppose it would be so.

8345. I ask you the question: Were you aware that there is existing a very elaborate set of regulations for preventing a licence falling into the hands of an improper person?—I have also a strong impression, from my reading, that it is made very much easier than your question suggests.

8346. Would you be kind enough to furnish us with what your reading has been which has given you that impression?—I shall be extremely glad to be privileged to attend this Commission again to do that. I propose to-day to speak only on the ethical side of the question.

8347. I think if you will merely furnish us with the names of the works which you have read, that will be sufficient. I suppose I should not be wrong in imagining that you would give us the pamphlets of the various anti-vivisection societies as being your authority?—With all submission, I think that you are going beyond your right in saying that. But I only profess to come here to speak on the ethical side of the question.

8348. You see, if I may say so, that we wish only to know on what grounds you are justified in coming here to represent the ethical side. You have not taken up the position that all experimentation upon animals and that all deprivation of animal life by man for any purposes whatever is immoral?—No, I do not take that line.

8349. Therefore we wish to know on what grounds you have formed the opinion which you have expressed?—I profess my statement, and I am perfectly

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prepared to be examined upon the ethical argument which I have put forward.

8350. But in your *précis* you have made certain statements, and we are, I think, entitled to ask you on what grounds you have come to those conclusions?—I set forth all those grounds in my statement, the moral grounds and the ethical grounds.

8351. But you must have had some grounds; I want to know whether you were really acquainted with the law as it is settled, and you have answered that you do not know its details?—I only know in general that the law is as I state it—that it is understood that certain men have to prove so far as they can that they are right persons to perform such experiments.

8352. What is your ground for the statement that the medical profession is not united (of course, it is quite correct to say that it is not unanimous) as to the necessity for or the value of vivisection?—I think that has come within my own observation—that many medical men of standing do not believe it is of any use, but that it is misleading.

8353. That is another question?—That is the question.

8353A. Do you think that the bulk of the medical profession hold that view?—No; I should gather from my observation and reading that the majority of the medical profession believe in vivisection; but, as in the case of vaccination, opinion is divided.

8354. Then you laid great weight in the next statement that you made before us upon the point that observations made during artificially produced agony or distress may mislead?—Yes.

8355. Do you think that those persons who have studied physiology for many years do not know how to make allowances for those misleadings?—I can only say that I hope they do.

8356. They probably take that into consideration, do they not, in performing experiments?—To tell you the truth, I do not know what they take into consideration.

8357. But surely you would give them the credit for taking into consideration such very obvious things as that?—I do not know what the soldier in the heat of battle takes into consideration. He loves his profession, and he glories in the slaughter. Was it not Colonel Burnaby who was refused permission to go out with Kitchener to the Soudan, and in spite of the refusal he secretly went, and simply revelled in the fight?

8358. I do not think that comes into it?—I do not know. I think it is an answer.

8359. What do you mean by that; do you think that these experimenters revel in a number of experiments?—I should certainly revel in them myself if I were a vivisector. Every man loves his profession—loves the details of it.

8360. But there are not professional vivisectors, if I may say so. What do you mean by a professional vivisector?—I mean a man who regularly vivisects—a man who does it. I do not mean, and I do not believe, that it is a man's only business.

8361. What object do you think these people have in vivisection?—My dear Sir, I can imagine myself the delight there must be to the anatomist, for instance, in the dissection of the body. If I could do it, and loved it, I should delight in it for its own sake.

8362. I fail to follow you, but I will not ask you anything further on it?—It seems so obvious to me that a man who is a chemist must love his profession; it does not matter whether the man is making money or fame through it, the chemist must love his profession, and all the stinks of it. He must take delight in it. A great chemist in the north said to me, "People come here disgusted with the smell of this place; I love them all."

8363. Your own belief then is, that these vivisectors vivisect for their own amusement because they are so fond of doing it, and not for any purpose?—I would not say that. What I would say is that a man who believes in vivisection and believes that there are discoveries to be made which may be useful—I would admit all that honestly—thinks it is a useful thing, and may think he is doing God's service, even.

8364. Do you think people vivisect without a special object?—Yes, most assuredly; I should myself if I were a vivisector. I should experiment for the love of it.

8365. But you have no other ground except your own feeling for believing that?—I do not know; I think it is human nature. I think there are thousands of chemists to-day, and electricians, who are experimenting and worrying themselves, and working as hard as possible without any object except the chance of finding out something. I know it; I know many experimenters.

8366. And that is what you believe is the case with physiologists, too?—With many of them; I should think with most of them. I was going to say I hope with all of them. A man ought to love his profession.

8367. That is to say, that they put an animal under chloroform, and cut it up without knowing what they wish to observe?—No, I should not say that. The chemist knows what he wants to observe, the electrician knows what he wants to observe, but they do not know what is coming of their researches.

8368. Could progress ever take place in knowledge of any sort or kind if you knew beforehand what would come?—But then you do not know; the greatest discoverer does not know what is about to come. The man who discovered radium did not know what was coming. He was a seeker for years and years before he got the result, and the result came unexpectedly.

8369. You made a special reference, I think, to what you called Dr. Abernethy's verdict? What was that?—I quoted it.

8370. I am afraid I was unable to follow it; I was not acquainted with it myself?—Do you wish me to read it again?

8371. If you please?—"Vivisection has the direct effect of deteriorating the moral sense; and once the moral sense is destroyed it is impossible to foresee the consequences."

8372. Would you give me the reference to Mr. Abernethy—he was Mr. Abernethy. Was it in his lectures?—I will do my very best to find the reference. I have not got it here. I did not know that you required the book and the page.

8373. But you see you offer that as an important fact, and we want to know in what connection Mr. Abernethy said it. Do you know when Mr. Abernethy wrote that?—I was not with him when he wrote it.

8374. Do you know how long ago it was?—I shall be very glad to look it up and let you have the reference if you wish it.

8375. Did he write it before or after the time when anæsthetics were used in experimentation?—I cannot say.

8376. Then perhaps you would kindly let us know the reference?—I will do my best to find the reference.

8377. Do you consider that man has a right to use the lower animals for his convenience, and for his food?—For his convenience almost assuredly; for his food I have doubts.

8378. Are you a vegetarian?—No, I am not. I have very serious æsthetic doubts about it.

8379. But you are quite clear that man has no right to use the lower animals for the advancement of knowledge or the benefit of science, and indirectly, of medicine?—No, I have not said that. I think that man has no business to use the lower animals in the way of vivisection for that purpose.

8380. So that you would go for the total abolition of vivisection?—Certainly.

8381. Do you think that there is a great difference between seeing operations done on animals and seeing them done on man?—I do not know; I cannot say. I have never had any reason for observing. I have not seen them myself.

8382. But you are aware that in the medical profession, when men are learning their profession, they witness operations upon man?—I should think that in all these cases they are distinctly for doing good to the subject. That is not the case in vivisection.

8383. You stated also that you thought that there was a spirit arising which would lead to experimentation upon man in hospitals?—I should think that is very natural.

8384. But is not almost every case in hospital an experiment?—I do not think so; not for the purpose, surely, of finding out knowledge independent of the patient. I should hope that in every case whatever was done was for the patient's good.

8385. But is not almost every case watched in order that something perhaps may be learned from it?—That is very dangerous.

8386. (*Colonel Lockwood.*) You are entirely ethical, and you know nothing of your own knowledge as to experiments on living animals?—That is so.

8387. Or as to how they are conducted, whether under anæsthetics or without anæsthetics?—As to my own personal knowledge I certainly do not know. I should imagine that very few people are acquainted with that. One only gathers from one's earnest, honest reading.

8388. All your knowledge of experiments is second-hand?—It must be.

8389. And we are to take your evidence for what it is worth on the purely ethical question?—That is so; that is what I said.

8390. Can you suggest to this Commission any better regulations for experimentation on living animals?—No, I cannot.

8391. You probably would not like to suggest any, because you are against the practice altogether?—That is so. I cannot suggest any improvement of the regulations.

8392. That is all I wanted to know; you have no suggestions to make?—None whatever with regard to regulations. Simply on the ethical ground I am against the whole thing.

8393. (*Sir William Collins.*) You state in your *précis* that the practice of vivisection is ethically bad, and I understand you to say that it ought to be prohibited by law. Is that so?—I should think that it ought to be prohibited by law. I suppose it must be if it is to be prevented. What I should wish would be that men would not want to do it.

8394. But we are a Commission charged to advise His Majesty as to what alterations, if any, should be made in the existing law, and I wanted to know what suggestion you make in regard to that?—My position is that of an abolitionist.

8395. Is that because vivisection is ethically bad?—Not only on that ground; because I did venture in my statement to include a doubt as to its efficiency, its utility, and the possibility of its being misleading.

8396. If it were found to be useful and yet ethically bad, should you advise its prohibition by law?—I think I should, on the ground that I do not quite think we have a right to exploit these mysterious creatures, very mysterious creatures in my judgment.

8397. Is it because you think that everything that is ethically bad should be prohibited by law?—Oh, well, that is a very broad question. I want as little law as possible. I want men to want to do right.

8398. I can understand your being opposed to vivisection on ethical grounds yourself and your desire that other people should think with you; but I wanted to know whether you think that because it is ethically bad and perhaps useless as well, therefore it ought to be prohibited by law?—I think that vivisection should be prohibited by law. I should have to consider a good deal as to all kinds of ethically bad things. For instance, lying is ethically bad; I do not know whether you should prohibit it by law; it is such a big question.

8399. May I ask why you select this one of many ethically bad things for prohibition by law, while others would remain permissible?—That is a question which I have answered in my statement. It is ethically bad on various public grounds. For instance, take the case of a doctor. I have a reverence for the medical profession; I am not against doctors, I reverence them; every year I preach a sermon in favour of doctors, but I should be very sorry to have attending me, if I were ill, a doctor who was bluntly addicted to vivisection.

8400. I want to follow out how it would be practically possible, even if it were desirable, to prohibit vivisection by law?—I suppose you could make law that no one should practise vivisection, and yet many people would do it on the sly. I doubt whether you could actually do away with it by law, but I would like it abolished by law. I should doubt the efficiency of such a law, just as I doubt the efficiency of the law as it stands.

8401. Do you think that a more stringent prohibitive law than that which at present exists would be liable to be evaded?—I think so. I believe that the provisions of the law are evaded now.

8402. Then, is there any practicable suggestion which you think we could make to His Majesty as to the alterations in the law, if we desired to restrict vivisection further than the restriction which exists at the present time?—I do not know of any.

8403. Then, is it rather by propagating your views and educating opinion that you would attain your end, than by penal legislation?—I am afraid that the other method would take too long.

8404. What is the other method?—The other method is what you suggest, appealing to the law.

8405. That, I understand, you thought would be an impracticable method?—Yes, I do. I have said already that I do not suggest it.

8406. Then, there is no practical suggestion that you are able to make?—Except abolition.

8407. I understood you to say that that was not practicable, because it would be evaded?—There is always the chance that you may catch your criminal.

8408. I thought you admitted a large liability to his escaping?—I think that now, while you do permit it, you do get a certain number of people who practise vivisection under legal conditions, and they are not criminals and therefore they go on with it; but there are criminals—all laws are evaded; and I do not think it follows at all that you must not legislate, or that you should fear to legistlate, because the law may be evaded. The law now is justifying vivisection; I want the law not to justify vivisection

8409. Do you think that it raises public respect for the law if you pass laws which it is well known will be evaded?—I would like the sinner to be a criminal. I put it roughly, but you are putting it roughly.

8410. I have not yet got from you a valuable suggestion as to what modification, if any, is desirable in the law?—I have said over and over again that I do not propose any modification of the law. I want the law abolished.

8411. But you limit yourself to ethical grounds, and you do not wish to give evidence upon the scientific, or so-called scientific grounds?—I have said so. I am not a scientist.

8412. (*Sir John McFadyean.*) You desire to give evidence on the ethical side of the question, and I notice that you rather resent being asked any questions bearing on the actual practice of vivisection?—I do not resent it.

8413. But I am going to put a question to you in this way: Are not your views as to the morality of vivisection based on certain conceptions which you now have as to what the word actually covers in practice? I mean that vivisection calls up to your mind certain practices, does it not?—Yes.

8414. And it is on that that your views as to the morality of it are based?—Not upon actually what is going on.

8415. But surely your views as to the morality of vivisection must depend partly on what vivisection actually is?—What it is in itself.

8416. Then it is permissible for me to put to you a few questions to ascertain whether your views as to what vivisection as practised in this country really means are wrong?—No, that is not it.

8417. Perhaps the sentence was too long?—No, it is not that; it is vivisection *per se*.

8418. But there is no such thing, if you will allow me to say so, as vivisection *per se*. When you speak about vivisection before this Commission you must mean what is practised under that name in this country, must you not?—I do not know that that is so.

8419. Do you, or do you not, mean that?—No. I do not confine myself to what is vivisection as practised to-day. If there was no vivisection going on I should be opposed to vivisection *per se*.

8420. But vivisection is not an abstract thing. Your views as to the morality of it must depend upon what the nature of vivisection is?—Oh, no; you might just as well ask me about murder. I disapprove of murder *per se*. I am not to be asked whether I disapprove of murder as carried on in this country.

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8421. Will you forgive me for saying that murder stands in no need of definition; but vivisection may. I will lead up to the point in this way. I notice that whenever you refer to the pain of vivisection you use expressions which indicate that in your opinion vivisectional operations in this country involve great pain. For instance, I noticed that you use the words "horribly tortured," "revelling in this disgusting work," "the artificially-produced agony in a dog," and other such expressions. I submit that it is perfectly fair for me to ask you what information within your own possession has led you to conclude that any experimenter in this country is in the habit of torturing a dog, or artificially producing agony in a dog?—If I had thought that I should have been asked for references to justify me in those statements, I should have brought my pile of books and publications, from which I could have quoted.

8422. I am afraid that you did not realise your responsibility. You have really made a very serious charge against licensed vivisectors in this country, and I submit that you ought not to have lightly done that without being ready to produce evidence of what would be an offence against the law, namely, for any licensed vivisector to artificially put a dog in agony, or to carry out any experiment which could correctly be described as "horribly torturing" that dog. I want to know, do you hold to these expressions as being correct?—Certainly; from my reading and conversations I feel justified in making the statements.

8423. Then I must ask you, for the information of the Commission, to extract from this huge mass of material some half dozen cases in which you think that the author's account of the experiments justifies their being described as "horrible torture"?—I have not restricted myself to the practice of vivisection in this country. I have written and spoken about vivisection as practised anywhere.

8424. I will put it in this way: This Commission has only got to deal with vivisection as practised within the limits of the three kingdoms. Supposing the Commission, from the evidence put before them, arrive at the conclusion that your estimate as to what vivisection is is totally wrong, that is to say, that it does not involve the horrible torturing of animals, would you admit that the Commission ought to discount your evidence as to the morality of vivisection?—I do not think so.

8425. I am quite satisfied with the answer. Did I rightly understand you to mean that man is never justified in inflicting pain upon an animal, except for that individual animal's good?—I did not say that.

8426. Will you tell me what does justify man in inflicting pain upon an animal?—If I answered broadly Yes to that question, you might say that a man must not give his horse a taste of the whip. It is a big broad question. You may do all sorts of things. You may whip a dog that you love passionately?—I do not love any dog at all, but I have seen women who do give a dog a lash with the whip.

8427. Are these words not to be found in the evidence which you read to us a few minutes ago?—I do not think so.

8428. I submit that the words "Causing pain except for the animal's good" came in in some connection, and I wondered in what connection it was. You objected to vivisectional experiments because the pain was not inflicted for the animal's good, did you not?—Yes. But taking vivisection as it, there is a difference between vivisection and a lash of a whip.

8429. Taking vivisection as you conceive it to be, if you will forgive me, I think is a safer expression?—As it is in the world at large.

8430. You do admit that man is justified in certain circumstances in inflicting pain on the lower animals?—It is a broad question to which I could not say yes or no.

8431. I will also take that for an answer?—I would like to get at the bottom of it, and I think in five minutes we might if we talked it out. It is such a broad question. It is a question which seems to seek for an admission which will be turned to my disadvantage.

8432. There is no question of disadvantage really. We are seeking here to arrive at the truth?—It is a criticism of what I said in my statement, and you are putting a question as broad as that—"inflicting pain on animals." There are grades of pain from the lash of a tiny whip to cutting up an animal alive.

8433. I will help you out with it. Is man justified in inflicting on the lower animals considerable pain for any other purpose than the individual animal's own benefit?—Any vivisectional pain?

8434. Let us put aside the way in which it is inflicted. Is man in any circumstances other than the individual animal's benefit justified in causing considerable pain?—There must be degrees when you speak of inflicting pain. I will give you a case. I do not love my dog; my wife does. I have seen her give it three strokes with a whip—gentle strokes, fairly gentle—and I have thought: Well, on the whole, it is rather a sharp thing to do. But if she were to kick that dog mercilessly, I should say it was base. It is a question of degree.

8435. I will put the question to you in this way: Do you think that man is justified, for the mere purpose of improving the quality of his animal food, in subjecting male animals to the severe vivisectional operation which is known as castration. You know what the operation of castration is as applied to an animal?—Yes, but is that for the animal's good?

8436. Although you have never seen it done, with the imagination that you possess you must be able to figure to yourself how exceedingly painful an operation it is when done without anæsthetics. Do you think it is justifiable?—Did you say for the animal's good?

8437. I did not say that?—I thought you did.

8438. I said that it was done for man's good—to improve the quality of his food?—I should not like the man who did it.

8439. It is not a question of the man who did it. Will you answer the question?—But there might be hundreds of questions of that kind, and I should give that sort of answer to them all.

8440. But I do not know what the answer is. Do you think it is justifiable?—No.

8441. I want to see whether you are perfectly consistent. Do you invariably ask for bull beef when you send to the butchers' for beef?—Never. I did not know that there was any difference.

8442. You will find the difference if you ask for it once and get it. Do you not think that it is very inconsistent in you, merely for good digestion or taste?—But I do not.

8443. But you spoke elsewhere of foregoing advantages derived from vivisection; why should you not set your face against this cruelty?—I should, decidedly. If a butcher told me, Here is one bit of meat got through this operation, and here is another, I should say, I will have neither of them.

8444. That is hardly going far enough. Ought you not to ask for bull beef, for instance?—I did not know there was such a distinction. I am not such a connoisseur, such a *gourmand*, in the matter.

8445. That is because you have always eaten the good sort, which is got by a painful method?—No, because I shrink from eating meat at all. I take no interest in the matter. I am almost persuaded to be a Christian in regard to that.

8446. But you are not quite consistent?—I admit it; I am not.

8447. (*Mr. Ram.*) Do you deny that any useful knowledge has been, or can be, obtained by vivisection, or that in any case would the acquisition of such knowledge be lawful?—I do not deny that knowledge can be obtained.

8448. Do you, then, deny that in any case it is lawful to attempt to get such useful knowledge?—Through vivisection it is not lawful.

8449. Do you base your objection to vivisection wholly upon the pain inflicted upon the animal?—I have never seen vivisection done, but I should imagine that there are various sorts of distress, not merely pain; but distress would be pain, too. I do not know what happens to an animal under vivisection, except that there are all the signs of misery, apparently.

8450. All the signs of misery, apparently?—Yes.

8451. How do you know that?—In all the experiments I have heard of—not on one side, as was suggested just now, from anti-vivisection societies, but I have read books and pamphlets by believers in vivisection, which indicate distress.

8452. Accounts which indicate distress?—Distress and agony.

8453. Then may I take it that it is because of the pain in the cases of which you have read that you base your objection to vivisection?—Not entirely; that is one reason.

8454. You have told me that useful knowledge may be obtained?—No.

8455. I asked you whether you deny that knowledge may be obtained, and you said no?—If there were. I do not say that there is.

8456. Take it hypothetically. If useful knowledge can be obtained by vivisection of an animal which is subjected to no sort of pain or distress should you object to that vivisection?—I do not know that it could be.

8457. But you see you have come here to talk ethically to us, and I am trying to get really at what is your meaning?—Is it possible that you are in earnest?

8458. We have heard of many cases of it; anyhow, will you kindly imagine an animal subjected to no sort of pain or distress at all, an animal put under anæsthetics without pain, which is to die before it recovers from the anæsthetic. Is it wrong in such a case to attempt to get knowledge?—I do not know what happens under anæsthetics; I cannot say. There are degrees of the distress.

8459. I am putting to you the case of an animal that is absolutely anæsthetised, and without pain or anæstheic before it can possibly recover any consciousness. Is it wrong to attempt to obtain useful knowledge by examination of that animal?—I do not know that it is quite fair to put a hypothetical case like that.

8460. I will take that answer, if you please. May I take it that you decline to answer that hypothetical case—is that your answer? (*After a pause.*) Is that the answer?—It would be a hypothetical answer to a hypothetical question.

8461. Let us have the hypothetical answer if you cannot give anything else. What is the hypothetical answer?—That is the answer.

8462. I will leave it there if you like. Do you think it is wrong for a surgeon to gain knowledge of anatomy through examining a dead animal?—No.

8463. Do you think it is wrong for a surgeon to kill an animal painlessly in order that he may examine it after it is dead?—I think it is.

8464. Is it wrong under any circumstances to take the life of an animal?—I do not think it is. That is my present stage. I have a doubtful opinion about it. I have said that already. With regard to killing animals for food, I am in a transition stage, as many men are.

8465. Since luncheon?—No.

8466. Was it permissible to kill the animal of which you last ate a portion?—I am in doubt about it.

8467. Take the case of the Dogs' Home. There is a miserable, lost, suffering, diseased dog at the Dogs' Home; no one claims it; it is put into the lethal chamber, and perfectly painlessly it dies. Is that right or wrong?—That is all right.

8468. Are you sure of that?—Yes.

8469. Thirty thousand dogs a year?—If you give me the case of an individual dog I will give you an answer. Your question a little while ago was so broadly hypothetical that it covered my wife's dog.

8470. There was nothing broadly hypothetical about the case of the animal that was killed to give you the last meal that you had of animal food. Was that right or wrong?—That is too broad.

8471. I come to the individual animal, the last animal that was killed in order that you might have animal food. Was that right or wrong?—I say I am in doubt about it; I am in a transition stage, as a vast number of men and women are to-day about that.

8472. You have not made up your mind what you will have for dinner to-night, then, I take it?—Very likely. Over and over again I say, "No, thank you, no meat."

8473. But still you are weak enough to take some?—I am in a doubtful frame of mind about it.

8474. Would it be the same thing with regard to fish and soup?—Yes.

8475. You are a vegetarian pure and simple?—No.

8476. Is it wrong to kill animals painlessly in order that their hides may be turned into leather?—That, again, is a doubtful question.

8477. I think I see a pair of very well soled shoes?—I know; I know we are all inconsistent.

8478. I am glad to say I am not in the least inconsistent about that?—We are all inconsistent—we are bound to be. You see some things clearly and other things you see doubtfully.

8479. You put the case of vivisecting a baby, and you said that everybody would rise with horror against such an idea. Do you put babies and animals exactly on the same plane with regard to vivisection?—That is precisely what I did not do. I drew a broad contrast.

8480. What is the contrast?—That is what I want to know. That is the question I put in my statement. That is a question for the vivisectors to answer.

8481. I thought it was rather a question for an ethical professor to answer?—No. It is a question that I have put to them.

8482. There is no objection to eating an animal that has been killed for food, at all events, if you have no doubts about it, is there?—But are there doubts?

8483. There is nothing wrong in eating animals as food?—That is the same question. I am in a doubtful state of mind about it.

8484. Is it doubtful whether you ought to eat a baby killed for food?—No, it is not.

8485. What makes the difference between a baby and a sheep?—That is what I want to know. I have been asking that. My statement asks that.

8486. We think that we do know?—No. I want an answer from the vivisector. The vivisector is bound to answer me that.

8487. Do you think it is wrong to drive a horse in a cab?—No.

8488. Do you think it is wrong to drive a man in a cab?—If the man liked it——.

8489. But if he did not like it, with the assistance of the whip, is that right or wrong?—I should think it very wrong.

8490. Why is it right to drive a horse and not right to drive a man?—Well, you can consult the man; you cannot consult the horse.

8491. But supposing the man says "No," and yet you drive him, is that right or wrong. Wrong you would say?—No, I would not.

8492. Not wrong to drive a man if he said "No"?—I should ask whether you are in earnest.

8493. Whether I am?—Yes.

8494. I am absolutely in earnest. I want to get at the distinction which you have quite gravely put to us, and I am quite gravely wanting to ascertain, if I can, the meaning of such phrases as you have used here. Why is it wrong to drive a man against his will and not wrong to drive a horse?—So far as I can get into the mind of a horse the horse has no opinion upon it; the man has.

8495. Is that the reason why you think it is right to drive a horse and wrong to drive a man—because the horse does not care whether it is driven or not?—No, it is not that. I do not know what the horse thinks about it; I have no means of knowing the horse's mind. I know the man's mind. But it may not be wrong to drive a man in a cab if he is foolish enough or slavish enough to permit it.

8496. Is that the only answer which you desire to give?—That is the answer.

8497. That is in your mind the distinction between so treating a man and so treating a horse?—Yes, that is the distinction.

8498. Can you give any other distinction?—That will do; that is enough.

8499. And you cannot give me any other?—I do not think that any other is needed.

8500. Have you any other that you could give me. (*After a pause.*) May I take it that you have not?—You may take it that I think that is the necessary answer; that answer is all that I care to give.

8501. I have only a little more to trouble you with.

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Will you tell me this: have you any suggestion to make as to the recommendation which this Commission ought to make to His Majesty the King?—I have already said that I should be delighted if the Commission would recommend that vivisection should be abolished by law, that is to say, if vivisection were not recognised by law.

8502. Then that means that you desire the repeal of the present Act?—Certainly.

8503. And would you desire the enactment of any Act, and, if so, what, in its place?—It is immaterial whether you abolish the present Act or pass an Act forbidding vivisection. I should prefer that an Act should be passed forbidding it.

8504. Abolish the present Act and pass an Act forbidding it?—Yes.

8505. Do you think that would stop vivisection?—I am afraid not.

8506. Which do you think is the worse, so far as vivisection is concerned, the state of affairs in England or the state of affairs in either France or America?—That I cannot say.

8507. You have not studied it?—I have generally read about it, but I do not know truly what is going on in England.

8508. Do you know what is going on in either France or America?—I have read of it.

8509. Have you read of the horrors which take place there?—I have read very much about France.

8510. Take France, then. Have you read about the horrors that are taking place there?—Yes.

8511. Are you aware that there is no law regulating vivisection in France?—I have already said that I should like a law passed forbidding it.

8512. But are you aware that to-day there is no law in France which regulates vivisection; do you know that?—I took that for granted from what I have read.

8513. You know that there is a law in England which regulates vivisection?—Yes.

8514. Which do you think is the better condition with regard to vivisection, the state of things in France or the state of things in England?—That is not a fair question, because I have already said that I want a law passed to forbid vivisection—to make vivisection criminal.

8515. If you do not like to answer the question I will leave it. You want to make vivisection criminal?—Yes.

8516. Would that be wholly regardless of the object with which any vivisection might be performed—that you would make it criminal in every case?—Yes.

8517. Would you also make criminal the performance of any operation on any animal except for that animal's good?—Speaking generally I should.

8518. Are you aware of the farmyard operations which take place by the thousands every day on colts and young stock of all sorts?—For the animal's good?

8519. Not in the least for the animal's good: in order to make their meat taste better for man. Would you allow them or disallow them?—I should disallow them.

8520. Would you make them criminal?—Is there vivisection?

8521. Undoubtedly there is vivisection of a most exceedingly painful kind without any anæsthetic?—I think I should.

8522. You would make it criminal to perform any such experiments?—I think I should.

8523. You said that the practice of vivisection was ethically bad and hurtful to public morals, because it is based entirely upon human selfishness and the assumed right to capture, torment and destroy animals for repeated experiments that may prove nothing, or mislead, and for experiments which turn upon distress, and begin and end there. Is it because those experiments, as you express it, in your opinion, are for torment, and turn upon distress, and begin and end there, that you object to them?—That is the general reasons—that is the ethical reason.

8524. And if experiments did not torment, and did not turn upon any distress, or inflict any distress at all, should you still object to them?—I do not believe it is possible.

8525. Will you imagine the case?—I cannot imagine it. I do not trust my fellow creatures enough.

8526. To do what—to imagine a case?—To keep within the limits of what you suggest. You see what I mean.

8527. No, I do not in the least.—There is the old hypothetical question again. The answer must be hypothetical. If human beings could be trusted to keep within those limits, I might say that I did not object. I think it is too dangerous.

8528. Is it because you do not trust any of your human fellow creatures?—I do not say "any."

8529. Because you do not trust some of your human fellow creatures to abstain from unnecessary pain, that you would object to vivisection altogether: is that the reason?—It is very near it.

8530. Can you put me any nearer, or do you adopt those words?—That is quite enough.

8531 (*Mr. Tomkinson.*) By the repeal of the law you mean, I presume, that the immunity which is given to the system now should be removed, and that all experimenters should be subjected to prosecution for cruelty to animals?—Yes.

8532. That would be the effect of a mere repeal of the present regulations?—I think that is so.

8533. Would you go further and have a special Act passed to make vivisection criminal?—If the mere repeal would be sufficient I would not want a law to specially make it criminal.

8534. You have been asked as to whether you think the results of a repeal of the law might be ill. Of course your object is to diminish the sum of animal suffering?—That is the main object, but it is not only that. My statement indicated other reasons—to diminish the danger to the vivisectors, for instance.

8535. It has been put to you whether the result of abolition might not be to drive more of the system into other countries, where infinitely less mercy is shown to these animals?—That is likely.

8536. Or whether, secondly, it might drive the practice into secrecy?—That is very likely.

8537. Instead of its being practised openly in laboratories?—Is it?

8538. Not openly to the public—I mean that there is inspection—that it is under regulation; that it might have a tendency to drive it under the surface and into secrecy in private houses?—Yes.

8539. That occurs to you as a possible alternative?—Yes; but that is true of a great many things, you know; that is true of prostitution. Those questions all require a good deal of discrimination in both asking and answering. You may drive a thing under the surface and make it worse—one admits that possibility; but none the less should the State, the nation, clear itself.

8540. Of complicity?—Yes, that is what I should say.

8541. In regard to the question of exploiting animals for food, I gather that your mind is in a state of suspension?—In a great state of suspense.

8542. You said something in your statement about sport, but you only instanced such a thing as pigeon-shooting?—Yes.

8543. Does the same objection apply to all sport?—It applies to rabbit coursing and all those things. I would not apply it to horse racing, for instance.

8544. But to shooting?—Yes, I should think so. I never read, for instance, about the King's kind of shooting without a feeling of disgust.

8545. If you object to shooting, would that lead you logically to refuse to eat a pheasant or a partridge?—It makes me feel on the whole that I rather want to be a vegetarian.

8546. I see that you are turning that way; but hitherto you have not refused to eat a pheasant or a partridge?—I have eaten them.

8547. But you must be aware, as we all are, that an immense amount of cruelty is committed in even slaughter-houses?—Yes.

8548. And that pain is inflicted upon animals in those various operations which have been spoken of?—Yes.

8549. Do you draw any distinction with regard to

animals. You admit, I suppose, that it is necessary to destroy rats and animals of that sort?—Yes.

8550. Otherwise it would be a case of being eaten up by rats?—There again it is a question of degree—how you destroy your rats. There is a certain way of destroying a rat which is permissible. There is another way of destroying a rat which is vulgar and brutal.

8551. In regard to the common way of destroying or diminishing the number of rabbits, have you thought what trapping means? Have you ever seen a steel trap, which is the common mode of catching a rabbit?—No, I have not. I can quite imagine that very many ways of destroying wild animals are very brutal.

8552. They are extremely cruel. Do not you think that, logically, one ought to set one's face against that, as well as against vivisection?—I should be very sorry to catch these creatures in that cruel way.

8553. Then, as to that distinction of stray dogs, this instance has been put to the Commission: given a dog that is under sentence of euthanasia, to be put into the lethal chamber——?—I rather like that.

8554. Would you protest against that same dog, which is under sentence of death, being utilised for some experiment?—I should not feel enough faith in my fellow-creatures to believe that it would be used without what I disapprove of. The lethal chamber, I suppose, is a genuine article, and there is no distress there, and there is no risk of margin for prolonged agony. In fact, I would rather like to be put in the lethal chamber myself if I were under sentence.

8555. With regard to that question which you found it so difficult to answer, I imagine that it really amounts to this: that you are sceptical as to perfect painlessness being insured, or anything approaching to perfect painlessness?—That is a very essential part of my feeling.

8556. But, even if you could be satisfied that no pain was inflicted, you would still object to the principle?—Not only to the principle; but, in my short statement I give two or three reasons why I think, apart from the misery of the animal, it should not be allowed—namely, the doubt as to the results being of value or the reverse, and the doubt as to the influence on the vivisector's ethical condition. That is a very important matter.

8557. You are aware, probably, that an enormous preponderance of operations on animals are very slight operations, of inoculation?—Yes.

8558. Operations which, in themselves, practically cause no pain?—Yes.

8559. To those you have less objection probably?—Certainly.

8560. (*Sir Mackenzie Chalmers.*) You have given evidence on the ethical side of vivisection?—Almost entirely.

8561. You know that other people take a different view from that which you take?—Certainly.

8562. And that, although, ethically, to you it is wrong, it is justifiable to other people?—That is all we can do.

8563. I want to ask you what is your test of what is ethically right or wrong—your own particular conscience?—My own conscience, my education, and my general spiritual condition.

8564. That is to say, it is merely a question of individual judgment?—Certainly.

8565. You have no outside standard to apply?—No. In regard to matters of this kind one must judge from one's own spiritual and moral condition as to what is right for others, although they may not feel it so. You must go by your own judgment and moral sense in any case.

8566. And where there is a conflict of what I may call ethics, or ethical opinion, is there not great difficulty in legislating?—You do not find it so in ordinary life. Lying is lying.

8567. But you would hardly legislate against lying?—I would not legislate against lying.

8568. You would not?—When it came to practical operation, as in lying about a bargain, or fraud in relation to a contract, you must legislate; but against lying as lying you cannot legislate.

8569. Are we talking always of the same thing. What do you understand by vivsection—any experiment on animals, or only experiments producing pain?—I have freely said that there are grades of objection to vivisection; it extends from kicking a dog, which I mentioned just now, up to most cruel experiments. There are various grades of objection.

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8570. By vivisection, you mean anything that would cause pain to an animal?—That is rather too broad a definition. If you added "for the animal's good" the objection does not quite go so far.

8571. Are we talking about the same thing when you object to vivisection, and other people are in favour of it, or are we covering different spheres?—I do not know at all. I should have to get into your mind.

8572. That is why I asked you how you would define vivisection?—I cannot define it except by taking the dictionary meaning of the word: "Cutting up an animal alive." Then I say that there are endless grades of objection from what is almost unimportant up to devilry. And then when you ask: "Are we talking about the same thing?" I should say that we are talking in the main about the same thing; but perhaps we may not at the same moment be talking about the same degree.

8573. Do you object, for instance, to feeding experiments. A great many experiments which are licensed under the Act are feeding experiments?—I do not know much about them. I know nothing about them, in fact.

8574. They come in under the present law, as you know?—Not under vivisection.

8575. They have to be licensed?—That is another thing altogether; but I know nothing about them at all.

8576. But still they are included in the returns of experiments?—They are very improperly called vivisection.

8577. As you are aware, there is no such term as vivisection known to the law. The existing law of England deals with experiments upon animals?—Yes.

8578. Have you ever read the Act?—I must have read it.

8579. You will find that the word vivisection is not mentioned; it is a law regulating experiments on animals. What I want to know is how far you object to all experiments on animals?—I know nothing whatever about the feeding experiments regulated by law. I did not know that they existed. I knew that there were very many experiments as to foods, that is to say, with human beings.

8580. Still if by any proposed amendment of the law you took the existing Act and made all the experiments under it unlawful, you would make feeding experiments unlawful. You would not wish to go so far as that?—I should like to know what those feeding experiments were; they might be abominably cruel. You might stuff an animal till it was suffering greatly.

8581. I will give you an example. A licensed experimenter applied the other day for leave to feed kittens with cow's milk instead of cat's milk, to test the effect of changing the mother's milk. Would you prohibit that?—Well, putting it as you do, I should say that I would like the Act entirely abolished.

8582. That is to say that everything done to an animal for the purpose of advancing knowledge you would forbid?—No, I think I would abolish the present Act. That would leave the experimenter still with his experiment on cat's or cow's milk.

8583. You would let it depend upon the general law of cruelty?—Yes, it would take its place in that way.

8584. Would that help you?—I do not know. I should hope that it would.

8585. I want to test you a little further to see how far we agree. We have had some evidence this morning to the effect that we lose in India 21,000 human lives a year from snake bite. How far in your opinion is it justifiable to experiment on animals with a new suggested cure?—With the virus?

8586. No, that the animal is to be inoculated with the virus, and then the suggested cure is to be applied to see whether it is efficacious?—I would rather go for the snakes.

8587. That is not quite an answer to my question. How far is it ethically justifiable?—Indeed that is the answer. It is very similar to the case of vaccination, and what I said about that was that I would rather go

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for the insanitary conditions. I would turn people's attention away from the cure by inoculation, and turn their attention entirely in the direction in which they can prevent disease altogether. I think that is the safer and more profitable course.

8588. I think you may take it for granted that we cannot exterminate snakes in India, and that you would lose many many thousands of lives before you reduced the number of snakes. Is it ethically justifiable to try a new cure upon an animal for the purpose of saving human life?—I take that case of snakes in India, and I should say I doubt whether what you suggest is the right way.

8589. Take the case of plague, from which we lose many millions; is it justifiable to try any experiments on rats with regard to new cures or new modes of prophylaxis?—Could you confine it to rats. The trouble with me all the way through is that I do not trust the operators. I should go a great deal further in relation to being practically careless about vivisection if I could trust my fellow creatures more. I cannot; I do not believe it is possible.

8590. When you say that you cannot trust them, is it on purely general principles, or because you know anything about vivisectors?—Because of what I know about human nature.

8591. Have you made inquiries in laboratories, or made inquiries from people who carry on experiments?—I do not know them. My knowledge is derived entirely through reading and conversation.

8592. Conversation with whom?—Not with anti-vivisectors only.

8593. Conversation with people who have studied the subject?—Yes. Last Sunday I was talking to a London doctor, himself in favour of vivisection. I get information from these people as much as from others. I do not want to begin inoculation; I dread it—I suspect it.

8594. But you have never made any practical inquiry into the conduct of individual people as to whether they use full anæsthesia or not?—I have had no opportunity of doing so. I do not always know whether any of the friends that I talk with do vivisection or not. I do not know whether my doctor friend that I talked with last Sunday does. I did not ask him.

8595. You do not know whether he has held a licence, or whether he has ever been in a laboratory?—I do not know.

8596. Your distrust, then, is founded upon your general knowledge of human nature, and not on individual inquiry which you have made into the practice in England?—I could not say that. I have been for eight or ten years a rather industrious reader on the subject, reading all sorts of books, and talking with people, medical men and the like, on both sides.

8597. I understand you to say that you equally condemn killing an animal for pure sport as you would for the advancement of knowledge?—No.

8598. Which do you consider the more wrong?—There again you get on to degrees.

8599. Take the case of a good day's rabbit shooting?—I would not like to go rabbiting—that I confess.

8600. Would you prohibit it by law?—Certainly not.

8601. Much greater pain may be caused in that way?—I have no strong opinion about rabbit shooting at all. I have a very strong opinion about what goes on in London and the neighbourhood of London on Sunday afternoons—letting rabbits out of bags for dogs to capture.

8602. And whippet racing?—That is simply vulgar brutality—that is another grade.

8603. In ordinary rabbit shooting you may take it that 5 per cent. of the rabbits are not killed, only wounded?—I am very sorry.

8604. Still, you would not prohibit that by law?—No, I do not think I would.

8605. So that ethically, if I may put it so, it is less wrong to kill or maim for sport than it is for the advancement of knowledge?—No, that is not so, because you leave out there the many other considerations which make habitual vivisection dangerous and cruel.

8606. Which do not make sport dangerous and cruel. I am not talking of racing or anything of that kind; I am talking of sport, in which the object is to kill the animal?—It depends upon what you want to kill it for.

8607. Take foxhunting?—I hate it. The only ethical reason for that is that you must destroy foxes, which would destroy more useful things, but foxhunting, as carried on, is to me most hateful.

8608. It is not the quickest way of killing a fox?—It is most hateful. I almost begin there to want to prohibit it. That is another stage. I resent it very much.

8609. I suppose you know that every horse in England that is not kept for breeding purposes is castrated?—I did not know that. I am very sorry to hear it.

8610. If you look in the streets you will not find a cab horse that is not a gelding in England?—I am sorry for it. I would rather take my chance of the horse.

8611. You know that the operation of gelding a horse is most hideously painful?—I hate it; I loathe it, now that you tell me of it.

8612. Would you prohibit it?—When you ask me whether I would prohibit these various things, what I said was, I would leave all these matters to the common law of cruelty to animals, and then judge every case on its merits.

8613. I daresay you remember that under the Act against cruelty it must be wilful cruelty. The words, you know, are "cruelly tortured"; it does not refer to the amount of pain, but to the intention of causing pain, or negligently causing pain without any useful purpose. Would that help you?—It makes it worse.

8614. Then your only practical suggestion to us is to repeal the present Act and leave the existing Law of Cruelty to Animals to take its place?—Yes, and to amend that as may be found to be necessary.

8615. (*Dr. Wilson.*) You say that you cannot trust men in general who experiment on animals?—That is so.

8616. Supposing that all the witnesses who have been experimenting on animals who have given evidence before this Commission had assured the Commission that their experiments, which it is taken for granted by the law shall be painless, have all been carried out painlessly, could you doubt the statement?—If I knew the men to be honest men and perfectly competent and sympathetic observers, I should believe what they say, and I should be very glad of it.

8617. Do you say that with assurance that you would believe them?—If I knew the men; I would believe any man whom I believed to be honest, competent, and sympathetic.

8618. (*Sir William Church.*) Might I ask you one question more arising out of what Dr. Wilson has just asked you. Why do you feel so doubtful of the honesty of experimenters upon animals?—I do not feel any more doubtful about their honesty than I do about that of other classes of human beings.

8619. Is it not the case that the experimenters who are carrying on experiments on living animals in this country are a highly educated class of men?—Yes.

8620. Men who hold honourable positions?—Yes.

8621. And whose conduct is open to observation by numbers of people daily who work in their laboratories?—Is that so? Do they not do a great deal of vivisection very much left to themselves.

8622. It must be done in a licensed laboratory?—That is all right.

8623. Which is open to their students?—Yes, but the students sympathise, as a rule, with that work.

8624. I am not asking about the students; I am only asking about the facts?—I do not know that it makes any difference that they belong to a high order of professional men and that they occupy honourable positions. We find lapses from righteousness in all grades of society.

8625. But apparently you have spoken this afternoon as if you could not trust any of these gentlemen?—Not "any." I never said any. I do not trust, in the present stage of social development, any order of men with doubtful powers. That is so with soldiering, and it is so with all kinds of occupations.

8626. Do you mean that these gentlemen deliberately break the law?—No, I do not. There are very many people who, in the pursuit of money, for instance, get themselves into entanglements.

8627. In the pursuit of money are they working in a licensed place, and probably in the presence of a number of students, or, at all events, of their assistants, who are aware of all that they do?—Yes, and more, because they work in the City of London under the inspection and observation continually of hundreds of clients and fellow-financiers.

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8628. Have you not given evidence to-day on what you think is the case on many points without having made any inquiries as to whether it is so really?—I cannot say that; I do not admit that at all.

TWENTIETH DAY.

Wednesday, 29th May 1907.

PRESENT:

The Right Hon. The Viscount Selby (*Chairman*).

Colonel the Right Hon. A. M. Lockwood, C.V.O., M.P.
Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir J. McFadyean, M.B.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. J. Tomkinson, M.P.
Mr. G. Wilson, LL.D., M.B.
Captain C. Bigham, C.M.G. (*Secretary*).

The Rev. Lionel Smithett Lewis, M.A., called in; and Examined.

8629. (*Chairman.*) You and Miss Woodward, I believe, have been deputed by the Church Anti-Vivisection Society to present their views to this Commission?—That is so.

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8630. And I see that you have sent us in a *précis* of your evidence. You speak entirely on what I may call the ethical question?—Entirely, except in so far, of course, as I give some evidence of the statements I make. I make a statement in the last paragraph, paragraph 13, that the logical outcome of experiments on animals is experiments on man, and that the right to perform such has been claimed and carried out. In making a statement like that, I should put in some facts.

8631. You deny that right?—I deny that right.

8632. That is all; it is only stating the converse view?—My evidence is on the ethical side entirely.

8633. We have had a good deal of evidence on the ethical question, and we all of us feel the importance of hearing it; but the Commission have been considering that matter, and I think it is right that I should say that they think that they have heard it at a length from some witnesses which makes it unnecessary for them to hear at the same length from others; they have given a very free hand to some witnesses as to the length of their evidence, but the points are really short and capable of being shortly put, and if I may say so, you seem to have put all the points (almost all of which I am bound to say have already been put before us very tersely and clearly in your *précis*; and what I propose to do is to take that statement of yours as being a statement of the views of the Church Anti-Vivisection Society. I do not think we ought to call upon you or invite other witnesses to enlarge upon this question, because it really comes to our hearing a certain number of speeches in favour of certain opinions. We want the opinions, we want to know how far those opinions are backed, but we are all of us, I hope, people of some common sense and experience in the world, and when we have heard these propositions stated fully two or three times, it is almost as much as our minds will absorb on the same point, and we do not gather much by repetition. That certain considerable classes of the community agree in those views, and that they should put their views quite shortly before us in support of the general principle, we are quite ready to admit, but many witnesses have given us a *précis* and have followed it by long written statements which I do not think, having regard to the time which should be taken in this Inquiry, we can continue to hear. Therefore, if you will allow me, I will ask you, first of all, what is the Church Anti-Vivisection Society, what is the nature of the body, how many members does it contain?—The total clerical members in the last report were 375, the total lay members 496, making 871 in all.

8634. Do they consist of all classes, or mostly of what we may call the more educated classes?—They consist of all classes, but mostly of the educated classes.

8635. Then, so far as I am personally concerned, but without prejudice to any other members of the Commission who may wish to ask you questions, I would ask you: Do these views which you have stated in the 13 paragraphs of your *précis* express fairly the views of the Church Anti-Vivisection Society?—I believe so. I sent my evidence in to them and they forwarded it; therefore I suppose that they approve of it.

8636. And I need not say that they are views which you yourself hold also?—Most strongly. My statement on the morality of vivisection is as follows: (1) Before we argue as to whether an act pays, we must first consider whether it is right. (2) Power of enjoyment shows the intention of the Creator that the possessor should be happy. (3) Power of enjoyment is the most dominant feature in man and beast. (4) Pain is intended to give warning that continuation of an act will prevent well-being and enjoyment; that is, pain is intended to preserve the *individual*, to promote his ultimate enjoyment. (5) An action involving temporary pain inflicted on another may secure to the sufferer greater physical good than would otherwise accrue. (6) An action involving temporary pain inflicted on another may secure to the sufferer greater moral good than would otherwise accrue. (7) It is immoral (because contrary to the primary intention of pain) to inflict pain unless it is honestly believed that the pain will procure, at least in part, the moral or physical good of the individual sufferer. (8) The right to take life does not imply the right to make living unendurable. (9) To control one's aversion from causing suffering, and to inflict temporary pain on another for the sake of future gain for the sufferer ennobles; to inflict pain on another unless this motive is present is cruel, and hardens and degrades the one inflicting it; to profit knowingly by pain inflicted on another where this motive is absent makes the person benefited a coward. (10) Because we see suffering all around us, to think that we may blindly inflict it is both foolish and impious. It is as if a great surgeon performed an operation for the benefit of the sufferer, and a

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mischievous ape who had witnessed it attempted to imitate his action without his knowledge. (11) The dog or cat, etc., gives up its wild nature, because it has learned to trust man; and this trust is betrayed, and by an act of treachery, when the victim is placed upon the vivisectional table. (12) I have tried to prove that vivisection by its nature is a perversion of the intention of pain (and therefore immoral), cruel, and cowardly, and that, furthermore, it is treacherous. That being so, while I might painlessly sacrifice the lives of many animals to save one valuable human one, I would not have one mouse painfully vivisected to save the greatest of human beings nor the life dearest to me. (13) The logical outcome of experiments on animals is experiments on man. The right to perform such has been claimed and carried out. A race of medical men hardened by vivisection is a public menace. The rights of animals rest on exactly the same foundation as the rights of man—justice and mercy. The temptation to a man who has hardened himself to make experiments on animals which were not for the good of the sufferer, to make experiments on human patients which are not for the good of the sufferer, must be very great. He has blunted his moral sense by inflicting pain in a way which was exactly opposite to the intention of pain. He has broken down the one safeguard which separates humanity from cruelty. Without the knowledge of God he is impiously claiming the power of God to inflict pain where he sees fit.

8637. (*Sir William Church.*) With respect to paragraph 12, have you any special object in choosing the mouse as an animal?—No object whatever; simply to take some very small creature which is not valued very much by most people (most people try to get rid of mice), and it occurred to my mind as an illustration.

8638. (*Chairman.*) To put your case stronger?—That is all.

8639. (*Sir William Church.*) You were not aware that there has been a great deal of experimentation of late upon mice with regard to cancer?—That did not enter into my mind when I wrote those words.

8640. You were not aware, either, that mice are very much used for diagnostic purposes?—No, I was not. That would not make any difference.

8641. As discriminating between different diseases?—No, but that would make no difference.

8642. (*Colonel Lockwood.*) You simply chose the mouse as the humblest representative of life?—I will not say the humblest, but as one that people do not value much.

8643. (*Sir William Church.*) Do you think we are authorised in killing rats and mice at all?—Certainly, when it is necessary; but, in my view, the death should be instantaneous. I think, for instance, the tooth trap, and I think that ordinary trap, the cage trap, are simply barbarous. I think the only trap that should be set is the one that breaks the back at once. As you ask me that question, I may say that I have never had a trap set in my own house, and if people would only cover up their food, which is very much more healthy for them and much more cleanly, they would not need to set mouse traps.

8644. Then, to protect your food, you think you may take proper steps to clear your premises of mice and rats?—As I have said, I think, in my *précis*, I admit the right of killing; but the right of killing does not imply the right of making living miserable. I think I can illustrate it in this way. Rightly or wrongly, at present the laws of this country think that it is permissible to hang a man for murder, and when people consider there has been a gross murder, the papers do not make a great stir about it; but if we read to-morrow morning that a prisoner who committed a murder and was condemned to death had been tortured in his cell, I do not believe there is a single paper that would not take it up; and from that I argue that the power to kill and the power to make living miserable are two very distinct things.

8645. Do you approve of the destruction of rats with the object to prevent the spread of plague?—I hate killing unless it is an absolute necessity. I see no sin, when people are absolutely convinced that plague is spread by rats, in their being killed. I do not say I believe that the evidence is sufficient; as one of the public, who form the jury in those things, I am entitled, of course, to my opinion, whether we have not got a craze for killing everything. I remember quite recently somebody wrote and suggested that all the pigeons in the City should be killed, because they deposited germs. One begins to wonder what you can do for fear of germs.

8646. But might I ask you, do you consider that we have the right to kill rats or other animals to prevent the spread, or, we think, with the object of preventing the spread, of disease in man?—I think that to kill painlessly for that object is no more sinful than to kill for food.

8647. Is it wrong to kill for food?—I think not.

8648. But you think that, although it is right to try and protect ourselves in this way, it is not right to take the life of an animal for the sake of investigating our views as to the production of disease?—I have never objected to an animal being killed for that purpose. I object to its being kept alive in suffering, which, to my mind, is a quite distinct thing.

8649. But might I ask you just to explain what you mean by being "kept alive in suffering"?—Of course, I am not here to give scientific evidence, but I felt that if I was going to give ethical evidence I must be able to give some reasonable cause for holding the opinion that vivisection inflicts pain; and, consequently, I can refer to evidence already given.

8650. I am speaking with regard to these mice. The mice are not used usually for vivisection in the way of demonstrating organs or anything of that sort; they are chiefly used, as I say, now largely in the investigation of cancer, and they are largely used as a means of differentiating disease by means of inoculation?—We have the evidence given before this Commission already, that very great pain sometimes—I can give you chapter and verse, I have it down here—results from inoculations. For instance, Mr. Thane, the inspector, at Question No. 457, says that under Certificate A, inoculations will cause, his words are, "great pain and suffering sometimes, and he gave as an instance the injection of tetanus toxin, infection with plague, and also the insertion of certain drugs; therefore, as I object to animals being kept in suffering, I most strongly object to a mouse being treated in that manner.

8651. (*Sir William Collins.*) In your paragraph 12 you state that while you might painlessly sacrifice the lives of many animals to save one valuable human one, you would not have one mouse painfully vivisected to save the greatest of human beings nor the life dearest to you. You appear there to draw a distinction between painful and painless vivisection?—I want to be very exact in what I say; that is why I put in that word "painfully." If you want to elicit my opinion upon that point, I think that under the present safeguards it is impossible to know what are painful vivisections and what are not; and when we consider what happens in slaughter-houses, which are so much more open than laboratories (and in every case when a man has animals within his power), what things can go on there, certainly in a closed laboratory, I think the risk is enormous of having vivisection carried on at all; but if it were possible to be sure that an animal was completely under anæsthesia, and was destroyed for certain before it recovered, then I should have no more objection than I should to killing an animal for food. But personally I may say this. I would not hand myself over to vivisectors under those circumstances, and, therefore, I would not have any animal handed over to them. I would not trust any body of men of any calling with full power behind closed doors, especially in the case of those who cannot speak for themselves and tell us afterwards what they have suffered. That is my view.

8652. My question merely was: Am I right in thinking that you draw a distinction between painful and painless vivisection?—If it was possible to get painless vivisection I should not disapprove of it, because it is the infliction of suffering to which I object. I want the total abolition of suffering.

8653. I thought your paragraph 12 implied that you considered there was such a thing as painless vivisection?—I have never said that I am satisfied that there is such a thing. I wanted to be very exact, and I wanted just to make quite clear my point, that it is the living in suffering that I object to—that we should inflict it upon them.

8654. Do you doubt that there is painless surgical operation?—Personally I have never known a person who was operated on (I believe the right word is aseptically) but what they have felt pain afterwards.

8655. But do you doubt that under anæsthetics a surgical operation may be performed of considerable magnitude, and the patient subsequently state that he has felt no pain?—Really I am here to answer ethical questions, and that is a scientific question, which I do not think it is quite fair to put to me; I did not come to give scientific evidence.

8656. (*Chairman.*) It is hardly a scientific question to ask you, do you think that if a boy or a grown-up person has a tooth taken out under gas or ether that is a painless operation?—If the questioner in asking me do I believe that there can be complete anæsthesia of human beings, of course I do, the same as any other reasonable person twelve years old would probably admit.

8657. (*Sir William Collins.*) I will not trouble you further on that. I will proceed to your paragraph 13, which deals more particularly with the moral aspect. I see you state that the logical outcome of experiments on animals is experiments on men, and the right to perform such has been claimed and carried out; that a race of medical men hardened by vivisection is a public menace; and that the rights of animals rest on exactly the same foundation as the rights of man. Do you put the rights of animals as regards either destruction or, as the term is frequently used, exploiting them, on the same level as the rights of man?—I did not say that the rights of animals are of the same magnitude as the rights of man. I said that they rested on the same foundation. Two trees of very different size may grow upon the same soil.

8658. Am I right in thinking that by that you mean that there is a difference in degree, although they may be of the same kind?—Certainly. I do not think I have the right to kill my brother man for food, but I think I have a right to kill animals for food.

8659. Then I take it that your opinion is that the rights of animals differ in degree as regards their use by man from the rights of men?—I do think so; at the same time I would just like to read this—they are the words of Jeremy Bentham on the rights of animals: "The day may come when the rest of the animal creation may acquire those rights which never would have been withholden from them but by the hand of tyranny." I am not quite sure of the next word, which was copied for me, but I could not read the person's writing. "The French"—I think it is—"have already discovered that the blackness of the skin is no reason why a human being should be abandoned without redress to the caprice of a tormentor. It may come one day to be recognised that the number of the legs" (again I am not quite sure that is the word; I think it is) "the callosity of the skin, or the termination of the *os sacrum* are reasons equally insufficient for abandoning a sensitive being to the same fate. . . The question is not, can they reason, nor can they talk? but, can they suffer?" That is taken from the works of Jeremy Bentham, published under the superintendence of his executor, John Bowring, volume 1, pages 142-3, in a note. It is on the subject of An Introduction to the Principles of Morals and Legislation.

8660. That quotation would appear to contemplate a stage in the evolution of morals which we have not yet attained to, would it not?—We have not yet attained to that, but we have attained to it very much more than when that was written, I hope and think. For instance, it was practically in the Victorian era that people realised that an animal is entitled to wages for its work, is entitled to food and shelter, and if a man neglects those, the law of the land will deal with that man.

8661. On what moral principle would you differentiate between, as I understand you to say, the legitimate sacrifice of animals for food and possibly also for clothing and for convenience, and yet forbid it for the purpose of advancing knowledge or for an endeavour to improve medical treatment?—The great distinction that I draw is this. We have the right to take life when necessary, but to torture an animal, to use a strong word, or, rather, I prefer to say, to make living miserable, is a perversion of the intention of pain. It is perfectly clear to my mind that pain was given for the preservation of the individual, and I think we can see that in the case of fire. Supposing we had not felt pain from fire, we should have very often run in such danger that we should have been burnt up or seriously maimed before we knew of the danger; but the feeling of pain is a warning to the youngest child that a thing is to be avoided; and those of us who are older and have learnt that lesson, if we suddenly get near a dangerous flame, and get warning from the heat of pain we feel, cease to do that which might have injured us. That seems to me to be the intention of pain. In my point of view every wrong is an excess or perversion of the use of a right power. I think we can see that practically every vice is an excess or perversion of a virtue. The infliction of suffering in a manner contrary to the intention of pain (the preservation of the individual) is a perversion of a power, and therefore a wrong, and nothing can make it right.

8662. I notice that you used with some hesitation the word "torture." Making another quotation from Jeremy Bentham, perhaps it may be regarded as a question-begging epithet. It implies, does it not, needless cruelty or intentional infliction of pain?—In that case, if that is your definition, I should most certainly call vivisection, in every case which inflicted the slightest pain, torture, because it is the perversion, in my opinion, of the intention of pain, which was to preserve the individual; instead of which the experiment is used to inflict pain on the individual, not even in part for its own sake, but absolutely for others. I hold that to be immoral, because I consider it to be a perversion of the intention of pain.

8663. (*Sir John McFadyean.*) In paragraph 7 I understand you to hold that the infliction of all pain on animals is immoral unless it is for the benefit of the individual animal?—In my copy of the *précis* I have inserted before the words "the moral" the words "at least in part." The passage runs:—"It is immoral (because contrary to the primary intention of pain) to inflict pain unless it is honestly believed that the pain will procure, at least in part, the moral or physical good of the individual sufferer."

8664. You are aware, of course, that for the purpose of improving the quality of beef and mutton and pork farmers and others who own animals habitually submit them to a very painful operation, to unsex them. Do you think that is immoral?—I have thought of that question for some time. To begin with, I have not the slightest doubt that in many cases that operation is performed when it need not be, and, knowing what human nature is, there is not the slightest doubt that it is very often cruelly performed; but, of course, a thousand wrongs do not make a right, and I think one of the great values of attacking one wrong is that the people you attack always draw strong attention to another, so that the field of attack opens out enormously. You ask me whether I think it wrong. I have to apply, of course, my principle. My principle is that you have no right to inflict suffering unless it is at least in part for the good of the being upon whom it is inflicted. The first thing I should have to decide is this: is it not in part for the good of the animal that this pain should be inflicted? So far as I know it seems to me that many animals lead a much happier life because that operation has been performed than they would have otherwise. If you can convince me that that is not the case, I should simply oppose it absolutely, because I cannot alter principles to fit facts; facts must be altered to fit principles.

8665. I suppose you do not seriously contend that the intention to benefit the horse or the ox or the pig that is unsexed by that operation has ever entered into the farmer's mind?—It may not have entered into the farmer's mind, but there is someone above the farmer, and that is the public; and there are many things which are permitted, whatever the original inflictor's intention may be, because the public can see that it is not without good to the being upon whom it is inflicted. That would be my answer.

8666. Do you discriminate between the lower animals and man in this connection?—In what way do you mean?

8667. I mean in connection with the operation of unsexing?—It should not be done, of course, except for the person's good.

8668. Do you suggest that it is for the good of either a human being or an animal to be unsexed?—In the case of animals, I am really not an authority upon a point like that. If it violates that principle which I laid down, I should strongly oppose it; but I am of opinion that a great many horses lead happy lives doing useful work, being loved and well treated, which probably would not lead such happy lives if that operation was not performed.

8669. Why should that be?—Because people, I understand, say it is impossible to use them unless that operation is performed.

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8670. I am not speaking about the using: I am coming to that presently; I am speaking about those animals which are kept for food purposes and killed for food purposes?—I do not know how far it is a question whether they could be kept at all in the country fields.

8671. Of course they could?—In that case, certainly, I think it wrong. If your facts are right, then I think the operation wrong.

8672. But supposing it is a mere matter of convenience to the owner; that is to say, that it would be more difficult to keep them in fields and so on, but not impossible, at greater expense, in your view that would not justify these operations?—Not if it causes great suffering.

8673. It is a matter beyond any dispute that many millions of animals are subjected to these painful operations in every twelve months, so that the amount of pain inflicted in this way on the brute creation must far exceed the whole amount of pain that is caused through what is called vivisection. Would it not be better to attack the greater evil first?—Everybody who supports an evil thinks somebody else's evil greater.

8674. But how do you measure the magnitude of an evil which consists in inflicting pain on animals?—You measured it; I did not. You said the greater; I did not.

8675. I did. I am asking how you measure it. You objected to my measurement; how do you measure it?—I attack all cruelty wherever I find it. I am not only an anti-vivisectionist; I attack all cruelty.

8676. Have you ever attacked this particular form of cruelty before?—It has not been brought to my mind. I have thought of it for some time—for about twelve months or so—and I have not got very much knowledge. I may tell you that I spoke to a doctor about it not long ago. I asked him my point. I said, "Is it not sometimes performed for the good of the animal itself?" "Oh, undoubtedly," he said. Like every other form of cruelty, like cruelty in a slaughterhouse and everywhere else, I abominate it. I attack every possible source of cruelty. You speak about attacking it. It is quite right; it would have to be done; we must have no squeamishness; but it is a very difficult subject to tackle in public.

8677. But that need not restrain you?—No, quite so; and every influence I can possibly bring to bear upon everybody, I would on that subject.

8678. So I understand that if your efforts in connection with the question of vivisection were successful you would probably in the next place try to bring about legislation to suppress these practices which farmers carry on?—You greatly mistake my attitude. I never wait until one evil is gone before I attack another. I do everything in my power as I see the ability to do so, to attack all evils at the same time. I prefer not to wait till one evil has gone to attack another.

8679. Of course, if you are going to do that immediately——?—But I am not yet convinced that it is an evil so great as you say. I am in the state of a person who begins very gravely to suspect and to fear.

8680. But you must have misunderstood me. I did not intend to express an opinion such as you ascribe to me. I was not giving an opinion as to whether it is an evil. I do not myself believe it to be an evil. I merely wanted to find out whether you were consistent or not?—I thought you called it the greater evil.

8681. Oh, no?—I thought you asked me why I did not attack the greater evil rather than vivisection.

8682. From your point of view it was an evil; I prefer to describe it as so much pain. I said that these operations involve much more pain than all the operations conducted by vivisectors?—That is a matter of opinion.

8683. You are not of that opinion?—I say that is a matter of opinion. I cannot give an opinion on that.

8684. But if I tell you that there are 10,000,000 animals on which the operation of castration or ovariotomy is performed in full consciousness every year, would you doubt it?—I say that of course I should want to know at what age it was performed; whether the animal suffers really at that age. I should be afraid it is so, and if the animal suffers, certainly it ought to be compulsorily under anæsthetics; and if anyone says that there are not enough anæsthetics to do so, I have not the slightest doubt that when the demand comes the supply will come also.

8685. That was not the question. It was a question of the comparison of the amount of pain that was inflicted by men. I think, in paragraph 9 of your *précis*, you indicate that in your opinion it is cowardly to share in any advantages which may be derived from inflicting pain on the lower animals when it is not intended for their own good. Are you a vegetarian?—I am not.

8686. Then you will hardly deny that you do share in an advantage which is attained by the unsexing of pigs and rams and bulls, unless you habitually ask for bull beef from your butcher?—I do not think you quite understand the words of my *précis*, "To profit knowingly by pain inflicted on another where this motive is absent makes the person benefited a coward." What I there want to bring out is this: I have in my mind such things as this: Supposing I was suffering from diphtheria, there is nothing in the world that would induce me to use or have used on me anti-toxin, because I believe the production of anti-toxin to be a sin. What I mean by being cowardly is this: I look upon vivisection in this way. Every now and then we hear with great pride, coupled with great sorrow, of some disaster, where, when every legitimate means have been taken to save the crew and passengers, when nothing more can be done, the women and children are put into boats and the men go down at their posts. I do not think anyone will say that that is done because the lives of women and children are necessarily of more value than those of men; it is because they are weaker; and therefore when everything lawful has been done the men stand at their posts bravely and go down. Now, some years ago there was a most lamentable incident which happened at a charity bazaar, where the men lost their heads and beat the hands of women and children, making them loose their hold on the balustrade so that they might save themselves.

8687. (*Colonel Lockwood.*) That was in Paris?—It was. That was an instance of panic. Now, vivisection is permitted, to my mind, for two reasons: partly because people fear death, and partly because they fear suffering, and, instead of being like the sailors at their posts when inevitable suffering or death comes, and they have taken all legitimate means—instead of saying "This has been sent to me, and I must bear it with fortitude as a man and as a Christian"—they turn to what I consider unlawful means in panic. That is what I mean by its being cowardly to knowingly use a thing which is the product, absolutely the product, of what is unlawful. But now let us suppose for a minute that a lawful thing is vitiated by the sins of people, what we have to try and do is to get that sin put a stop to. But we need not cease to do a lawful thing. It is lawful to eat meat; it is, therefore, our bounden duty to see that the animals are killed as painlessly as possible. And if your facts are perfectly correct it is also our bounden duty to do our best to see that these animals are operated upon in the way you mention, under anæsthetics, but until that is done, because it is the vitiating of a lawful deed, I do not think we are compelled not to eat meat at all when it is part of our nature to eat meat.

8688. (*Sir John McFadyean.*) I am afraid really you misunderstand my question, which is: Do you consider it your bounden duty to abstain from eating the meat of these castrated animals, because it is undoubtedly better meat, which sells, I suppose, 25 per cent. dearer at the butcher's shop; it is an advantage, that is to say, in purchasing meat for one's diet, not merely from a nutritive point of view, but from the point of taste. Do you consider it your bounden duty to abstain from it?—If the meat itself was absolutely the product, then I should consider it my duty; but there will be meat whether that cruelty, if it be cruelty, is performed or not, and I consider that that makes a very great difference. I say, there is meat. I have animal teeth given me to eat that meat, and I am not going to abstain from eating that meat because somebody else has prepared it in a wrong way. What I will do is to try and get that wrongful preparation of that meat stopped. That is my view.

8689. Are you entirely opposed to field sports in consequence of the pain they inflict upon the lower animals?—No, but I think to take a pleasure in them is simply horrible. I think animals must be killed,

but how anybody can take pleasure in going and killing anything is simply past my understanding.

8690. I was not concerned with that at the moment. I wanted to know whether you think field sports which involve the infliction of pain on animals obviously not intended for their own good, are moral or immoral?—The animals have to be killed.

8691. No, not necessarily; they are bred on purpose to be killed many of them. Will you deal with those cases?—Take, for instance, foxes. It is plain that foxes would have been exterminated had it not been for hunting, therefore, as the fox enjoys its life, hunting has been a benefit to it.

8692. (*Chairman.*) Take pheasant shooting?—In the old-fashioned way, when a man went out with his dog to shoot——

8693. No, take the present time?—Do you mean, do I approve of battues?

8694. I mean pheasant shooting as carried on at the present time?—Personally nothing would induce me to take part in it, any more than I would go to a butcher's shop and slaughter a beast for my food. I know many sportsmen who are the kindest hearted men, they have certain rules and regulations, and they have a keen sense of fair play, but personally the way in which I look at it is this: There are many offices of life that have to be performed. I do not intend to perform them as long as somebody else can be got to do it. For instance, sewers have to be emptied. Not one of us at this table, I suppose, would wish to go into them. I venture to say, without speaking in the slightest degree contemptuously of sportsmen, that personally I look upon sport as one of those occupations I myself cannot understand the pleasure of.

8695. (*Sir John McFadyean.*) What is your defence of the pain inflicted by breeding pheasants and shooting them afterwards?—I do not understand the pain inflicted in breeding them.

8696. I did not say so. I said, in breeding them and shooting them afterwards. I mean that they are expressly bred in order to furnish sport with the gun. Is it your view that it is necessary to have that carried out?—I doubt the statement, to start with, that they are only bred for that. I think that many people like to see them about the place, because they are extremely beautiful.

8697. (*Chairman.*) You might leave out that small percentage and deal with the 95 per cent. where they do it for sport?—May I have the question once again?

8698. (*Sir John McFadyean.*) I understand you to class the shooting of pheasants and the infliction of pain which it involves, with such things as emptying sewers and slaughtering cattle in slaughter houses—that is to say, things necessary, but things you would not like to do yourself. I say, is that so; do you classify it with those things and justify it on the ground that it is necessary?—Never mind motives for breeding pheasants; as long as pheasants are in the world, and are an article of food, it is no more cruel to kill a pheasant by shooting it than it is to kill cattle in an abattoir. Personally, if you gave me my chance as a fox or a pheasant, or as an ox to be killed in a slaughterhouse, I would choose to be a fox or a pheasant.

8699. Is the use of the whip and the spur on horses justified under your code of ethics?—Certainly, because to be unrestrained, and to have one's way completely is bad for everybody. If you look at my *précis*, paragraph 6, I say: "An action involving temporary pain inflicted on another may secure to the sufferer greater moral good than would otherwise accrue." That will cover all punishment.

8700. I am not speaking about the use of the whip and spur as punishment, but as a goad to make the animal go quicker; that is the ordinary use of the whip and spur. Is that represented as being for the animal's good?—A man who uses his whip on an animal that is doing its best is a man who is not inflicting punishment; that is not punishment. If he uses his whip to make the animal go quicker it means that he thinks it can do more; he either does it by simply moving his whip, the mere movement is enough if the animal is willing; but if the animal is lazy, and needs a little punishment, that is punishment.

8701. I ask you to consider the case where an animal is not lazy, and is going at eight miles an hour, and the driver desires him to go at ten miles an hour. That is hardly for the animal's good. I want to know, is it justified?—My answer is simply that if the horse is going as well as it can the man has no right to inflict punishment. If the man wants it to do better, if it is a willing horse he does not have to hurt it with the whip, but if it is an unwilling horse then a little punishment is needed, and if he gives it properly there is no cruelty.

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8702. But there is no horse willing to go its utmost speed?—I do not know about that.

8703. Suppose a horse going eight miles an hour and the owner, for his own convenience, wants to make it go twelve miles an hour, is he justified in using the whip and spur?—I am astonished at your statement that there is no horse willing to go at its utmost speed. There are some horses you can hardly hold in.

8704. I admit the exception—1 per cent., perhaps. Please deal with the average?—I think the proper punishment of animals is lawful.

8705. It is not punishment at all. I ask you to assume the case of a horse that will go without punishment at the rate of eight miles an hour, and the owner, for his convenience, desires it to go at the rate of twelve miles an hour. Is it immoral for him to use the whip and spur?—If going ten or twelve miles an hour will inflict injury upon that horse he is wrong, but it is no more wrong to induce the horse by moving the whip, or a painless flick of the whip, to go faster than to encourage it by word of mouth. A willing horse will go by mere sound.

8706. But always assuming that he has used the whip in such a way as to inflict pain, which is quite a common thing, or that he has used the spur in such a way as to inflict pain—very little pain, I admit, but still much more than is inflicted by some vivisection operations—is that immoral?—The horse is a servant; it is paid wages to do certain work, and if work is not demanded from that horse which it is unable to do there is no cruelty in inducing it to do it. If your means of communicating with that horse which that horse best understands is your whip, you are simply making a justifiable demand upon it; you are not asking it to do too much. There are many horses that you do not have to touch with the whip.

8707. In this case you draw a distinction between man's duty to his fellow-man and man's duty to his fellow-creatures, because you would hardly justify the use of a whip on a paid servant who is not working to the satisfaction of his employer?—Perhaps you will forgive me for pointing out that there are different means of communication. There are three forms of speech: There is articulate speech, there is tone, and there is gesture, and with the animals it is tone and gesture that generally convey our meaning to them, and therefore just as we have practically by tone and gesture to convey it to them, so it may be necessary to convey it by the whip. And, after all said and done, I expect most of us round this table, when we were boys, felt the whip when we were lazy. I do not know that there is any cruelty in that. I should put animals in the same category as children.

8708. (*Chairman.*) We are getting a long way, I think, in these refinements about cruelty—about how far we are entitled to whip a horse or not. You called it punishment just now; but I did not know that the horse had a moral duty, for the breach of which it ought to be punished, from your point of view.

8709. (*Sir Mackenzie Chalmers.*) Have you a list of your Council. Would you hand us in a list?—Here is the sixteenth report (*handing in the same*).

8710. Are you a member of the Council?—No, I am an ordinary member of the Society.

8711. But you were chosen to represent it?—Yes.

8712. Are you a beneficed clergyman?—No; for the last seven years I have been a curate in the East-end of London.

8713. As you know, the Commission is appointed to investigate the operation of the Act of 1876, and to see whether any or what alterations are required. What does your Society suggest in the way of altering the existing law. That is the point we have to consider?—I am here to give moral evidence only. It would be better to ask other members as to that.

8714. But in consequence of the moral evidence that you give do you suggest any legislative changes?—That is not in my *précis*. I was told I was to keep myself absolutely to moral evidence. I prefer to do so.

8715. Somebody else will answer that?—I may just

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say for a minute that the Society itself is pledged to try and obtain the total abolition of vivisection.

8716. That is what I want to know?—I thought you wanted a technical point.

8717. No. You want to abolish vivisection altogether?—That is the aim of the Society.

8718. And by vivisection what do you mean, because the word "vivisection" does not appear in the Act. Do you mean only experiments causing pain, or all experiments on animals?—Personally, I have explained that I want the total abolition of pain.

8719. For instance, feeding experiments and any experiments which might be useful but do not cause real pain you do not object to?—It is very difficult (I am talking for myself, not for the Society) to draw the line apparently, and some of the questions asked in Parliament and correspondence with the Home Office arising therefrom have shown the extreme difficulty there seems to have been in drawing a distinction between some experiments inflicting pain and some not, and, personally, the very thing which a vivisector has called the mere prick of a needle is one of the things I am most opposed to—namely, inoculations. I think the suffering is very great after them. I have already read one statement which I brought here among others.

8720. They may or may not be followed by great pain?—Sir James Russell, the Assistant-Inspector, at Question 543, before this Commission, spoke about a cat, for instance, which suffered "severe pain" after the insertion of Daboya venom. He spoke about pain from drugs. He said that some rabbits suffered "much pain," and then he spoke of some rats as "miserable and dying evidently in suffering."

8721. I asked those questions for this reason: Rule 2 bases the League's action upon the conviction that morality and religion have the right to impose and maintain limits upon research and experiments in human nature. I did not know whether you meant that there ought to be restrictive legislation, as there is at present, or whether you would carry that restriction much further and abolish experiments on animals for the purpose of research?—The object of the Church Anti-Vivisection League is total abolition. I do not believe there is any anti-vivisectionist living who would object to an animal being experimented on and destroyed without any suffering.

8722. If he was sure that anæsthesia was complete?—Yes. But many antivisectionists say what Lord Shaftesbury said—I think I have Lord Shaftesbury's words here, and I should like to quote them; he said, "We are bound in duty, I think, to leap over all limitations and go in for the total abolition of this vile and cruel form of idolatry, for idolatry it is, and like all idolatry, brutal, degrading and deceptive." That is an extract from a letter to Miss Cobbe.

8723. Now, as regards the moral question, I want to ask you this. You are aware that a large body of opinion, opinion of men whom we highly respect on moral grounds, thinks that vivisection is justifiable?—Yes, I am aware of it.

8724. Practically the whole opinion of the medical profession substantially?—We have had some great exceptions, and it is very hard for a man to swim against the prevailing current of his profession; therefore when you say that you have practically got the whole it makes those exceptions you have of very great value.

8725. What I was coming to was this: We have, at rate, in evidence here, conflicting moral views. What do you suggest as the objective test for the Commission to apply. We have had evidence on both sides here from the moral point of view. What is the Commission to take as a test?—What I have tried to lay down; that I consider we can abundantly see that the intention of pain was to preserve the individual, and therefore, unless that partly enters into the infliction of pain, pain ought not to be inflicted. But I say that vivisection is a perversion of the intention of pain, and therefore immoral. Sometimes I have heard people liken vivisection to the Inquisition. I think it does a very great injustice to the Inquisition; because, disastrous as the Inquisition was, they at least had this in their minds, that they believed that the pain they inflicted was for the good of the soul of the person upon whom it was inflicted. No such claim can be made for vivisection.

8726. That hardly answers my question. Is there any objective test by which we, as a Commission, can apply a standard when we have these conflicting views of morality. The vivisectionists, as I will call them, the people who justify experiments on animals, say that the infliction of pain is justified when there is an adequate return for it?—You have said that the greater part of the medical profession is in favour of vivisection. The medical profession are not specialists on the moral question. That is not their province; and I would refer you to the great thinkers of the world, Shakespeare, Browning, Tennyson, Dr. Johnson, Voltaire; in the law, Lord Chief Justice Coleridge, Lord Brampton; and I have with me (I thought, perhaps, it would be better to bring some; I should like to read them, if I may) the opinions of those men and others.

8727. I only want to get the principle?—Exactly.

8728. (*Chairman.*) That hardly arises out of the question. We have had all the opinions of those who have expressed opinions?—My point is this, that on the one hand you have the doctors, who are not, it is not their province to be, specialists in morality; they plead as to the utility or not; it is men who are moralists who plead as to the morality or not; the jury is the public. I want to show that besides those great thinkers I have mentioned, I have the opinions of two Bishops of Durham, Drs. Westcott and Moule, Cardinal Manning, and Bishop Mackarness of Oxford, Bishop Moorhouse of Manchester, Bishop Harold Browne of Winchester, as moralists strongly condemning vivisection, and if I wanted a guide for morality I should go to those. If I wanted a guide for utility I should go to doctors.

8729. I was not objecting to your stating that there were these number of people expressing an opinion; I rather objected that we should hear all their opinions read out?—They are rather strong and important. I do not know if I may read any of them.

8730. (*Sir Mackenzie Chalmers.*) We have practically to recommend legislation. In recommending legislation you say that we ought to consider the opinion of certain people who have studied morals?—Then do not you think I have the right to ask, as I am giving evidence on the question of morality, that you shall hear very short statements by those very great specialists in morality, the bishops.

8731. I want simply to get at the test which we have to apply?—I should be guided best in my morality by moralists, and in my idea of utility, by doctors.

8732. By bishops?—Yes, and the other great men I have mentioned.

8733. I will take that. I only wanted to know what your test was. There are one or two tiny points arising out of your evidence as to which I just want to see how far you go. You said that you would refuse to be treated with antitoxin for diphtheria?—Yes.

8734. And you think it really cowardly to adopt a remedy which might possibly save your life because you think it is wrong?—Some years ago I thought I was going to have diphtheria. I went back and wrote down a request and left it addressed to my father that if I became unconscious he was not under any circumstances to have antitoxin applied.

8735. Now, I want to put this question to you: I suppose most of us have seen children die from diphtheria—helpless children. There is not great courage in letting a helpless child die from diphtheria. Do you think you are justified, to save the life of a helpless child, in using antitoxin or not?—I would not allow my own child to have antitoxin administered to it to save its life. You should not do evil that good may come. The thing is wrong in itself; it would not exist in the world if it had not been for vivisection; it is not the sort of thing that might have been discovered by something else.

8736. That is quite true, as an instance?—Therefore I regard it as absolutely immoral to use it under any circumstances whatever; that is my view.

8737. You would let the child die?—I should.

8738. I only wanted your answer?—May I point out that that opinion was shared by Robert Browning, the poet—he distinctly expressed it—and it is shared by Bishop Moule. Bishop Moule says: "I cordially echo the words used by the late Dean Close, of Carlisle, that he would rather undergo suffering himself than have it inflicted on animals. Even if the suffering of one very dear to me could be alleviated by *the conscious torture* of dumb animals, I think I should have the courage to say that the animal was sacred to its Creator, and that not even to relieve my dear one should the unresisting animal suffer. Having said

this, I think I have gone as far as man can go." Those are the words of Bishop Moule, of Durham.

8739. Although the animal suffers very little, and the child suffers, from what one sees, an intensely painful death?—The question of how far the animal suffered is again a matter of opinion. One has heard dreadful things about animals that die in getting immunised.

8740. Exactly the same principle would apply to vaccination?—Yes; it is not so serious—not so terrible, to my mind, but I am an anti-vaccinator; all anti-vivisectors are not; but I cannot see how anybody can be an anti-vivisector without being an anti-vaccinator—that is logical.

8741. Now, I want to put this case to you, just to test it: We lose in India a great many lives from snake bites, as you know—20,000 lives a year. By experiments on animals lately, as we have heard in evidence, a new method of applying a chemical remedy has been discovered. Are we justified in applying that remedy now, although it was derived from experiments on animals?—Certainly, because it is not the product itself of vivisection.

8742. The experiments themselves were wrong, were they?—The thing itself has been misused, in my opinion. It should have been tried on animals or people who had been already bitten by snakes, by the act of Providence—by accident.

8743. Then take this case, as a matter of right and wrong. Here is a remedy, you do not know its effect; you believe it may be useful. Are you justified, when a man is dying from snake bite, in trying an experiment on him?—If it is for his own good, then it is noble, like the art of the surgeon; if you inflict pain for the good of the person. If you tell him you are running a risk, if you think the man is going to die without—if you say to him "We know of nothing else, would you like this experiment made?"; that is the proper person to make it on. If God sends me suffering it is for me to bear it and take all possible chances of recovery, but not in a panic to go and cause all sorts of wretched creatures to have it inflicted upon them that I may escape a little suffering and death, which must come some day.

8744. Do you think it is justifiable, not knowing whether it would be successful or not, to try it on man, rather than taking a certain number of animals, and finding out that it was a real remedy?—I was talking about the case of a man whose life was in danger.

8745. You have a list of remedies that you may apply. Are you justified in trying a new remedy and rejecting the old ones in order to find out whether the new remedy is a good one? There are several remedies for snake bite; a new one is suggested, are we justified in neglecting the old remedies and trying the new untried one for the first time on a human being?—Certainly not if the doctor thinks there is any chance with the old one; but when the doctor knows there is not, then let him make his trial if he thinks he has a more powerful remedy, if he can get the consent of the person. One of the things about inflicting experiments on animals is that they cannot speak, and do not know that it is being done. If suffering comes to anything naturally, then let us do all we lawfully can to relieve it; but do not let us take the creatures which enjoy their lives before God and inflict horrible diseases upon them.

8746. (*Mr. Tomkinson.*) I understand that you object *in toto* to the principle of vivisection of living animals?—I do.

8747. On the ground that pain must necessarily be inflicted thereby?—As I have said before, I am for the total abolition of pain. I object to making living miserable.

8748. In paragraph 12 of your *précis*, I understand, you say "That being so, while I might painlessly sacrifice the life of many animals to save one valuable human one, I would not have one mouse vivisected to save the greatest of human beings nor the life dearest to me"?—I added in my copy the word "painfully"—"painfully vivisected."

8749. You mean without anæsthetics?—Yes, and without being destroyed before it recovers. I say I am afraid that is impossible.

8750. You appear to have a great disbelief in the total invariable application of anæsthesia to these operations. You can hardly believe that in all cases, especially in operations, the anæsthesia can be complete?—That is a scientific question that I am not qualified to answer; but I think I may say this: that in questions of inoculation where the animals have to be watched for weeks sometimes and many days, of course it is impossible; and those are some of the most cruel of all probably. There are other cases in which we are told that the very administration of anæsthetics vitiates the experiment. And then there is a certificate on purpose to dispense with anæsthetics; and if there is a certificate which is used, we imagine that there are operations performed without anæsthetics; and we have evidence that it is so.

8751. We have had it stated to this Commission that rinderpest, which has been a tremendous scourge in South Africa and in India, has been practically guarded against by experiments upon a very few animals—by inoculating a healthy animal with the blood of an animal that has died of it, and obtaining from the blood of that animal experimented on a serum which has proved an effectual prophylactic against the scourge. Does that commend itself to you?—I do not think we have the slightest right to do that; it is against the principle I have enunciated several times. And I may just add that I happened to be at an anti-vivisection debate the other day, when that very point was mentioned, and there happened to be a South African lady sitting next to me, who challenged, from some statement she had lately had from South Africa, the statement that rinderpest has been combated. We are so accustomed to have all sorts of cures boomed; they all come with a great flourish of trumpets, and then they all pass away, like Koch's cure for consumption, and the rest.

8752. (*Chairman.*) But you would not say that when a physiologist or a well-known veterinary surgeon made a statement of that sort about the effect of antitoxin in South Africa, its value was very seriously deteriorated from because a South African lady contradicted it?—The real value, I should think, was that the process of time is beginning to show that he had made a mistake, if she was correct.

8753. Of course, he may have made a mistake, but it is not much evidence that he did?—I do not say so; as I say, I am not giving scientific evidence.

8754. (*Mr. Tomkinson.*) But surely, you would think it right to inquire how far that is a question of degree; how much suffering was caused to the animal from which the serum was obtained, and whether, by what may have been only inconvenience or a little pain and illness, the lives of thousands of that animal's fellow creatures were saved. You would surely take that into consideration, would you not?—Evidence of that sort, you know, first of all, is always very dubious; but leaving that quite alone, I say, no. You must have some guiding principle in life, and if my principle is right, then that act is wrong. May I just say that I have evidence of some dreadful operations which were performed by Professor Neisser?

8755. (*Chairman.*) That is getting into scientific questions, you know. I thought you rather refused to go into them?—It is a question of morality. He has made a statement on morality.

8756. It is a statement made by some foreign professor?—Professor Metchnikoff, Director of the Pasteur Institute, in his new book, "The New Hygiene"—it is a question of morality—says: "In questions of health morality should not attempt to lead hygiene, but should rather follow her."

8757. I think we have had that quoted to us before somewhere. I rather protest against the opinions of foreign physiologists, who are acting under totally different laws altogether from ours, on the question whether or not we ought to alter our laws. I do not care for Professor Metchnikoff's views about morals; his views on morals will not influence me any more than those of the man in the street?—But Professor Metchnikoff's statement is that morality is to be secondary to hygiene, and when he was over here an English gentleman got up and proposed a vote of thanks to him for his lecture, and yet he had made this quotation in support of the most horrible inoculations of women and children and monkeys with a terrible disease—syphilis.

8758. These are things which could not have been done in England?—But, at any rate, when these foreign men come over, we do not have instances of English

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physiologists repudiating what they do. On the contrary, they meet them with acclamation, and honour them in every possible way.

8759. I do not think we need keep you any longer?—I do not know whether I ought to say this. In my 13th paragraph I say: "The logical outcome of experiments on animals is experiments on man. The right to perform such has been claimed and carried out." I do not know whether I ought to substantiate that. I have got the materials here.

8760. I do not think that quoting a number of opinions of people to the same effect helps us; you are giving your evidence, but not the evidence of all these people?—I am referring to my paragraph 13.

8761. I think we have had that before. Probably the statements which you have are the same statements which we have had of certain physiologists to that effect, that some physiologists have made experiments and have actually carried them out on themselves?—But these are very different; these are experiments carried out in asylums and hospitals.

8762. In England?—In Germany and America, and some in this country.

8763. Were those in this country a breach of the law?—There you ask me a big question. In America, for instance——

8764. I do not think we can go into that?—Not to show the danger? As a clergyman, who has been giving my life up to the poor, I can assure you that I am very much affected indeed in my opposition to vivisection by the danger which besets them. If we are going to have experiments on animals, the time must come when, logically, they must be tried upon men, and I have here serious admissions, such as giving people epileptic fits and different things, which I consider are most certainly most dangerous.

8765. I do not think we can have all these views of foreign gentlemen.

(*Sir William Collins.*) I understood the witness to say that some were in this country.

(*Witness.*) I should like to have given the whole lot—Ringer's Handbook of Therapeutics, for instance, New York edition, pages 585-8, 590, 591, published by Wm. Wood and Co.

8766. (*Chairman.*) That seems to be an American work?—No, I believe—I am not quite sure of this statement—that after the anti-vivisectors made a great stir about it, an expurgated edition was issued, when some of the experiments were objected to because of the outcry, but Dr. Sidney Ringer, Professor of Therapeutics in University College, London, tried the poison with salicine, "Using healthy children for our experiments"; those are his own words. I can read the description, if you wish it.

8767. Were these experiments experiments which were likely to cause any serious illness?—I can read you what was said: "Poisoning with salicine. 'In conjunction with Mr. Bury, I have made some investigations concerning the action of salicine on the human body, using healthy children for our experiments, to whom we gave doses sufficient to produce toxic (poisonous) symptoms. Our first set of experiments were made on a lad of ten. . . . He was admitted with belladonna poisoning, but our observations were not commenced until some days after his complete recovery.'" I want to dwell upon that—"some days after his complete recovery." One would have thought that he ought to have been sent to his parents. Then, "Among the effects recorded during this experiment are 'severe frontal headache, so severe that the lad shut his eyes and buried his head in his arm'; 'became very dull and stupid, lying with his eyes closed'; 'complained of tingling like pins and needles'—and other symptoms indicating severe depression."

8768. Was this on children in a hospital?—You see it is on "healthy children." I believe in a hospital; but that is a point one could look up. The fact that patient I was speaking of as being "admitted" points to it.

8769. You do not know?—No. "In case II. the experiments were made on a lad who had recovered from pneumonia, 'his temperature having become normal ten days previously.'" After he had this drug, the symptoms included "vomiting, dulness, deafness, laboured breathing, spasmodic movements, and quickened respiration and pulse." The report concludes with these words: "We must confess we felt a little relief when the toxic (poisonous) symptoms, which became far more marked than we had expected, abated." That is one example.

8770. (*Colonel Lockwood.*) What is the date of that?—Sidney Ringer's book, about 1883, I should think.

8771. (*Sir William Church.*) You must mention, if you please, that it was the New York edition?—I have mentioned that.

8772. Then I ask what is the date of this New York edition?—That can be ascertained.

8773. But Dr. Sidney Ringer's original work was long ago, before the Act?—Then there are some other cases I can bring forward.

8774. I mean, have these cases been added in the American edition?—I cannot say. I said that I could not be quite sure of that point; I said so before I read it.

8775. (*Chairman.*) The date being 1883, what Sir William Church suggests is that Dr. Ringer, in his original book, gave an account of various experiments which did not include some of these. I understand that the New York contained something more than the English edition.

(*Sir William Church.*) I thought it might be so.

(*Witness.*) What I have gathered some time ago, but how far it is correct I do not know, is that there was a considerable outcry at the time, and the next edition of the book had some of these cases not in it. There is another case which was reported in the "British Medical Journal" of January 7th, 1882, page 5. This was a report of a lecture by Jonathan Hutchinson, F.R.C.S., Senior Surgeon of the London Hospital, Consulting Surgeon to the Moorfields Ophthalmic Hospital, Professor of Surgery and Pathology in the Royal College of Surgeons, and that was the case of a man kept in a shocking state of eruption—to use the words of Jonathan Hutchinson, "he was in a miserable condition from pain and irritation." The man was kept purposely uncured for a few days in order that the students might see that the disease would not cure itself. Then afterwards, to use Jonathan Hutchinson's words, he waved the magician's wand and the man was cured. I say that such a thing is unjustifiable, that a man should be kept in a miserable condition from pain and irritation. The eruption had been out about ten days, and "affected the mucous membrane of his mouth as well as the skin." I am quoting the exact words. "You may remember that we kept him in bed for a few days before we used the magician's wand, in order that all might see that there was no natural tendency to amelioration. More *bullæ* came out." He mentions that the limbs, head, and other most sensitive spots were so covered with *bullæ* that there was nowhere a space as large as the palm free from them.

8776. (*Chairman.*) I do not think we can follow these out at length. We should all agree, I think, that to experiment upon children in a hospital, not with a view to curing them of the complaint that they have, but with a view to experiment upon them for other purposes to a degree that would inflict pain and possibly risk upon them, would be absolutely wrong, and not only wrong in itself, but most prejudicial to the interests of all hospitals, and it would also be a legal offence to do it. I think having stated what you have done is enough to show that if that is an accurate statement in that New York edition of what Dr. Ringer did on that occasion, it was a thing which most people would entirely disapprove of?—I have two very strong cases in England, one, the giving of epileptic fits, which is mentioned without any reprobation in the Gulstonian Lectures by Dr. Gowers.

8777. If these were cases of a breach of the law relating to vivisection the evidence would be most germane, but as it is they only indirectly come in, and therefore, I think, if we have that general statement and one or two instances to illustrate what you mean, that is all you can do?—May I give a case here of a man who after experimenting on cats, the first of which died in 20 minutes and the second in 33 minutes, and other animals, goes on to say, "We have made some observations clinically. To 18 adults, 14 men and 4 women, we ordered 10 grains of the pure nitrite of sodium"?

8778. Who made the experiment, and where?—This again is a report by Dr. Ringer and Dr. Murrill in the "Lancet" of November the 3rd, 1883. After trying it

on the cats, the first of which died in 20 minutes and the second in 33 minutes, they then turned round and tried these experiments on human beings, and here are the most painful results to read of what the people came back and said who suffered from an experiment which was the direct result of vivisection, apparently.

8779. I am afraid that these cases cannot be gone into at this length. I understand that your general principle is that vivisection might lead up to experiments on mankind. We have already had a great many of these cases introduced on statements made in papers, and when we have come to have the actual persons before us who were present to tell us what really took place, the effect of the whole account has given quite a different complexion to the particular passages quoted, and as this is not germane to vivisection, but is only being used as an illustration of what experiments upon animals might lead to, I think we cannot have it in detail, because we cannot have it in such a shape as would involve our having to investigate all these cases ourselves?—Then, my Lord, may I end with this. I have said, and I have tried to establish it, that I am afraid we have grave ground to fear that vivisection on animals will naturally lead to experiments on patients. May I conclude with reading the opinions—it is very short—of important people which support that view, for instance, Lecky.

8780. I really think that reading the opinions of a number of people, or saying I have got a number of opinions or letters written to me by people who agree with me, is a thing that we cannot have. We cannot investigate all their opinions?—But these are public statements in public books. Lecky, the great historian, in his History of European Morals, says "It is abundantly evident"——

8781. I see, Mr. Lewis, you do not take a hint from me. I think we must make an end of these quotations on a subject which is not really before us?—It is very hard to give moral evidence without giving quotations to support it.

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8782. It has little or nothing to do with our Inquiry?—Dr. Johnson warned us that it would lead to this sort of thing, and Lecky and some of the great thinkers of the world tell us that it is one of the great dangers of vivisection that a man who gets callous to pain, and does not inflict it for the good of the sufferer, will not be over particular when he gets straight from the vivisectional laboratory to the bedside of a hospital patient. That is what I have tried to explain.

8783. (*Sir John McFadyean.*) Might I ask one question with regard to this new evidence? You state that the logical outcome of experiments on animals is experiments on man. Is that true, just in the same sense as the logical outcome of running horses in cabs would be to run men in cabs?—Not in the slightest degree. I cannot see the logic of the second proposition. I can see the logic of the first.

8784. It is because I do not see the logic in the first one that I asked you to show me the defect in the logic in the second one?—Is it not plain that the vivisection of animals is performed for the sake of mankind, to cure the diseases of mankind?

8785. That is so?—Very well, then; that being so, sooner or later those experiments must be made upon man. It is very difficult, of course, to argue from an animal to a man; for instance, a pigeon, I believe, can take enough opium to kill about 80 or 90 people; therefore, considering that the time must come when experiments are made on man, those experiments will be made by a man who has deadened his feelings if he has been a cruel vivisector, or has not cared what pain he has inflicted upon the animals, so that he may attain his end; and I may say that the danger to patients, especially to the poorer patients, and to the community, is enormous, when they are in the hands of a man who has deadened himself in that manner.

Miss A. L. Woodward, called in; and Examined.

8786. (*Chairman.*) You also appear as a witness on behalf of the Church Anti-Vivisection League?—That is so.

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8787. Of which you are Secretary, I think?—I am one of the Assistant Secretaries. I am the Acting Secretary, in fact.

8788. You were in the room when I explained to Mr. Lewis that we wished to take the ethical question somewhat more shortly than we have done with some of the previous witnesses, who have gone into it at great length, and perhaps you will bear that in mind?—Yes, I do not want to dwell very much on the ethical side. My evidence is rather a statement of matters of fact, which I have tried to arrange as briefly as I can, so as to take up as little time of the Commission as possible.

8789. You have sent us in a *précis*, I see, and the first thing you desire to give us is a statement showing that illegal vivisection has been practised in England?—The special point that I desire to bring before the Commission is the fact that the absence of convictions is no proof that the law has not been broken, and, further, that the Act has been so administered as to make such convictions a matter of practical impossibility. Thirdly, I would point out that a former witness was mistaken in saying that no complaints have been made of unlicensed vivisection, because such complaints have been made; and, moreover, that, so far from complaints of vivisection having been vague and general, and "nothing that one can lay hold of," as stated by another witness, they have, in some cases at any rate, been most clear and defined and circumstantial. The case which I would especially emphasise in regard to that, which is mentioned in the second paragraph of my *précis*, occurred in connection with a meeting of the Church Anti-Vivisection League in the Technical College at Bradford on September the 30th, 1898. The meeting was a somewhat rowdy one. I am now going to quote from "The Standard" of the next morning, which said that the meeting was "completely taken possession of" by our opponents ("The Standard" is not an anti-vivisection organ, as you know), "who set aside all ordinary rules generally governing the conduct of public gatherings." I am merely giving you an idea of the state of the meeting.

8790. What you are coming to is to show that Dr. Hime, of Bradford, had performed operations without a licence?—Yes, I am coming to that as rapidly as possible. The meeting was packed with medical students, and we had an instance in the Portman Rooms the other day of what that causes. There were also 26 doctors present, a whip having been sent round to over a hundred doctors in Bradford and Leeds. Having given you an idea of the state of things which accounts for what was said, I will suppress my own account of it and pass on to the statements of three persons who were present which were sent in to the Home Office.

8791. And Dr. Hime was present?—Yes.

8792. (*Sir Mackenzie Chalmers.*) Were you present yourself?—Yes. If these statements are not sufficient I can answer any further questions, but I hope they will sufficiently explain the matter. The first statement is by the Reverend Conrad Noel. These were properly attested statements, of which the originals, I suppose, remain at the Home Office. "Dr. Kerr, a Bradford doctor, arguing against the proposal that we should work for the total abolition of vivisection, *i.e.*, of all licences granted to vivisectors, as it would only result in illegal experiments being made, stated that such (illegal unlicensed) experiments were already being performed by his fellow doctors in Bradford. He further said that the reason for these unlicensed experiments being performed was that it was necessary in the cause of science that such experiments should be made, but that Dr. Hime, of Horton Road, Bradford, was the only experimenter who carried on such researches under the necessary licence. He alone held the licence. The Secretary of the Church Anti-Vivisection League handed him the Government returns, which made no mention of any licence held by Dr. Hime; and Dr. Hime, who was himself present, rose, and admitted that it was a mistake for Dr. Kerr to have said that he held a licence. Dr. Hime did not attempt to deny that he carried on experiments. I drew attention to these points in a letter to the 'Bradford Observer,' appearing on the following Tuesday, concluding by saying: 'Surely some explanation should be forthcoming from the doctors!' To the best of my knowledge no answer has been given by them excepting that Dr. Kerr at once wrote me a postcard, marked private, the contents of which I suppose I should not divulge, but which I may at least say does not in the slightest degree contradict his statement as to the illegalities being practised, but rather confirms these statements.—(Signed) Conrad Noel, November 29th, 1898, 279, Tamworth Road, Newcastle-on-Tyne, Curate of St. Philip's, Newcastle-on-Tyne," one of the

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speakers at the meeting. He adds in a postscript: "The above is drawn from notes made at the time compared with newspaper reports of the following morning."

8793. Is there any evidence that Dr. Hime, while he held no licence from the Home Office, did perform operations except the fact that you state that he did not state that he did not do it?—If you would kindly allow me to read the statement of the second witness it will perhaps make it clear to you. If not, I can answer any questions then.

8794. You say the second witness. You are the witness at present. I am asking of your knowledge, have you anything in your knowledge to show that, because if you say that a doctor performed operations contrary to the Act without a licence we ought to have evidence of that?—He made no attempt whatever to deny it. At the meeting I told him the matter would go to the Home Secretary. The second person who makes a statement goes a little further than that.

8795. Was the person present who makes the statement?—Yes. Dr. Hime throughout never attempted to deny it. There was a good deal of correspondence in the Bradford papers, and so on. He had every opportunity of denying it if he was flustered at the time.

8796. The charge was put in quite a different way—that he had no licence. But you say that you have a second statement?—I have it here.

8797. But in order to show that Dr. Hime operated without a licence, if that is what is intended to be proved ——?—I think I have already stated that the second statement is rather more emphatic on that point and will give you a little more information as to what he did say.

8798. But if you will allow me also to put what I have to say, it is that a statement made by somebody to the effect that Dr. Hime operated without a licence is not a thing that we can have except from the witness who makes it?—I am not asking you, who are not like the Home Secretary the judge of this case, whether you think it proved or not that Dr. Hime did commit an illegal vivisection, my point is that complaints of illegal vivisection were brought to the Home Office and disregarded by them; it is the second point. Whether in your own mind you believe Dr. Kerr to have spoken the truth or falsehood is another question altogether.

8799. Nobody suggests falsehood in the sense in which you suggest it, but you are going to make a complaint of the Home Office?—I am coming to that.

8800. Was this statement which you are about to read sent to the Home Office?—Yes. This is from Mrs. Ring: "I was present at the meeting of the Church Anti-Vivisection Society held in Bradford during the Church Congress Week this year, and heard a Bradford doctor (whose name I do not know) state that there were 'hundreds of vivisections done in Bradford'; he also said that Dr. Hime was the only licensed vivisector. Dr. Hime did not contradict this, but said he had 'performed hundreds of vivisections, and should continue to do so.'" That is the point, that Dr. Hime according to this statement distinctly admitted it.

8801. That is the statement of some person which was sent in to the Home Office?—Yes. "Then later on, when the Secretary of the Anti-Vivisection Society produced a list of licensed vivisectors, and asked to be told under what licence he did them, as his name was not down—he called out that it was a 'mistake,' he was not licensed.—(Signed) G. A. M. Ring, Rawmarsh Rectory, Rotherham, November 30th, 1898." And this is the third brief statement.

8802. Was it sent in to the Home Office?—Yes; they were all sent in. "I also was present at the Bradford meeting, and can testify to the accuracy of the above statement.—(Signed) T. P. Ring, Rector of Rawmarsh." Therefore two people, if you lay stress upon that point, speak to the fact of Dr. Hime having said that. My own written statement had been previously sent in.

8803. What happened upon that?—A good deal of correspondence with the Home Office, and this is the final letter of the 11th January, which I will with your permission read.

8804. Thank you?—"Whitehall, 11th January, 1899. Madam,—With reference to your letters of the 19th October and 1st December, 1898, drawing attention to a supposed breach of the Act 39 and 40 Vic., cap. 77, by Dr. Hime, of Bradford, I am directed by the Secretary of State to inform you that he has caused inquiry to be made, but that no case of illegal experiments on living animals has been discovered or brought to his knowledge by Dr. Kerr or by any other person.—I am, Madam, your obedient servant, (Signed) Henry Cunynghame." I venture to say, my Lord, that to require Dr. Kerr himself to come and give this evidence was a very extraordinary excuse to make. It was strange enough that in a moment of excitement he should have betrayed his friend, but even a person of most ordinary intelligence would see that it is preposterously absurd to ask that he should be expected to come in cold blood and repeat that action without any motive moreover for doing so. It seem to me, with all due respect to the Home Office, that it would be very much as if the police should decline to act in a case of alleged burglary until one of the burglars was kind enough to call at the police station and give evidence of the details of what had been committed. However, the matter was not allowed to rest there. I wrote again, and very strongly, to the Home Office, and said that questions would be asked in Parliament. I have here a copy of extract from a proof of the "Parliamentary Debates" of the 2nd March, 1899, which with your permission I will read:—"*Mr. MacNeill* (Donegal, S.): I beg to ask the Secretary of State for the Home Department whether he is aware that at an anti-vivisection meeting held at Bradford on the 30th September last, Dr. Kerr, speaking against the proposed withdrawal of licences granted to vivisectors, said the effect of the proposed step would be to increase the number of illegal experiments on the bodies of living animals, and that such experiments were performed by many of his fellow doctors at Bradford, as it was necessary in the cause of science that such experiments should be made; whether he is aware that Dr. Kerr stated that Dr. Hines"—(it should be Dr. Hime)—"of Horton Road, Bradford, was the only vivisector in Bradford who carried out experiments in vivisection under the licence required by law, and that Dr. Hines, who was present, stated that he had performed hundreds of vivisections and should continue to do so; whether he is aware that the Secretary of the Anti-Vivisection Society produced a list of licensed vivisectors, on which the name of Dr. Hines did not appear; and whether, in view of these facts, he intends to take any, and what, steps for the enforcement of the law against the practice of unlicensed vivisection." Then Mr. Jesse Collings replied: "The Secretary of State for the Home Department believes that at a somewhat excited meeting in Bradford on the date mentioned statements to the effect suggested were made. His attention was drawn to the matter at the time, and he caused inquiry to be made, but could not elicit any clear statement of what was actually alleged, nor any evidence of illegal vivisection being performed by Dr. Hime—not Hines. There are, therefore, no steps which he can take in the matter. I may add that it should be remembered that performing upon an animal an experiment calculated to give pain without a licence is an ordinary criminal offence which any person may prosecute." The important part of that answer appears to me to be the admission that the Secretary of State believed that statements to the effect suggested by Mr. Swift MacNeill were made.

8805. At the meeting?—Yes, at the meeting. Therefore he absolutely disregarded the testimony of four people, two of them clergymen and presumably men of education and accuracy.

8806. Do you know whether the Secretary of State applied to Dr. Hime for an explanation?—I have no certain knowledge. I heard so, but that is not a matter that I could prove.

8807. Do you know whether any other evidence except statements by Dr. Kerr and the statement of Dr. Hime at the meeting were before him?—That I could not say.

8808. At any rate he said that he had investigated the matter and came to that conclusion?—Yes.

8809. (*Colonel Lockwood.*) Did you hear Dr. Hime make these statements?—That is why I wanted to lay stress upon the rowdy character of the meeting. There was a great deal of noise going on, and it was difficult to hear on account of the students with their usual proceedings. I could not swear from my own knowledge that he said that he would continue to perform them. There is the testimony of two people to it that he said he had performed hundreds and would continue to do so.

8810. (*Dr. Wilson.*) Do you know whether any inquiry was made as to whether this was in connection with the production of vaccine lymph?—We had no opportunity

of making a legal inquiry. It is useless to tell you the reports in Bradford. I heard reports of very different things about dogs, but being merely report it is not a thing that I wish to bring before the Commission.

8811. (*Chairman.*) The fact that you desired to bring before us was the conduct of the Home Office?—First of all, the mistake in saying that cases of alleged unlicensed vivisection were not complained of; and, secondly, as to the past administration of the Act. The next case I have is not so flagrant an instance, but it still does prove again that complaints were brought. This has reference to a Lecture on Physiology by Dr. Langdon-Down in University Hall, Gordon Square, on December 9th of the same year. A lady who was present at it wrote to us complaining that he had vivisected a frog (I cannot say that personally) at a meeting in connection with the Child Study Association, a society which had a licensed vivisector for the president of one of its principal branches, and as the hall in Gordon Square was not registered for vivisection we in reply advised this lady to communicate with the Home Office. She did so, and this was the reply received: — "Whitehall, 28th January, 1899.—Madam,—In reply to your letter of the 14th instant drawing attention to demonstrations given by Dr. Langdon-Down in the course of a lecture on 'Physiology and its relation to Child Study and Education,' I am directed by the Secretary of State to say that he has made inquiry and is informed that the demonstrations you refer to were not 'experiments calculated to give pain' within the prohibition of Sec. 6 of the Cruelty to Animals Act, 1876.—I am, Madam, your obedient servant, (Signed) Charles S. Murdoch."

8812. What is your complaint against the Home Office there? Supposing what Mr. Murdoch says there were true, that would be an answer, of course?—I make no complaint against the Home Office there.

8813. (*Sir Mackenzie Chalmers.*) Then what is the point of the evidence?—To show that it is quite a mistake to say that complaints of illegal vivisection were not made. I give a second instance of that.

8814. You mean that a false complaint was made in that case?—No, I do not. I think the letter itself is an admission that a frog had been vivisected there.

8815. (*Chairman.*) A complaint was made about something which, on investigation, the Home Secretary satisfied himself was not a breach of the Act?—He satisfied himself, or, rather, he believed the report of the gentleman, that there had not been sufficient pain caused.

8816. He satisfied himself on such materials as he had before him that there had not been a breach of the law?—If you ask my opinion about the Home Secretary, I would say that he is very easily satisfied, but I do not wish to lay stress upon that. My only point there is that a complaint was made.

8817. (*Colonel Lockwood.*) Two complaints?—Yes. That was with regard to the Child Study Association. There is not the same volume of evidence there, and personally I do not see that the Home Secretary was to blame for not acting upon it; but it proves that Mr. Byrne was mistaken in saying that complaints are not brought.

8818. Could you give us the reference to Mr. Byrne's evidence?—It is at Question 147.

8819. (*Dr. Wilson.*) But are you sure that the frog was vivisected, because the web of a frog is often used to illustrate a lecture without wounding the frog at all?—I am quite sure, from what people said who were there. I was not present myself, as I was on the other occasion.

8820. (*Chairman.*) Your evidence has been addressed to showing that Mr. Byrne's answer to Question 147, in which he says that any alleged breach of the Act has always been brought before the Home Office by the operator himself, is not correct, and that complaints have been made from outsiders?—Yes, exactly; and, furthermore, as our League is only one of many anti-vivisection societies, it is so extremely improbable that every case would be brought before us that I think you may reasonably suppose that these are but typical cases of those occurring from time to time. Just as an instance of that, I may mention the well-known case of unlicensed vivisection at Burroughs and Wellcome's Laboratory at Herne Hill, which is so well known to the public that I do not wish to take up your time by going into it in detail. In that case the Home Office, instead of punishing the breach of the law, gave them a licence afterwards.

8821. Mr. Byrne, of course, clearly made that answer, and those letters which you have read show that it is not quite accurate—at least, they seem to show that; but, of course, they both come to nothing in the opinion of the Home Secretary?—I am afraid that accusations always have hitherto come to nothing in the opinion of the Home Office. That is the unsatisfactory state of the administration of the law.

8822. (*Colonel Lockwood.*) Still complaint was made all the same?—Yes, that is my chief point.

8823. I was going to ask, whether it came to nothing or not, complaint was actually made and lodged with the Home Office and answered by them?—Yes, that is my point.

8824. Therefore, that answer of Mr. Byrne's is incorrect?—With regard to complaints being vague and general, and "nothing one could lay hold of," I have read with surprise that those statements emanated from Professor Thane. I allude especially to his answers to Questions 1284 and 1285, in the latter of which he stated that he could not specify anything as to whether such complaints were general objections as to a matter of principle or to any nuisance. The reason for my surprise is that I am informed that Professor Thane has for years held the position of Professor of Anatomy at University College, and it is very difficult, therefore, to understand how he could have failed to be cognisant of a very extensively signed memorial presented in the autumn of 1903 to the head of the college, Mr. Gregory Foster, by the great majority of female students in the Slade School of Art, as to the very great hindrance to their studies caused by the terrible cries of the vivisected animals. Anyone who knows the dread which any body of young people have of approaching the authorities of their college will understand that things must have been very bad indeed before so marked a step was taken. My special point, however, is that if Professor Thane, the Inspector under the Act, had really wished for information, he had every opportunity of obtaining any amount of information at first hand, because the names of the signatories were all open to him. Our League, as is expressed in its constitution and as one member of the Commission has remarked it to-day, bases its opposition to vivisection solely on moral and religious grounds, and that being the case, we have made a special feature of our warfare against the practice that it is a system of corruption of youth. In connection with that, I may mention that a very short time ago I was interviewing a young man who, at the age of 14, was employed in Professor Victor Horsley's laboratory. He threw up his situation at the end of three months, saying to me that he felt that if he continued it (I am using his exact words, and you must please excuse the English) "he would become one of the most cruel-hearted devils alive." I think if the Commissioners could have heard the cry wrung from that young soul they could not have helped asking themselves whether, even if the claims made for this pseudo science to utility could be made good, the game is worth the candle. Again, in University College a few years ago, the dogs for vivisection were bought at a shilling apiece from the caterer's boy. Now, I hold that it would be an insult to the intelligence of the purchasers to suppose that they did not perfectly well know that such animals must have been stolen. I do not know the present fate of that lad, but if he has become a thief, and if he is practising in his manhood what he learned in his boyhood at University College (and there was such a case, if you will allow me just for one moment to mention a case which is not English, not so long ago in the police courts in America, where the excuse made by the criminal was that he had been taught to steal animals when a boy for a similar laboratory), if that young man has had a similar fate, I say boldly that before God and man the authorities of that college are responsible for his moral ruin and for all the evils brought by the habitual criminal upon the community at large—as responsible as if they themselves had practised the profession of Fagin. I have only one last point to bring forward as to the corruption of youth. I hold in my hand a book which was used by the London School Board for pupil teachers. It took our League a hard fight of six years to get it properly expurgated. The book is by their special science teacher, William Furneaux. He there recommends those young people in a very atrocious ex-

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periment to irritate the exposed nerves of the victim, saying that when they do that at certain points of the experiment the animal will show signs of most acute pain, and in another case he says of intense pain.

8825. (*Chairman.*) Are you quoting that from his writings?—Yes, I have the book here and the page marked.

8826. Will you just state the page?—It is page 189 of "Animal Physiology," by William Furneaux, Fourth Edition, 1892—the special science teacher of the London School Board. I think, my Lord, those words of his form a very curious commentary on the assertions as to the painlessness of vivisection so freely made to this Commission. I have listened, of course, to what has been said this morning, and I venture to think that they supply a very good answer to the insinuation made that we anti-vivisectors ought to turn our special attention towards other forms of cruelty. I think they give us a right to say that beyond all other forms vivisection is a danger to the community and a foe to humanity, and that therefore our very special attention should be directed against it. That is all my evidence in chief. I want, if I am allowed, to add one word about the work of our League on one small point. It has been stated here that the "Shambles of Science" has been withdrawn, and it has been stated that it has not been withdrawn. Our League is in a position to give a little information upon that point.

8827. I think we quite understand the position from Miss Hageby?—I will not keep you one minute. Possibly Miss Lind-af-Hageby did not know this. Every year we have offered that first edition publicly for sale to the public at our stall at the Church Congress, and we should not have done that if we had not been very sure of what the Lord Chief Justice said—that is all.

8828. (*Sir Mackenzie Chalmers.*) I want to ask you about Dr. Hime's experiments. What experiments did he perform?—That I cannot tell you; they were not mentioned.

8829. But really, when you accuse him here of having performed unlicensed experiments, surely you must have inquired what they were?—No, I did not; it was not my business. I brought it before the Home Secretary, whose business I supposed it was. Of course, Dr. Hime might naturally have said that it was a piece of impertinence if I had asked him. He was not bound to give me information; he was bound to give it to the Home Secretary.

8830. Why was he bound to give it to the Home Secretary. He was not a licensee?—Because the Home Secretary is the person whose business it is to ask questions.

8831. I beg your pardon. An unlicensed person who performs experiments is like any other person who commits a criminal offence. Will you show me anywhere in the Act where it is the duty of the Home Secretary?—Then if it is not, it only goes to show how unsatisfactory the present Act is. But the Home Secretary himself did not make that excuse for not acting.

8832. He made inquiry?—Yes, he accepted his responsibility. A good deal of correspondence went on.

8833. He may have tried to satisfy himself and to satisfy you, because, as it turned out, there was nothing in it. But you made no inquiry yourself, and even your society made no inquiry, as to whether any experiments had been performed?—We were not the people to do that, but we did do this, and I think we were about the only people to do it. Mr. Noel and I went before a magistrate the next morning and asked if we could have a search warrant to search Dr. Hime's house. That is provided for in the Act, and we were on the point of getting it until another magistrate came in who was more hostile to us and refused it.

8834. Did you swear an information?—No, because we had not come to that point when the other magistrate came in.

8835. Before you can get a warrant you have to swear an information, have you not?—Yes, we had a copy of the Act with us.

8836. At any rate, you applied for a warrant and the magistrates refused it?—Yes.

8837. Are you aware that if an offence is committed by an unlicensed person anybody can prosecute?—I am aware that that is the theory of the law, but you must perfectly well know that as long as vivisection is performed in secret no private person can prosecute; it is a practical impossibility.

8838. Then how can you say that the Home Secretary is in a better position than any other person?—I do not think it is my business to answer that. I must respectfully ask that the questions are such as are reasonable, and can be replied to. I am not here to answer on a legal point. I am only here to speak to facts. That is a question to be asked of a barrister.

8839. Then I will put it in this way. I am not asking a legal question, I am asking a matter of common sense. If an offence is committed in secret how is the Home Secretary in a better position to find it out than any private person?—He is in a better position to find it out because he is more likely to have his questions answered, but he cannot always find it out either. It is precisely one of the strong arguments against allowing vivisection that it is incapable of regulation.

8840. We are discussing now vivisection by an unlicensed person; that is illegal at present. How would you make it better by making vivisection illegal? Would it not go on in secret?—Some would, but, of course, very much less, because people would be afraid of its being found out, and, moreover, all the corruption of students would be done away with then. I think the system would mostly die out. Take a case that the law does forbid now. The law forbids murder. Unfortunately murder is committed, but if the law legalised murder I think you would find that there would be a great many more murders.

8841. Coming back to Dr. Hime, you say that you do not know what experiments he performed or whether he performed any experiments?—No, I have no knowledge. It is no use to repeat reports that went on in Bradford.

8842. You have told us what was said by the Home Secretary; you have not told us what was said by Dr. Hime himself. You know that he wrote to the Bradford newspapers?—Yes.

8843. Why have you not given us his letter in which he repudiated the statement altogether?—I have the letter here.

8844. Will you kindly read it?—I said to the Chairman that I wanted to be as brief as possible.

8845. In being brief you omitted to read Dr. Hime's letter in which he contradicted all the facts?—I do not admit that he contradicted all the facts. If I read his letter may I read my reply in the Bradford paper? It is only fair.

8846. (*Chairman.*) Is this a letter from Dr. Hime to you?—No, it is a letter to the paper. We both wrote to the paper. This is his letter: "Sir,—I ask your favour to permit me to reply to a statement under the above heading, which appeared in the 'Observer' of March 2nd and again on March 3rd. On March 2nd it was stated that Mr. MacNeill, M.P., was to ask the Home Secretary in the House of Commons, *inter alia*, if he was aware that I had stated at a public meeting that I had 'performed hundreds of vivisections, and should continue to do so,' though I had no licence for experiments. I may say that Mr. MacNeill's question, as printed in the notice paper of the House of Commons, referred to 'Dr. Hine's,' which is transformed in your paper into 'Hine.' If Mr. MacNeill intended the above statement to refer to me, I repudiate it, and deny that there is one particle of truth in it. It seems to me a gross abuse of the privileges of a member of the House of Commons that, while shielding himself under the form of a question to a Minister, a member may make an utterly unfounded charge against a person, who, knowing nothing of his intention, cannot take the necessary steps to have the falsehood killed when and where it is uttered. Mr. MacNeill's proceeding is not one whit justified by his statement being founded on the untrue remarks of a third party. No fair-minded person will approve the conduct of the Minister who, before replying to such a question, involving slander of an absent person, who knows nothing of the liberties being taken with his name in the House of Commons, failed to communicate with the person referred to in the offensive question.—I am, etc., THOS. WHITESIDE HIME." This was my letter:—"Sir,—If the subject were less sad Dr. Hime's assumption of injured innocence in his letter to you would be amusing. He must be aware that newspaper paragraphs are not always absolutely accurate. Has he forgotten that one, if not more, of the Brad-

ford papers announced, on Mrs. Hime's authority, that on the night of the historic meeting he was ill in bed, and had lost his voice, although all of us who were at that meeting can testify to his being in very good voice when he appeared at the head of the rioters? I make no doubt that what Mr. Swift MacNeill really said was that Dr. Kerr had stated that Dr. Hime was performing a great many visisections ('a lot of vivisections' was Dr. Kerr's actual expression), and that when it was proved he held no licence, Dr. Hime made no attempt to deny the vivisections, even when informed that the matter would be brought before the Home Secretary. I do not see myself that Dr. Hime gains much by the publication of the correct version, and, if he will take the advice of an opponent, I should strongly counsel him not to perform illegal vivisections in future. The Home Office has shielded him this time in defiance of a large amount of evidence, the Home Secretary having been supplied with the written statements of four persons present. But should Dr. Hime venture in the future to torture helpless and confiding creatures, it is possible that he has not yet heard the last word as to this matter.—I am, etc., A. L. WOODWARD." The first part shows us, I am afraid, that reliance is not always to be placed on the word of that gentleman—I refer to his saying that he was laid up with a sore throat.

8847. (*Sir Mackenzie Chalmers.*) Did he say so?—The letter, on the authority of Mrs. Hime in one of the Bradford papers, said so. Again, if it was not true it is peculiar that he did not deny these various statements publicly made. Still, that does not affect my main point, which is that complaint was made and that every obstacle was thrown in the way by the Home Office.

8848. Did you produce any evidence of these experiments to the Home Office?—I have told you precisely what we did.

8849. Will you answer my question, please; did you produce any evidence of these experiments to the Home Office?—Beyond what was said at the meeting, certainly not.

8850. (*Chairman.*) I did practically ask that question, and I understood Miss Woodward to say that the evidence produced was the statements made by these three gentlemen about what took place at the meeting. (*To the Witness.*) You did not produce any evidence from Dr. Hime's laboratory to show it?—We tried to get that evidence, but the magistrate would not allow us to go in, you see.

8851. (*Sir Mackenzie Chalmers.*) You were as unsuccessful with the magistrate as you were with the Home Office?—Precisely, as everyone will be while the law remains what it is now.

8852. That was in 1899. Then you referred to another case in 1899; what was the experiment in that case; at any rate, you made an allegation in the case of an experiment at a lecture?—One of the persons present did.

8853. Will you tell me what the experiment was?—No, I cannot tell you the details of it; I was not present there and I am not a physiological person.

8854. Then why do you bring it before us now?—To show the fact that complaints were made from time to time.

8855. Founded or unfounded?—That I leave to the Commissioners to judge for themselves. I am not the jury in this case.

8856. Now we will go on to Dr. Langdon-Down. You say there was a memorial from the students of the Slade School of Art?—Yes.

8857. You have not got a copy of it, have you?—No, I never have had a copy. I have been told of it by several people at first hand.

8858. How far is the art school from the laboratory?—The women's part is very much nearer than the men's, which accounts for its being female students chiefly. I have been told by people who have been there for years that the male students cannot hear the cries of the animals except in one or two rooms, in one of which they study the antique, as my informant called it.

8859. What I want to get at is this. You talk of the cries of vivisected animals; what is your authority for saying that these were the cries of vivisected animals?—It is the students who say they are the cries of the vivisected animals.

8860. What is the use of your coming here to tell us what somebody else says?—Because my point was how very easy it would have been for the Inspector to have information if he had really wished for it. Other people have told me the same. I can mention the names of other people who have also heard those cries.

8861. You heard cries from an animal, but I want to know why you think they were the cries of a vivisected animal?—The sounds came from the laboratory, and they were the cries of animals apparently in terrible pain. No other animals were likely to be in great pain there except vivisected ones, and if I heard horses make a sound in a stable I should naturally infer that they were the sounds of the horses kept in that stable, if I knew that there was a stable there.

8862. You know nothing about it yourself. I will take that?—I never said that I did.

8863. Can you give us any case since 1899?—As I have been asked about University College, if I might mention for one moment quite other work, in interrogating candidates for the Borough Council last November, two of their own accord told me that they were strong anti-vivisectionists, because of the cruelties they witnessed there.

8864. (*Chairman.*) We cannot have that. If these gentlemen connected with the Borough Council think the laboratory is badly conducted they can come and say so?—You can have them before you. I have no doubt they will be willing to come.

8865. We have had many witnesses on both sides?—I say it in answer to the Commissioner, who rather insinuated that there is not sufficient evidence. He can have plenty more at first hand if he wishes it.

(*Sir Mackenzie Chalmers.*) First-hand evidence here is the only evidence that is of use to us.

8866. (*Sir William Collins.*) Did I rightly understand, from the letter that you read of Dr. Hime to the "Bradford Observer," that he complained that he had not been communicated with by the Home Office in regard to what he called an offensive question?—I gather that, certainly.

8867. Then as regards the book that you quoted by Mr. Furneaux, did I correctly understand you to say that some change had been made as the result of a representation?—Yes, twice.

8868. What were those changes?—The first change was simply that he suppressed the words "intense pain" and "most acute pain." We, of course, were not satisfied with that as he described the same operations. We fought it at three elections, and at the last of those, which took place six years after our first fight, a majority of the Board were returned pledged to us, and asked us what we wished done as soon as they were elected. We suggested the entire alteration of that chapter, as with that exception, the book is an excellent manual on physiology.

8869. Did you gather that the passage in that book to which you take exception was an indication or intimation to students to practise that painful experiment, or was it a record of previous experiments which had been made?—I think if you read it yourself you will see that there is a decided suggestion that they should practise it. The Board themselves took it as such, because they first of all, so I was told by a member of the School Board, if I may repeat second-hand evidence, sent for some head teachers and told them they must take care that such experiments were not practised; so that evidently they recognised the danger of its being acted on.

8870. Is that manual still in use?—I am not quite sure.

8871. (*Dr. Gaskell.*) Was that a manual for children?—No, for pupil teachers. There have been some very extraordinary things in manuals for children too, but this was for pupil teachers. There was nothing so bad in the manuals for children.

8872. The animal experimented upon was a frog, I suppose?—I should think it was a monkey. That is rather insinuted, because the fingers were spoken of.

8873. Then am I to understand that your pupil teachers are so rich that they can afford monkeys to practise upon?—Will you allow me to say that I have no pupil teachers.

8874. The pupil teachers belonging to the Child Study Association, is it?—No, it was the pupil teachers of the London School Board.

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8875. I thought you said Mr. William Furneaux?—He was their special science teacher. (*Handing the book to Dr. Gaskell.*) The passage as to the fingers is on the next page. That is in the experiments I described.

8876. Yes. I have looked at it, and I should have thought it was simply a statement of fact as to what happened when you stimulate the posterior root of the nerve, so that it could not have been intended really that they should carry out the experiment?—If we judge by the ordinary rules of English grammar it is the imperative mood that he uses—"irritate."

8877. It is a statement of what happens when you do irritate the posterior roots of the nerve?—Yes.

8878. Is it a recommendation to students to get a monkey and irritate the posterior roots of the nerve of a monkey?—If it is not an injunction to do that, then Mr. Furneaux' grammar is very much out, as he uses the imperative mood.

8879. It is "if"—it is not imperative?—No; pardon me, it is "irritate" in two cases—the imperative is clearly used.

8880. I can only see "if"?—I can show you where the imperative is used.

8881. In the case of Burroughs and Wellcome's do you understand that there is a difference between vivisections which are done for commercial purposes and those which are done for experiment?—I do indeed. I think they are far more dangerous when they are done for commercial purposes.

8882. You speak of Burroughs and Wellcome as being unlicensed vivisectors?—Yes.

8883. Is not that still going on now?—But they have had a licence given them; it was brought by the International Anti-Vivisection Council before the Home Secretary in the year 1900. He then asked the advice of the Colleges of Physicians and Surgeons, and both of them reported unfavourably to granting a licence to Burroughs and Wellcome; but when the doctors happened to be on the other side they were not attended to, and the place has been registered for vivisection ever since.

8884. But for a number of vivisections that are done in commercial schools you do not require a licence; is not that so?—It was held that Burroughs and Wellcome's vivisections did require a licence. I should not think they would be legal without, but there again I am not here to answer legal questions.

8885. You are not aware, apparently, that Dr. Hime is one of those who supply glycerised calf vaccine at the present time?—I was not aware of that fact.

8886. And it is taken up by the Local Government Board and supplied throughout the kingdom, so that Dr. Hime's experiment, this vivisection that you are speaking of, was mainly for the purpose of obtaining calf vaccine—you were not aware of that?—Pardon me, I do not think that he held any such appointment at that time. I have not followed his career very closely of late.

8887. But when he spoke of hundreds of experiments was not that for providing vaccine?—I cannot say what he meant, but I think it very unlikely, as I suppose no licence would be required for that.

8888. (*Sir John McFadyean.*) He never had an appointment; he did it in his private capacity. (*To the witness.*) He might do it in his private capacity, might he not?—That would be vaccination rather.

8889. No; the manufacture of vaccine can be carried on by a private person?—That would hardly be called vivisection, although it is so, we know.

8890. (*Dr. Gaskell.*) When Dr. Hime spoke of the number of vaccine experiments, and said that he was going to continue them, was not that simply in the preparation of calf vaccine?—It would be very peculiar if it were so.

8891. (*Chairman.*) Do you know whether it was so or not?—How can I know? I have told you exactly all I know.

8892. If you do not know anything about it you can say so to the Commissioners?—Exactly, I say so; then I only added in courtesy that I think it is exceedingly unlikely, because vaccination is not what we ordinarily call vivisection. I know it is really, as the animal is now prepared, vivisection; but it is not generally called that. It ought to be, for it is much more painful, I believe, than antitoxin. Antitoxin, though we hold it to be unjustifiable, is not by any means the most painful of experiments. I rather differ from Mr. Lewis there, but the poor calf has its head fixed in a kind of vice in order that it may not, in the irritation of a number of pustules (which they tell me vary from 40 to 200) on the stomach, have the relief of rubbing them off.

8893. (*Sir William Collins.*) Do you happen to know whether Dr. Hime, besides raising stocks of vaccine lymph, also made experiments on calves with a view to seeing whether he could get vaccine lymph by inoculating calves with small-pox?—I know nothing of it.

8894. (*Dr. Wilson.*) In referring to that book of Miss Lind-af-Hageby, you said that you had the first edition on sale?—Yes.

8895. Where?—We have a stall at the Ecclesiastical Art Exhibition of the Church Congress. I have always been very anxious to show that it has not been withdrawn. I refused to return the copies I had, and I have always had it for sale.

8896. You want to test the question whether it is libellous or not?—I want to show that it is not libellous.

8897. (*Chairman.*) Is that a reprint of the first edition?—No, it is the very first edition. When Mr. Bell, the publisher, called all the copies in I refused to send in the copies I had bought. I hold them. I am very precious over them because they are very few, and I am not anxious to part with them, but I am anxious to show the public that they can buy it, and I have it there if they wish for it.

8898. (*Sir John McFadyean.*) Have you actually sold a copy?—Yes, I have sold one.

Mr. G. H. Burford, M.B., called in; and Examined.

Mr. G. H. Burford, M.B.

8899. (*Chairman.*) You come to give us evidence at the request, I believe, of the World League of Opponents of Vivisection?—Yes.

8900. Is that a London Society?—I believe so.

8901. Is London its capital?—That is my impression.

8902. Are you a member of it?—I am not a member.

8903. But you come at their request?—I do.

8904. Do you know anything of the Society—the numbers of it?—I am afraid I cannot give you any information on that point except that I have reason to believe that it is a society of some consequence.

8905. It has a wide-spreading title. I do not know whether its members cover all the world or whether they are mainly in London?—I am unable to give you any explicit information on that point; I know some of its members, and they are, in my judgment, persons of some consequence.

8906. But whether the numbers are 100 or 1,000 or 5,000 you do not know?—It is perfectly immaterial to me.

8907. It may be to you, but to us it may be material, because, as I understand, you come at their request and to represent their views?—That is so. I am not aware of the magnitude of membership.

8908. You have sent a very short note of your evidence, and I understand that the important point that you wish to bring before us is your objection to experiments for purposes of pharmacology and the statement that experimentation might be superseded altogether without any cruelty and with as good effect by following the methods of homeopathy?—That practically is my position.

8909. We are not here, of course, to inquire into the merits of the homeopathic form of treatment, but only as regards this question of experimentation on animals. Would you just tell us what is the method by which the homeopathic pharmacologist would test drugs?—For the purpose of investigating the remedial value of a drug from the homeopathic point of view, the experimenters are always healthy human beings; healthy in the first place in order that the vitiating results of sickness may be eliminated, human in the second place in that the major part of a medical man's work deals with human beings; and conscious human beings in the third place in that the great variety of subjective

symptoms and mental conditions which are unattainable by experimentation on animals may be added to the objective findings.

8910. Are you speaking of these experiments as being carried out on those who are already patients, or purely as experiments on people whom you are not seeking to cure of anything?—Only on volunteer experimenters.

8911. That is to say on persons who are not coming there for the purpose of being cured?—That is so. Investigation on the nature and scope of drug action is carried out solely on what I might call volunteer experimenters. Of course, in many instances these volunteer experimenters are medical men and women, and so are persons competent to observe their own condition. They must be of average health; that point, of course, is carefully gone into. The results, often of course very unpleasant to the experimenter, are recorded in part by the individual him or herself, and in part by the supervisor of the treatment; that is to say, the detail is committed to paper in as thoroughgoing a fashion as possible. The drug for investigation is taken first in small and then in larger and repeated doses, covering oftentimes a long period—weeks, months, sometimes longer than that.

8912. When you say small and then larger doses, we know that drugs in homeopathy are usually given in very minute doses, although I think most of us know that that is not the essential principle of homeopathy, but when you speak of small and larger doses what would you call a large dose; do you mean a dose as large as the extreme dose that could be given by way of experiment in ordinary drugs?—For the purpose of this investigation homeopathy is left entirely out of the question. We are committed only to the investigation of the action of a drug upon the human organisms before us, totally independent of the account to which this investigation may be turned.

8913. Totally independent of the methods of homeopathy in treating a patient?—Yes. In regard to a dose, the dose to begin with is always one which would produce obvious effects upon the organism; there must be something to record; after the patient has recovered from the effect of the first dose, a second is administered, then a third, then a fourth, usually in increasing quantities. The quantities bear no relation whatever to the quantities usually employed by homeopath. Besides the obvious detail produced by the action of the drug to the experimenter, him or herself, and to the ordinary supervisor, these experimenters repair during the course of the investigation to certain skilled observers—specially skilled observers —who each take in hand the various organs and parts of the body for which they are specially competent to speak, now the eye, now the ear, now the nose and throat, now the chest, now the abdominal condition, now the state of matters in the skin, and so forth; so that over and above the generalised disturbance which the drug produces there are also special reports of the special examiners detached for the purpose of special observation on the parts of the economy for which they are most competent to speak. We have, for instance, carried out in this country—that is, our Homeopathic Association—investigation on the drug colchicum in this way, and with your permission I should like to put in some extracts from the observations made on this drug so as to convey some idea of the character of the investigation and its thoroughness (*handing in some papers*).

8914. Was the object of those experiments to ascertain the effect of colchicum on particular diseases?—No, the pure and unsophisticated effect of colchicum given in material doses on the healthy human body as the basis for remedial application.

8915. Without reference to any particular complaint?—Without reference to any particular complaint. It was a search for pure knowledge apart from the special purpose to which the knowledge might be turned.

8916. Perhaps you can give us an explanatory statement of these papers in your own words?—In the course of that experiment there were five adult individuals engaged, each of whom noted day by day the different perturbations in health to which each was not accustomed, and when the experiment was over all their statements were summarised, indexed, and taken as manifestations of the action of the drug on their economy.

8917. How did you measure the effect upon the body; in what way did you ascertain the effect—by the pulse or by the temperature?—By all the methods which are usually employed for the examination of disease in a sick person.

8918. By what you would call clinical observation?—Exactly; observation of the temperature, the pulse, the blood pressure, an exhaustive examination of the blood character itself, and a very exact volumetric estimation of the urinary constituents passed day by day, together with the objective facts observed by the various special examiners to whom the experimenter in question was referred from day to day.

8919. But without any local observation, of course, of the internal organs while it was going on?—Without any local observation of the internal organs, I mean that there was no vivisectional procedure in so far, of course, as the researches of ordinary medical or surgical examination would apply. For instance, the eye was examined instrumentally, the ear was examined instrumentally, and certain other internal organs which could be got at were examined; but the same methods for examining a patient suffering from a drug disease were used as are customary for examination of patients suffering from an idiopathic disease.

8920. Colchicum, of course, is a drug which has been employed for a great many years?—Yes.

8921. It was not a new drug?—No.

8922. Will you tell us what new knowledge you derived from those experiments?—There were various technical points brought out so far as new knowledge is concerned which I am unable to summarise at present, except that in the judgment of myself and others the knowledge gained and acquired of a technical character was worth the experiment. With reference to another experiment of a similar kind carried out in America recently on belladonna, no less than 53 experimenters were engaged over a period of a few years in a similar investigation. In that instance there were no new facts of any magnitude or of new material character brought to light, but the old facts already known, or, at least, the more important ones, were accentuated and emphasised, and that gave us a clear idea that there was very little more to be got out of a new experiment with belladonna even with all the modern instruments of precision. Negative information of that kind we regard as satisfactory, although not as satisfactory, of course, as if new facts had been brought to the light of day.

8923. As the result of those investigations, have colchicum and belladonna now been imported into practical homeopathy at all?—I have here a book containing the belladonna investigation, if I may submit it to your notice. (*Handing in the same.*)

8924. It is too big for reference as we sit here, but we will keep it?—May I go a little further and say with regard to other remedies that many have been introduced into professional use through homeopathic methods which previously were entirely either unknown or practically ignored. Such remedies as nitroglycerine, for example, first came into the ranks of the profession through a homeopathic medium; and snake poisons, the crotalus snake poison, the lachesis snake poison, and the naja snake poison have also been introduced into professional use through homeopathic media, and the action of metallic gold also owes its professional usage to the same origin. The original observations were made on metallic gold by Hahnemann a century ago, and no material addition with regard to the virtues of gold as a remedy has been made since his time. The secretion of the cuttlefish, the virtues of the sepia as a remedial agent came through a homeopathic source. Original observations on bichromate of potash and its value in disease were made by Dr. Drysdale, of Liverpool, some 40 or 50 years ago, long anterior to its present revival; the bean ignatia came into professional use through a homeopathic source. The honey bee, apis mellifica, although investigated of late, had its action first exhaustively determined many years ago in America under homeopathic auspices. Cactus for cardiac affections, first introduced by Dr. Rubini, came into use through the medium of homeopathic experimentation; Rhus Toxicodendron, a favourite remedy nowadays for rheumatism and skin diseases, also owes its introduction to homeopathic auspices.

8925. You are taking these as being homeopathic remedies or drugs which are used generally in what homeopaths call allopathic treatment?—As being used by the general medical profession, but owing their introduction to the methods which I have in-

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dicated as practised exclusively by homeopathy. Calcium sulphide, a common favourite remedy nowadays for furunculosis, and used for this affection for very many years; nitrate of uranium, a common remedy for diabetes, which has found its way into professional use through homeopathic sources; and another remedy, lycopodium, which is used for fireworks or the coating of pills, has on the homeopathic method, that is to say, experiment on a healthy human being, been discovered to have curative virtues of an important and extensive character. This is merely an excerpt of the services which homeopathy has rendered to the profession at large in introducing a large number of new drugs of the virtues of which the general profession were previously not aware.

8926. Has homeopathy adopted the discoveries in drugs made by physiologists or pharmacologists who have followed the experimentation on animals other than man?—Homeopathy finds that experiments on healthy human beings yields results more ample, more direct, more fertile, and more reliable than the details obtained by experiments on animals.

8927. I may perhaps take an example which Dr. Cushny gave. You know Dr. Cushny by name?—I know Dr. Cushny by reputation very well.

8928. He gave us evidence at considerable length of importance as a pharmacologist and as a pharmacologist pursuing his inquiries by means of experiments on animals, and one instance I remember that he gave us was the use of digitalis, which he explained. I am not going to attempt to explain it in medical language at all, but he explained that, whereas digitalis was recommended as a remedy and used as a remedy or as having a particular effect on one particular complaint, it was found that in certain cases it would have a dangerous effect, while it affected other organs beneficially if properly applied. That was discovered by experiments. Supposing you were making an investigation on the operation of digitalis homeopathically, how would you do it?—I should begin by giving digitalis in a substantial dose (we know something of digitalis to start with) to a healthy adult, noticing the disturbances that it created upon his organism, taking his temperature and pulse, and having all his organs, so far as they could be individualised, examined by a corresponding expert; in fact, all the detail that has served the medical man for the diagnosis of a disease would be applied to the diagnosis of the drug conditions induced by digitalis; and the conditions which are produced in animals by digitalis given in this way would find more than their correlatives in a human being when sufficiently big doses of digitalis were taken for purposes of experiment. Then after the man had been made sufficiently ill, when it was obvious that there was nothing more to be got out of the investigation so far as he was concerned, the experiment would be stopped. Probably it would take him some months to get quite well again, and all the various instrumental observations on his pulse, on the beating of his heart, on the varying size of the heart musculature during its operation would form part of the supervisor's report; that is to say, the investigation would be carried out with the same thoroughness and the same detail as an investigation on a corresponding animal.

8929. I do not know whether you have read Dr. Cushny's evidence given here?—I have.

8930. Then you will remember, I daresay, what he said about the ocular demonstration that was given by experiments of the effect of digitalis on the blood vessels in certain cases?—Yes, I do. I can only say—Dr. Cushny is not here, therefore I must speak guardedly—that I am sure Dr. Cushny spoke in ignorance of the very considerable homeopathic literature that has accumulated since Hahnemann's time as the result of the observation of the action of digitalis on the human body; it fills volumes; and so far as the addition of modern methods of precision by instrumental appliances is concerned, it is found, in carrying out these investigations, that all the essential points as to the remedial action of digitalis can be obtained by observation on the healthy human being.

8931. After reading Dr. Cushny's evidence, then you consider that the experiments of which he gave a full description did not add anything to the knowledge which you already possessed. I am reminded that the experiments which he described were not his own experiments?—They added something to the knowledge we already possessed, but nothing of any material importance; they added something in point of academic completeness to this knowledge, but they did not add anything to our knowledge of the remedial virtues of digitalis. The experiments cited by Dr. Cushny placed no additional weapon in our hands so far as digitalis is concerned for the treatment of the disease.

8932. You mean no additional knowledge as to the operation of digitalis in such and such quantities and in such and such diseases?—No other knowledge than was already at our disposal.

8933. I daresay some other gentlemen who are much more competent to ask you questions than I am will ask you questions upon that?—I am sorry that I have been unable to express myself with greater precision.

8934. No, I quite follow what you say. Does that answer which you have given about digitalis apply to other discoveries made by animal experimentation—that it has added nothing to your knowledge for practical purposes?—If I may say nothing material, nothing of importance to our knowledge, that answer in general, with certain exceptions, would apply.

8935. I think that is the gist of your evidence; other gentlemen will ask you questions, I daresay, and elicit more information, but I will not myself ask you any more questions.

8936. (*Mr. Ram.*) Have you yourself ever seen any vivisections?—Yes.

8937. In England or on the Continent?—Both.

8938. (*Chairman.*) I did not ask you the question—are you a practising homeopathist?—Yes.

8939. (*Mr. Ram.*) How long ago is it since you have witnessed any vivisections in England?—During the time that I was a student.

8940. That was before the present Act?—Yes.

8941. Have you witnessed any since the present Act, in England?—No.

8942. Will you kindly tell me the deductions that you draw with regard to the use or abuse of vivisection, from what you have been telling us just now?—With regard to the abuse of vivisection, my deduction is that the major part of the detail derived by vivisectional procedure on animals has still to be verified in its application to man; and that being the case it would be better, in our judgment, to obtain that information at first hand from man himself, and to leave the introductory information from animals on one side, as being sometimes contradictory and occasionally very misleading.

8943. You spoke of verifying the results which have been obtained by examination of animals?—Yes.

8944. Then you think that there has been something obtained, though you may think it may need verification?—Something without doubt has been obtained, and, in some respects, something very considerable, but it is not the best way of determining the remedial powers of drugs.

8945. Have you informed yourself, otherwise than by reading the evidence which has been given before the Commission, of the discoveries which are supposed, at any rate, to have been obtained by experiments on animals?—I am a tolerably diligent reader of the Medical Press, both English and foreign, and I do not think much of moment has escaped my observation which has been recorded of that character.

8946. I may take it then that you have informed yourself of the literature. Do you think that there has been useful knowledge arrived at by experimentation on animals, or not?—I believe that it is impossible to obtain knowledge from vivisection on animals which cannot be turned to some useful account; knowledge is in its nature, of course, a thing which can be utilised; but I maintain as regards drugs that knowledge would be better and more completely obtained in another way. It is a case of relative importance.

8947. You admit then that there has been some useful knowledge obtained by experimentation on animals?—I admit that there has been useful knowledge obtained. No medical man I imagine could take any other view.

8948. You spoke of making experiments on different patients of different drugs?—Not patients; experimenters—healthy people.

8949. I use the term patients in the sense of those who endure what they have to bear?—Exactly; but they were not experiments on sick people.

8950. I take it so. You spoke of trying digitalis, for instance; have you ever had the chance of verifying by post-mortem the conclusion you arrived at by any of these experiments of yours on living people?—If you mean the school to which I belong—yes, many; chiefly from involuntary experiments in cases of chronic poisonings.

8951. Have you found by post-mortem examinations useful knowledge of the effects of the drugs you had been administering?—Useful in a way, but not nearly as useful as that obtained by clinical observation of the experimenters whilst the experiments were being conducted.

8952. You spoke in one case of a man who might be in such a state that it would take him three months to get well again?—Yes.

8953. Was he a volunteer?—Yes.

8954. Conscious of what he was submitting himself to?—Yes, a medical man, and doing it with open eyes. I am pleased to say that hundreds of medical men since the time of Hahnemann have done the same thing.

8955. This man took several months to get well again?—He is not well yet—but that does not count.

8956. Not in your view?—No.

8957. Would you have the right to try an experiment on a man who was not a volunteer?—As an ethical question, in my view of ethics, it would be decidedly immoral.

8958. Do you think that in all cases it is wrong to try an experiment on an animal even if that animal suffers nothing whatever, but is under anæsthetics the whole time and dies under anæsthetics?—That question is rather a large one, and I think the answer would depend entirely upon the specific purpose for which the experiment was conducted.

8959. Supposing a case in which a skilled experimenter believed that he could obtain useful knowledge, either as to the action of a drug or the nature of a disease, and he puts an animal under anæsthetics, and the animal dies under anæsthetics, and suffers nothing, is it right or wrong for the experimenter to try and obtain that knowledge?—Do you wish me to answer an ethical question?

8960. I thought you were here to do so; you have been putting the ethical point of view. If you would rather not answer, I do not press it?—I am perfectly willing to submit my own answer. If the experimenter was a competent person and was convinced in his own mind that he was doing what was right, it might be a right thing to do.

8961. (*Colonel Lockwood.*) Have you any suggestions to make to this Commission as to any additional safeguards which could be provided supposing that vivisection, as it is popularly called, was continued?—I think that prior to the experiments on animals with any new drug, there should be experiments carried out on human beings, not afterwards, but before, and that any point left undecided in the experiments on human beings might then be considered possible (I say, possibly) as a fit and proper subject for experimentation on animals; but I believe that investigation with drugs on healthy human beings should in all cases precede the experimentation on animals.

8962. But you believe that all experiments on human beings should in all cases precede experiments on living animals?—That is as regards drugs on volunteer human beings, of course.

8963. (*Sir William Church.*) Do the principles of Hahnemann still remain as principles in homeopathy?—Yes, that is to say, not all of them, of course, nor all in the condition in which Hahnemann left them; homeopathy, like medicine in general, is a progressive science, but the main principle of homeopathy; that is to say, the law of similars, which is, of course, the centre and pivot of homeopathy, homeopaths hold as scientifically valid.

8964. But, I suppose, it is admitted by them that there has been progress in medicine since?—Distinctly.

8965. In our knowledge of disease?—Distinctly, very much so.

8966. So that some of his main principles you have thrown overboard?—I imagine that blood letting and mercurialisation, and things of that kind, have gone by the board, too.

8967. How about psora?—Psora was a technical term used by Hahnemann for expressing the degree of pathological enlightenment at the time, or which we now consider as diathesis.

8968. He took as his principle what he called an established fact, that there does not exist a single disease which could have a material principle for its cause. That is no longer held by homeopathy?—That has gone by the board as being not in keeping, of course, with modern knowledge; but its retention or its omission affects in no material degree the practice of medicine; it does not affect the usage of the law similia; it is not an essential part of homeopathy. It was one of Hahnemann's views.

8969. Neither was psora an essential part of homeopathy?—Not as a pathological notion.

8970. I do not want to ask you anything further about pharmacology or the use of drugs, but we have advanced beyond the use of mere drugs, or the world thinks we have advanced beyond that. Do you not agree that serum therapy is of use?—Yes, as a private individual I do most emphatically.

8971. Could that have been arrived at without experiments on animals?—It is quite possible that it could have been arrived at in another way, and that the same properties which are utilised in serum therapy might have been used without having recourse to animal experimentation. I will cite one instance of that. The virus of consumption, that is to say the tubercular virus, was prepared by homeopathists and employed by homeopathists for the treatment of consumption long before Koch's discovery of the tubercle bacillus.

8972. I will take your answer?—I believe that what I am saying is capable of verification, and I am quite prepared to fight it out elsewhere if my statement is taken exception to.

8973. (*Dr. Wilson.*) Do you mean that the tubercle bacillus was discovered?—Not the tubercle bacillus, but the virtue of the material in which the tubercle bacillus was contained. The tubercle bacillus was not isolated, but the material in which it is contained, the lung substance, for instance, was prepared by homeopathic methods and used successfully before Koch launched his discovery. I will only quote Von Behring, who will have it that all this is homeopathy, and that Hahnemann's word homeopathy should be employed to indicate it. It is the cure of a disease by a substance which it itself manufactures and as nearly an instance of a similar remedy as could very well be selected.

8974. (*Sir William Church.*) You think then that our present knowledge of such substances as all the antitoxins could have been obtained without the use of animals?—Exactly how antitoxins may best be obtained is still an open question. It is possible that researches in the next 50 years may indicate quite other methods.

8975. I do not want to know anything about 50 years hence; I want to know whether in your opinion the present position of our knowledge with regard to antitoxins could have been attained without experimentation on animals?—Using antitoxin as a remedy which cannot be bettered in any way, of course all the knowledge with regard to antitoxin has been derived through experimentation on animals. I maintain though that antitoxin in its present form is not a final and ultimate preparation. There are other methods of preparing virus which, in my judgment so far as cure is concerned, are equally valid.

8976. Have these other methods been brought into use and generally accepted?—In the case of tuberculin, yes.

8977. We will pass from that?—I take that as a typical instance.

8978. You take tuberculin as a type?—As a remedial method of nature.

8979. Was it attenuated?—It was titurated with sugar of milk in a mortar, and the same dose readily obtained that Professor Wright now advocates—that is, anything from $\frac{4}{1000}$ th to a $\frac{10}{1000}$ th of a millegramme.

8980. Any other?—Homeopathic original research is unfortunately heavily handicapped in this country. We have no school and no well-salaried laboratory, and we have had to be content with our research in tuberculin. I take that as an instance of what might be done with other serums and viruses.

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8981. Apparently you have done a great deal in the way of introducing drugs into medicine. Have you introduced a new soporific, or anything of that kind, which has been of use?—I am afraid I can scarcely answer that question in the way it is put. If you will permit me to frame my answer in this way: The method of drug experimentation on human beings which homeopathy has introduced has very largely cleared away for remedial purposes those particular labels of soporific, diuretic, and so forth, which one was formerly accustomed to hear. This view starts by assuming that a drug has only either one property or one main property, but the results thus gained on animals with any particular drug can be enormously amplified by experiments on a human being, and thus results not obtained by animal experiments can be observed and verified on human beings, which results involve a far wider area of affection than was observed in the case of experiments upon an animal. Therefore we do not label drugs as diuretic, soporific, and so forth, but by what we call their *tout ensemble*, and we endeavour to fit in that *tout ensemble* with the case of any particular disease which may be placed before us. Wherefore homeopathy knows nothing of soporifics, but it knows a good deal of similar conditions.

8982. One of the bases of your work is the study of symptoms, is it not?—The study of symptoms constitutes a part.

8983. Therefore, of course, you do not go in for experimental work on animals?—No; but we go in for the observation of objective changes on the human being at the same time as the symptomology is being evolved.

8984. Are you acquainted with anything that has been done with regard to the neutralising of snake poison?—I cannot say that I have in my memory at present anything that has been done from a homœopathic source.

8985. You know that such a substance as antivenin has been discovered?—Yes.

8986. Do you think it is of any use?—I am really not competent to pass any opinion, because it is a branch of practice in which I do not daily engage.

8987. Perhaps you do not care to answer the question whether you think that could have been obtained by homeopathic methods without the use of experimentation on animals?—I prefer to reserve my judgment on that point, because I am not acquainted with the detail.

8988. (*Chairman.*) Is your society, the World League of Opponents of Vivisection, essentially a homeopathic society?—No, it knows nothing of homeopathy.

8989. (*Sir William Collins.*) Are the views that you put before the Commission this afternoon those of the Homeopathic School of Medicine generally?—Not necessarily. The School of Homeopathy takes divergent views on many questions which I have answered; but you may take my views as expressing the views of the major number of medical men practising homeopathy in Great Britain and America.

8990. And also as the views of the World League of Opponents of Vivisection?—That I cannot say.

8991. I thought you appeared to represent them?—Yes, but I have not informed myself of what the views of each individual member may be; I can only express my own views.

8992. Is the evidence you put before us then to be taken as representing the views of the World League?—So far as regards the desirability of transferring the scene of action of experiments with drugs from animals to man as a general procedure, I believe so.

8993. Do you contest the proposition that useful drugs have been introduced into therapeutics as the direct result of experiments on animals?—I do not contest the proposition, but I say that many more useful drugs have been introduced into general medical practice through the medium of homeopathic experimentation on the adult healthy person.

8994. You think that the method of experimentation on animals should be abandoned in favour of the method that you advocate?—I believe that legitimate knowledge should be legitimately obtained from any source, but, as I said just now, in case of experimentation to be conducted on a new drug for the purpose of discovering its remedial virtues, it should be conducted in the first place on a healthy human person.

8995. But do you take up the view that experiments on animals for a scientific purpose should be restricted or prevented?—I believe, as the result of my observation and experience, that it would be legitimate to very heavily restrict them in one direction—that is to say, that information derived from animal experimentation with drugs could and should be obtained from experimentation on human beings. They ultimately have to come to it, because animal experimentation results have to be verified on the human being as the ultimate issue.

8996. It has been alleged, for instance, before the Commission, that cocaine, adrenalin, sulphonal and other remedies, have been introduced as the direct result of experiments on animals. Do you suggest that those remedies are not useful or that they have not been obtained by that method?—I do not suggest that they are not useful, and I do not suggest that they have not been obtained by that method. I should be very sorry to do anything of the kind. I do suggest that they could, and should have been, obtained through the medium of human experimentation. You get your results then unsophisticated.

8997. How would the preparation of antitoxins be affected by the method which you advocate?—As I say, the opportunities of the Homœopathic School for Research are unfortunately rather limited, but in all probability the preparation of antitoxins would undergo what one might call homœpathising—that is to say, that when homeopaths turn their alternation more extensively to the subject probably some alternative method similar to that used for the preparation of tuberculin could and would be devised; we take that as an indication of what can be done with toxins of this description when they are submitted to homeopathic preparation. So far as diphtheria antitoxin is concerned, I am unable to give you any information with regard to a better preparation after the homeopathic fashion; it has not hitherto been attempted.

8998. If we wanted to get an antitoxin for use to-day we could not get it by the methods that you advocate?—Not as yet to-day.

8999. (*Sir John McFadyean.*) It has been suggested as one of the objections to animal experimentation conducted on healthy animals, that the conclusions based on such experiments cannot legitimately be applied to a diseased condition; do you think there is any force in that objection?—I believe that they cannot be applied by inference to a diseased condition in man, and, of course, it is to the human being that the results of all these experiments are applied.

9000. But putting aside the question of difference of species of animals, do you think there is any force in the contention that conclusions drawn with regard to experimental conditions in healthy subjects cannot properly be applied to diseased states of animals of the same species?—No, I should not hold that.

9001. You think there is no force in that objection?—No.

9002. Your idea of the proper method of investigating a new drug is that it should be tested in increasing doses, beginning, I take it, with what you regard as perfectly safe doses, on human beings?—That is so

9003. After all, that is an experiment, is it not?—That is an experiment, necessarily.

9004. But you think it is moral because the individual consents?—Exactly.

9005. It would be immoral without his consent?—That is my ethical position; it is a personal position, of course. That is what I personally wish here to convey as my own conviction.

9006. You also hold very strongly that results obtained on the lower animals may be misleading when applied to human diseases?—May be and often have been misleading.

9007. Does it not appear to you that you have barred out all method of investigating animal diseases and the action of drugs on animals?—For the purpose of investigating animal diseases it is quite possible that in the interests of the animals another system of ethics might prevail. So far as regards human diseases, I hold that this is the only legitimate method.

9008. It is a very important question. I daresay you can realise that in veterinary practice from the point of view of £ s. d. it is a very important matter?—Exactly.

9009. I want to ask you whether you think that the method of investigating a drug intended for human use which you advocate, might be extended to animals?—No, I should have the same scientific objection to extending observations on human beings to animals.

9010. But you rather misunderstand my question. I understand your position to be that when it is desired to obtain information as to the usefulness of a particular drug on a particular species, it must be tested on healthy individuals of that species?—Yes.

9011. Have you any objection to allowing that general principle to be extended to the investigation of what I may call veterinary drugs?—For a knowledge of the diseases of the lower animals, knowledge obtained from the lower animals is, of course, essential, and is a scientific necessity. That is not unethical; I would not maintain that position for a moment, I think it would be untenable. I do not, as my personal opinion, regard that as immoral.

9012. So that you think that experiments on animals which are intended to increase one's knowledge as to the means of curing and preventing animal diseases, are legitimate?—Much would depend upon the object of the research and on the investigator.

9013. I say that the object is to gain increased power of curing and preventing diseases of that particular animal species?—I do not see any alternative to obtaining valuable information as to the diseases of animals except through the animals themselves.

9014. Then you do think it is legitimate? I think you are bound to make that admission, because you lay down the general principle with regard to the proper line of investigation in man, and you are bound to admit that it is the only proper method of investigating the use of drugs which are intended to be applied to the cure of animals?—That is so.

9015. You have mentioned a number of remedial agents which you said have been introduced through homœopathic sources. I did not quite understand what you meant by introduced through homeopathic sources?—I mean that the knowledge of their properties was first gained by the homeopathic method—that is, experiments on healthy human beings, and then published for the behalf of the profession at large.

9016. But in that respect there is nothing different between homeopathy and other practice. There must be something in the first instance to suggest that a drug might be tried or might be useful?—If a man goes into a laboratory, opens a bottle, and takes a good sniff of some very powerful odorous medicament——

9017. Such as gold?—Such as tri-nitrin.

9018. But please take gold?—What it was that induced Hahnemann in the first instance to take gold I do not know, but I know what induced Professor Albert Robin to take up the investigation of the remedial virtues of gold; it was the results obtained by homeopathic practice——

9019. Such as?—The cure of tertiary syphilis.

9020. The cure of tertiary syphilis with what?—With gold.

9021. Then it was not through homeopathy that gold was introduced as a remedy?—Yes, it was.

9022. What led to its introduction?—That I could not say.

9023. It could not be a knowledge of its efficacy in the cure of syphilis that led to its introduction?—What was in Hahnemann's mind when he took up and investigated the properties of gold I cannot say.

9024. My point was that there did not seem to be anything special about your principle of homeopathy introducing a drug. I can quite see that there may be something special in your method of testing it, but you have not explained anything special in the method of trying a drug or introducing it in that way?—Of course, the same impetus impels the homeopathic experimenter as affects the experimenter on animals; there is a preparation brought before his notice; and he may be and is desirous of testing whether it has remedial virtues or no. What was the motive in Hahnemann's mind in that particular instance with regard to that particular preparation, of course, I cannot say.

9025. You do not suggest that those who believe in homeopathy have any special method of investigating surgical conditions and diseases?—The same method that is open to the profession at large.

9026. It did not appear to me to be quite clear why you were so emphatic in stating that when it is desired to investigate the action of a drug which is intended for use in man the experiments on human beings should precede those on animals? Is there any moral law concerned there?—I did not give that preference on the basis of a moral law, but on the basis of scientific results.

9027. But must there not always be a slight element of danger in using a perfectly unknown drug on a human being?—There is always an element of danger, but after some practice, of course, experimenters become more or less facile.

9028. They cannot be facile with a new drug?—But they can go to work in such a way as experience has indicated, just like explorers in a new country can form some idea of the probable dangers in front of them.

9029. It is not obvious how a man can estimate the probable safe dose of an entirely new drug?—It is obvious that it is a matter of experience. Supposing that a man has a new drug and dilutes it one million times, the probability derived from previous experiments is that a millionth part will not contain a lethal dose.

9030. It all depends upon what was the strength of the material before he began to dilute it?—Supposing he finds a white powder in a paper submitted to him, as being some material which hitherto has not been investigated, he has not the faintest idea whether it is innocuous or lethal. The first thing that he would do would be to take this crude substance and considerably dilute it, let us say one million times, and the records of previous experience with drugs would lead him to understand that in all probability a millionth part would not be a lethal dose. He might therefore with propriety begin his investigations, and then he can heighten the dose according to his judgment.

9031. But, admitting that the danger of subjecting him to too large a dose is slight, why not begin with a smaller dose on an animal; it is not denied, is it, that it forms some guide as to whether the thing will be dangerous or not?—It is denied that it affords some guide as to what is safe for use among human beings. The whole of the animal experimentations as a matter of fact have to be verified in some form or other and at some time or other on the human organisms. Why not, therefore, begin with the human organisms at once?

9032. I have tried to convey that in my opinion you should begin with an animal because it safeguards the human being, and that you do not lose very much time by it?—You do not lose very much time by it, and in the majority of instances you gain little.

9033. You have frankly admitted that the class of so-called protective sera have been discovered entirely by animal experimentation?—Yes.

9034. But you ask us to believe that it is quite possible that they might have been discovered by other means?—Yes, that is my position.

9035. There does not seem to be much force in the suggestion that they might have been discovered by other means. Everybody, yourself included, has had these other means at disposal, and yet you have to admit that nobody discovered anything in the nature of a protective serum until it was discovered by animal experimentation?—If homeopaths had had at their back all the immense resources, the revenues of a province often, which allow these investigations to be carried out, I do not think that homeopathy would have been behindhand in indicating in all probability another and a better way.

9036. But you do not require a very great laboratory, and you do not require a laboratory to be richly endowed in order to do valuable research. Some of the greatest discoveries have been made by people who have had very limited equipment?—But the work with regard to antitoxic sera has been done by people detached from ordinary daily life and the pursuit of their profession for this specific purpose. That is a little expensive. You have to pick your man also; he must have special faculties and ability for that work. Our body in England is not sufficiently big to detach men from daily life and practice for this specific research work, except occasionally.

9037. But you are aware that there is a great deal of money behind this question. If homeopathy can produce efficacious things like the various protective sera, an immense amount of money can be made out of it. Does the fact that they are not invented not tend to diminish the hope that our knowledge in that direction

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will be extended in any other way than by animal experimentation?—I should be sorry to think that the basis of scientific experiment was the love of gold.

9038. I did not mean to suggest that it was.

9039. (*Sir Mackenzie Chalmers.*) You are not yourself a member of the World League of Opponents of Vivisection?—No, I am a member of no anti-vivisectional body.

9040. How did they come to ask you to give evidence on their behalf?—The application came through a friend of mine, a homeopathic colleague, a member of the League. I asked to take this homeopathic point up with regard to the subject, and, as a matter of fact, he requested me to do it myself, and I had no objection.

9041. Practically your evidence is evidence on behalf really of the mass of homeopathic practitioners?—You may put it so.

9042. I want to ask you this: Speaking on behalf of people whose opinion you represent, what changes would you advise in the existing law. We have to consider the Act of 1876 and whether it requires amendment. Will you kindly tell me whether you suggest any amendment?—Are my suggestions to be put from a practical point of view?

9043. Yes, because we have to advise the Government?—The main suggestion that I would make is that when experiments with regard to the remedial action of drugs—I am not dealing with other elements now—are concerned, they should in the first instance be carried out on healthy human beings. That is my point.

9044. Would you have it enacted by legislation that a licence should not be granted for an experiment upon an animal until a corresponding experiment had been made on a healthy human being?—I would leave that to the discretion of the Inspector; I do not think that a hard and fast rule could be laid down in every instance; but I think that discretionary power should be given to a responsible individual in the case of every specific question of this kind being submitted to him, he being fully and entirely responsible for his decision.

9045. The Inspector merely inspects what is going on. I do not understand your suggestion. How would inspectors deal with it?—May I put it in this way: that the advisers of the Home Office should be held responsible in each instance for the vivisectional investigation on the lower animals of the properties of a new drug, and that they should give their specific reasons in each case why the prior investigation on a healthy human being was not adopted.

9046. You agree that that would limit experiments enormously, because there is not a large supply of healthy human beings willing to be experimented upon with unknown drugs?—I think you would find that in the interests of science a sufficient supply of healthy human beings was forthcoming. In America the supply of medical students in the Homeopathic Colleges is a never failing source of adequate material.

9047. You think it is not only justifiable, but right, that in the first instance a drug which may be remedial or may be toxic should be tried on man?—I believe it is not only justifiable but right, if it is carried out under the supervision of a competent experimenter. Anybody can bungle, of course.

9048. (*Chairman.*) Where do you get the volunteers from; are they from among medical students or from people who are paid to undergo it?—Some of them are medical students; some of them are persons who do not require any financial inducement; and some of them require just sufficient financial assistance to recoup them for detachment from the duties which they ordinarily discharge.

9049. (*Sir Mackenzie Chalmers.*) Just to test those principles a little further, take the case not of a new drug but of a specific poison like snake bite; would you say that it is justifiable to try an experiment on the lower animals with a view to testing a remedy for snake bite?—We do not go to work quite in that way. I am sorry to appear unsatisfactory in my replies, but it is not always possible to answer a question in the way in which it is put. In indicating what was useful for snake bite, homeopaths would run all over the details of the drug experiments, many thousands in number, which they had in their possession, and see which of those paralleled the condition of snake bite, and they would put their finger upon the series of experiments already made which most paralleled the condition of snake bite as indicating the remedy; it might not even be necessary to carry out any new investigation. If I may give an instance, when cholera in the thirties swept over Europe, Hahnemann was applied to for effective remedial measures to meet the cholera epidemic. He indicated a satisfactory remedial measure from homeopathy, without additional experiments, from what had been already made.

9050. With what result?—He got, if I may put it, very much better results than the current therapy of his time, and homeopaths afterwards in London repeated them, with such success that they were made the subject of a special Government Inquiry. All this is ancient history.

9051. And are his methods still applied in India where cholera is always present?—Yes.

9052. By whom?—They are mainly applied by those people in the Army with homeopathic leanings, of whom there are some; and also by such educated professional native gentlemen as engage in homeopathy, of whom there are not many; but there are some.

9053. I suppose you admit the utility of vaccination?—Is it my personal opinion that you want?

9054. I only ask it as the basis of a further question?—In the majority of instances I admit the utility of vaccination.

9055. Vaccination has been known for about 100 years?—Yes.

9056. Have homeopaths by their methods discovered any equivalent substitute for vaccination?—Homeopaths hold vaccination to be as striking an instance of homeopathy perhaps as could well be devised; the majority prefer to carry out vaccination in the ordinary way by subcutaneous injection of vaccine matter. Some of us prepare the vaccine matter according to the method that I have indicated for tuberculin—that is to say, by purely homeopathic process—and, it is said, with equally satisfactory results.

9057. (*Chairman.*) And it is swallowed?—Yes.

9058. (*Sir Mackenzie Chalmers.*) As a prophylactic or as treatment?—Both.

9059. May I ask from what it is prepared? I do not quite understand how the method applies in this particular case. Would you take a case treated in this way and tell us what is done?—Let us say, for instance, that an ordinary small child presents him or herself with three or four healthy vaccination vesicles; the material is taken from all these, dried, diluted, usually triturated in a mortar with sugar and milk, and when it has reached a certain degree of attenuation—how far attenuation is made depends upon the ideas of the individual—when it has reached what he considers a satisfactory attenuation, that is taken as the remedial agent.

9060. How is it introduced?—It is swallowed. When you dilute it two or three million times there is no longer objection to its being introduced into the economy through the mouth.

9061. Have you any figures showing how far this vaccination by homeopathic doses which are swallowed has any effect?—As far as vaccination is concerned, I cannot produce figures on that point (although I know that this thing has been done), because a good many of our homœopaths, as I say, still prefer the ordinary and what they consider to be the least troublesome way of introducing the vaccine matter into the arm; they hold that that is just as homœpathic as if it is administered through the mouth, only that a more considerable proportion of the drug is required.

9062. (*Chairman.*) Is it accepted as a substitute for vaccination?—That is a point that rests with the homeopathic public; some do and some do not.

9063. I meant was it accepted as a substitute by the authorities who require vaccination?—No, they want some local evidence of what has transpired, and we can afford them none in that way.

9064. (*Sir John McFadyean.*) You spoke about the necessity of greatly diluting the material from the child's arm. Have homeopaths or any other people ever produced any evidence whatever to show that material taken from a child's arm and given by the mouth to another will do it any harm?—Evidence of that kind is obtainable just as evidence is obtainable of any other remedial action.

9065. It is not remedial; it is an ineffective method. Can you give the Commission any reference to any experiments on human beings showing observable effects produced on human beings, by giving them an undiluted dose by the mouth of vaccine matter?—Do you mean immediate effect?

9066. Or remote; any effect whatever?—No obvious effect.

9067. Can you refer me to any effect at all?—When it has been tried in that way the homeopathic advisers maintain that the patients are immune.

9068. It is not a question of immunity with small doses. I want to know why there is a necessity for diluting it. Is there any evidence to show that it is harmful if you give it undiluted?—No, none.

9069. (*Sir Mackenzie Chalmers.*) Have any experiments been recorded showing any effect from these minute doses?—Any prophylactic effect, yes; that is to say, any protective effect. Many of them in America; I do not carry the detail with me.

9070. Not in England?—Not in England.

9071. There is only one other point that I just want to get your opinion upon. Homeopaths of course study physiology?—Yes, they have to do so.

9072. Do you think that experiments on animals for the purpose of elucidating physiological problems are justifiable or not?—Am I compelled to answer that question?

9073. I ask your opinion, that is all?—As a matter of fact it is still a moot point with myself; I have not made up my mind.

9074. I take your answer. Would you agree that physiological knowledge has been advanced by experiments on animals?—Nobody can deny that.

9075. Whether those experiments were morally justifiable or not is a moot point in your mind?—That is so.

9076. But nevertheless we are bound to avail ourselves of what has been done in the past?—There is no earthly reason why we should not. We still use rubber, although it comes from the Congo. I trust I have made it perfectly clear that homeopaths at least hold that their method of obtaining information with regard to the remedial virtues of a drug—that is to say, by experiments on healthy human beings—is in their opinion of immeasurably greater value than the alternative method of experimentation on animals.

9077. That I understand to be your opinion. May I take it, then, that your evidence to-day is really confined to that aspect of what we may call vivisection which relates to the action of drugs—pharmacology?—Yes, that is so.

9078. And whatever opinion your Society may hold, you are not here to go into other questions?—I speak for myself.

9079. Among homeopaths you have surgeons as well as physicians, of course?—Yes.

9080. Is it ever justifiable, do you think, before trying a new operation on a human being, to try it on an animal under anæsthetics, or would you try it first on the human being?—I should imagine that most competent surgeons (and I think that no one but a competent surgeon—that is to say, a thoroughly skilled, experienced person—would do it) would answer your question that most decidedly it would be better to try the experiment on a human being and not on an animal. I may say that I have myself some experience in surgery, and that personally would be my decided conviction. A man knows exactly what are the necessary limitations when he operates on the human body. He has some preliminary knowledge to tell him whether his proposed experiment is in flat defiance of what nature can bear or not. An experienced person would know exactly the tax that he proposed to levy on the economy before he started, which only experience could afford; and if he was simply skilled in operating on animals he would not know the exact tax that he proposed to levy on the human economy. He would be more likely to get a satisfactory verifiable result by his human procedure than by his animal procedure.

9081. I was thinking of this sort of thing. You have from time to time new kinds of sutures; you substitute, say, a catgut suture for a metallic suture. Is it justifiable to try that first on a human being, or would you try it first on an animal?—One of the most distinguished surgeons of the day, Professor McEwen, of Glasgow, had to deal with the same subject and answer that question. He made no animal experiments.

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9082. I am asking you, do you think he was right?—Certainly.

9083. It is better to try at first on the human being?—Most decidedly. When you have dealt with animals you have to deal afterwards with human beings, and thus you have to deal with different tissues; and after you have produced animal results you have to produce human results, to see whether those results are verified. It is better to go to the human being at once.

9084. I would like to get your opinion on this point. When you find a new surgical method successful with animals, does it not go a long way towards justifying you in trying it on human beings?—Yes, it would; but I hold that the experience and *naus* possessed by a competent operator would render unnecessary the preliminary procedure on an animal.

9085. (*Dr. Gaskell.*) I was wondering whether, in your method of investigating the action of drugs, you would say that you get accurate knowledge of the action of drugs on different organs or on the patient as a whole?—Both on the patient as a whole, and then in cases of poisoning, acute or chronic, by the *post-mortem* investigations that follow. We accept those *post-mortems*, of course, and what they disclose, as part and parcel of the investigation. Investigation such as this may cover many countries and many years.

9086. Do not you feel sometimes rather doubtful as to whether it is one organ or another that is chiefly affected by the drug?—What you get in the case of poisoning by a drug is exactly parallel to what you get in a condition of disease wherein it is very often impossible until the patient is dead to determine the precise local seat of the mischief.

9087. Is it not better, both in that case and in the case of disease, that subsequent experiments should be made upon animals to see whether that drug does act especially upon one organ rather than another?—That has been done so often, and so little practical information has been obtained—that is to say, the additional power which it has given with regard to the selection of remedies has been so insignificant—that it is no longer held among homeopaths as affording any material additional information of consequence.

9088. What I was mentioning rather was that in physiological experimentation we have the power of analysis which is deficient at the bedside; you can limit your experiments to one particular organ or your drug to one particular organ?—Precisely.

9089. And so you can gain information without complication from other organs; do you not consider that an advantage?—Not much. You often find in cases of disease, particularly a chronic disease, that you do not have to treat one organ solely and wholly. It is the organ in connection with the environment in which it finds itself that the homeopath calls the *tout ensemble*, the totality of symptoms and conditions which has to be treated, and not the simple outburst of one particular organ.

9090. Do not you think that, whereas clinical investigation has been present to the minds of medical men for generations, a great advance has been made of recent years; do not you think that within the last 30 or 40 years knowledge has been very much more quickly obtained than before?—Knowledge has been much more quickly obtained because the armamentarium which advance in science brings with it has been much more copious than before.

9091. Has it not been coincident with the method of experimentation?—It very often grew up side by side with it.

9092. You consider that the experimental investigation has not really largely assisted in elucidating the slow methods of nature's experiments?—Experimental methods on animals, in my judgment, have not added any material information to that afforded by similar experiments carried out on the human subject when they have been taken as the alternative; and when no similar experiments have been carried out on the human subject often nature's experiments were sufficient in number and demonstration.

9093. Would you say that of our knowledge of the tracks and functions of the central nervous system? Have we not gained an enormous amount of knowledge of late years from experiments on animals in that direction?—Physiological and pathological

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knowledge, yes. What is the outcome of it with regard to treatment?

9094. I was asking you about our knowledge; you can get a certain amount of knowledge of disease from the hospital bedside?—Yes.

9095. Have we not been able to understand those diseases much more in consequence of our more accurate knowledge of the tracks and functions of the nervous system, owing to experiments on animals?—Without doubt, as far as academic knowledge is concerned, decidedly; no medical student even could take an opposite view.

9096. Has not that enabled the surgeon to remove tumours when otherwise he would not have known where they were?—He does not always distinguish himself in that direction. The results of brain and spinal surgery, based on vivisectional experiments, are not to be compared with the results of abdominal surgery.

9097. Based also on vivisectional experiments?—No, the most skilful abdominal surgeon of the Victorian era, Lawson Tait, publicly foreswore any alliance or allegiance that he had to knowledge vivisectionally obtained. And the first ovariotomy done in England was not done on an animal, but on a human being.

9098. I understand, then, on the whole, that although you do not think that experiments on animals have advanced our knowledge in a useful direction very largely, still you do think that they have been useful to a certain extent?—They have advanced, without doubt, our diagnostic knowledge.

9099. Therefore, you would not be inclined entirely to abolish such experimentation?—I have already said that, on the whole, what I am inclined to is a considerable restriction of vivisectional investigations.

9100. (*Dr. Wilson.*) Reference has been made to experimental work in physiology. Would you be in favour of the illustration of physiological lectures by experimentation on animals?—I am afraid that is a moot point, upon which I have not made up my mind. I have had no actual practical experience of physiological teaching, and I should be sorry to commit myself definitely on that problem.

9101. (*Chairman.*) There is just one question I should like to ask you. You said that if the homœopaths had had laboratories and sufficient funds for carrying on investigations, they might have made as many discoveries as have been made in other laboratories where experiments on animals are conducted. What would be the nature of the experiments, on the principles that you have spoken of, in such a laboratory?—Experiments on human beings and the physical examination, so far as practicable, of the various system and organs of the human beings while the experiments are going on.

9102. You would not want laboratories for that, would you?—You want a laboratory for examining the blood, the urine, the perspirations, excreta, and so on.

9103. That is a very simple thing; that would be more in the nature of a consulting room?—No, the blood examinations made nowadays are most complicated and expensive businesses.

9104. I did not understand what you meant by saying that laboratories were required for your homeopathic investigations; but I see now what you mean?—They would have to be as extensive and as well equipped as those in use in the present day.

TWENTY-FIRST DAY.

Wednesday, 5th June 1907.

PRESENT:

The Right Hon. The Viscount Selby (*Chairman*).

Colonel the Right Hon. A. M. Lockwood, C.V.O., M.P.
Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. J. Tomkinson, M.P.
Mr. G. Wilson, LL.D., M.D.

Captain C. Bigham, C.M.G. (*Secretary*).

Miss L. Lind-af-Hageby, recalled; and further Examined.

Miss L. Lind-af-Hageby

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9105. (*Sir Mackenzie Chalmers.*) I want to ask you a few questions, please. I think you described yourself, in answer to one question, as a lecturer and writer?—Yes.

9106. Do you lecture and write on any subject besides vivisection?—I have written and lectured on a great many subjects—social, educational, and literary subjects. My articles have generally appeared in the Swedish press and magazines.

9107. In England are you working chiefly for anti-vivisection?—For the last five years I have exclusively worked for anti-vivisection.

9108. For the last five years you have given yourself up to it?—Yes, for seven years I have been most keenly interested in it, but for the last five years I have given myself exclusively to it.

9109. As I understand, you do that as a labour of love and without remuneration?—Yes.

9110. Who publishes your books, are they done by yourself or a society?—If you mean my books on vivisection, I have only written one book, "The Shambles of Science."

9111. Only that one?—Only that one, together with Miss Schartau; it was first published by Mr. Ernest Bell. After the trial, when Mr. Bell withdrew it, it was brought out by Miss Schartau and myself.

9112. As publishers?—As publishers.

9113. Not by any society?—No.

9114. Have you seen any experiments, except lecture experiments? Have you seen any research experiments?—No, I have not seen any research experiments, except, perhaps, after demonstrations before students. Sometimes it happened that one or two physiologists remained, and that some special inquiry was settled by one or two experiments.

9115. But everything you have seen yourself has been in connection with physiological lectures?—With physiological demonstrations. I have visited some physiological laboratories, and seen the effects of research experiments, if I may put it in that way.

9116. You have seen the animals afterwards?—Yes.

9117. Have you ever found any of those animals suffering?—At the Pasteur Institute I have found animals suffering.

9118. But there they are experimenting on rabies and other matters?—They carry on very many experiments of all kinds.

9119. I was asking about England. Have you visited any physiological laboratories in England, and seen the animals afterwards?—I have visited the Brown Institute, where I saw a number of fox terriers intended

for experiment, and I was not very pleased with the way they were kept. I did not consider it very humane. But I have not seen any animals after they have been used for private research.

9120. May I ask how many actual experiments you have seen in England?—We considered in 1903, when we counted over 100 lectures that we had attended, that we had seen demonstrations at about 50.

9121. Where were those lectures, please?—Some lectures were at the Imperial Institute, among them a course of lectures by Dr. Pembrey on animal heat. At the same Institute another course of lectures on the sources of animal energy by Professor Starling; on the circulation by Dr. Brodie; and on the chemistry of muscle and nerve by Professor Halliburton; on the action of anæsthetics and narcotics by Dr. A. D. Waller. Those at University College were on the mechanism of the secretory process by Dr. Bayliss; those at King's College on pancreatic secretion by Professor Starling; and on co-ordination by Dr. F. W. Mott. Then we attended all Dr. Brodie's lectures at the London School of Medicine for Women.

9122. You have kindly given us the names of the demonstrators?—I have given them now.

9123. The only one, I think, whom we have had before us is Professor Starling. Have you read his evidence?—Yes, I have.

9124. Do you agree with it?—I am afraid I do not.

9125. At the time you attended these lectures, may I ask whether you went simply to see what would happen, or whether you went having already made up your mind that vivisection was wrong and cruel?—I, of course, had made up my mind, as I think I explained to Lord Selby, in answer to one of the first questions put to me; but at the same time, I was honestly open to conviction—both Miss Schartau and I were. We started attending these lectures with a sort of idea that anti-vivisectors had exaggerated, and we did not start attending these lectures with any fixed idea to write a book or to come out with a public accusation.

9126. That is what I wanted to know, rather?—No, not at all; and, if we had, perhaps sometimes we should have observed details more than we did. I have noticed in reading through my notes, how clear it is that there was no such intention at all at first. It was only after considerable experience that we felt we would like to write about it.

9127. May I put it in this way. If you had not found what you thought was a certain amount of horrors, would you have been disappointed?—No, we should have been very glad.

9128. In the case of all these experiments the animal according to law, had to be anæsthetised?—Yes.

9129. And it had to be killed before it recovered?—Yes.

9130. In your opinion in how many cases was the anæsthetic insufficient?—That question could best be answered by a reference to the "Shambles of Science." I could not at this moment say how many experiments are described there, but I have already pointed out that there are two which describe the efforts to produce complete anæsthesia, especially "A Grand Demonstration," described at the end of the book.

9131. We will come to that presently; but in two cases specially you thought that the animal was not properly anæsthetised?—I should not say that. In two cases we thought efforts were made to produce complete anæsthesia, and those cases are recorded faithfully in the book. In the majority of cases we did not get that impression, and that is also recorded in the "Shambles of Science."

9132. Have you made any independent study on anæsthetics?—I had previously attended Dr. Waller's course of lectures on the action of anæsthetics and narcotics in order to get efficiency on that subject. I have just heard—I heard it before the Commission the last time—that he is considered a great authority on the subject. I have also attended operations on human beings and seen their administration.

9133. You have seen many operations on human beings?—Yes. I have also had chloroform myself and other anæsthetics.

9134. Perhaps I may ask you when you had chloroform yourself were you conscious?—No, I was not.

9135. You felt no pain?—No, I felt no pain.

9136. Have you seen the evidence of Mr. Morris, the President of the Royal College of Surgeons?—I have read it through, but rather carelessly.

9137. I merely wanted to ask you generally. What he told us was that when an anæsthetic is given, the power of feeling pain disappears before the power of motion; and he was talking of a long experience as a surgeon on human beings. Do you agree with that?—I do not think that is always so. I think there are cases, and I have here an extract from Professor Flint, who is an authority, to that effect in his "Handbook of Physiology," showing that a patient may actually feel the operation under light anæsthesia, and yet at the time of regaining consciousness perfectly may forget that he has felt the pain. He says, "Voluntary movements and cries are observed in persons subjected to painful surgical operations, when incompletely under the influence of an anæsthetic, concerning the character of which there can be no doubt. The movements are voluntary, and the cries are evidence of the acute perception of pain, but such patients have no recollection of any painful sensation. So far as can be judged from what is known of the encephalic centres, the pain under these conditions is perceived by some nerve-centre, probably in the pons varolii, but the impression is not conveyed to the cerebrum, and is not recorded by the memory" (p. 578). That is his view.

9138. That raises the point whether there is really pain in a sentient animal or only what you may call a local feeling of pain?—This would be a sensation of pain at the time, though the brain is unable to record it afterwards, to remember it. I have felt the same once myself, when I had nitrous oxide. I had had a tooth taken out some years previously by an incompetent dentist, and I had 11 tiny pieces left in my jaw which had to be extracted afterwards. I felt all the pain in a sort of way whilst this was being done, but when I recovered I had a faint recollection only that I had felt pain, and had heard what the dentist and anæsthetist said.

9139. Then your brain was not under complete anæsthesia?—It was partly.

9140. Mr. Morris's evidence was to this effect, that you might have a person really, as he believed, completely anæsthetised, but that in certain operations, especially touching certain nerves, you might have movements and cries and yet no pain?—That is said, I should think, not only by Mr. Morris, but it has been said by a number of surgeons. The testimony is always that the patient does not remember anything afterwards; but I think I have answered that, in giving my point of view, and in quoting Professor Flint's opinion. But I think that all anæsthetists are agreed that in perfect anæsthesia the body should be limp and motionless.

9141. Would you depend upon the nature of the anæsthetic used or not?—No, I do not think so. I presume that we are talking of ether and chloroform now.

9142. Or an overdose of morphia would have the same effect?—I do not think an overdose of morphia can be classed in the same category with an anæsthetic.

9143. Then you rather disagree with some scientific evidence which we have had?—Yes, I think I do.

9144. Then Mr. Morris further went on to say that to some extent consciousness and certainly power of movement came back before sensitiveness to pain. Would you agree with that?—I do not agree that that is always the case. I have a good deal of evidence which could be brought to show that that is not always the case. It may be the case, but not always.

9145. What I was coming to really is this: In certain animals you observed, as you have said, that there were movements which looked like the movements of an animal in suffering, from which you inferred that the animals were suffering?—Yes.

9146. Do not you think it is possible that those were merely reflex movements; that you may have cries and you may have movements without any real suffering?—I do not think so. I think those cases were cases of returning consciousness. At all events, it is not an ideal condition that the animal should move or cry; and as we can only go by signs, nobody can say, except by signs, whether the animal or person is suffering pain.

9147. Do you agree or not with some evidence we have had that in cutting certain nerves you always get a nervous contraction, although the animal is

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absolutely under anæsthesia?—Yes, you may get a nervous contraction; but that is quite different from the purposive movement which I have seen in some animals, notably in the so-called "brown dog," or the movements that I saw in the dog that I described under the title of the "Troublesome Dog." That is not a mere nerve contraction.

9148. Now I want to ask you, generally, have you seen Dr. Thane's observations on your book?—I have now.

9149. Having read them, do you still adhere to your statements, or do you agree to an extent with Dr. Thane?—I still adhere altogether to my statements in "Shambles of Science." I think Dr. Thane's criticism touches minor points, but leaves the book on the whole very much untouched.

9150. In what respect does it leave it untouched—generally, I mean to say. You say that it touches on some minor points?—For instance, he says: "The accounts given of the proceedings themselves are generally fairly correct."

9151. Yes; it is the inferences which you draw that I think he objects to?—Yes, I suppose he does.

9152. Which, he says, are not justified by the facts observed—that is his point?—He says: "The accounts given of the proceedings themselves are generally fairly correct."

9153. But that you misinterpreted what you saw?—He objects to the "tone" in which the work is written, and to the suggestion of "dishonesty and dishonourable conduct."

9154. Do you really say that there was dishonesty and dishonourable conduct in what you saw among the lecturers there?—I would not define it as "dishonest." I do not think that word is used throughout the book—or "dishonourable conduct."

9155. Do not you think that these men who were experimenting every day are capable of administering an anæsthetic properly?—I cannot pronounce an opinion as to whether they were capable or not. I can only speak to what I saw; that I did not think they did produce proper anæsthesia.

9156. Did you ever point out to any demonstrators at the time that the animal was not properly under anæsthesia?—No, I did not.

9157. Do not you think it would have been fairer, if you thought that the animal had been suffering, to call the experimenter's attention to it, and say: "Now, I saw movements which I thought indicated consciousness"?—I had experience of what happened when that was done once. There was a cat struggling very much at one of Dr. Bayliss's lectures, and a girl who was there said: "I am sure they have forgotten to give an anæsthetic." She told a student, a man, to go out and tell the lecturer that he had forgotten to anæsthetise this animal; and I am very pleased to see that Dr. Thane does not deny that this cat moved on the operating board before the lecture, or that Dr. Bayliss was fetched in consequence—it is in "The Struggling Cat."

9158. Where does Dr. Thane say that?—In his observations on "The Struggling Cat," page 6. Dr. Bayliss has evidently acknowledged that the cat began to move when he had gone out of the room to get his notes. Then, of course, his account is different from ours. He says that he came in and turned a tap.

9159. Yes?—Whereas we say that he came in and tightened the strings, and went out again, smoking his pipe, I believe—there is no objection to his smoking his pipe.

9160. Did he not also turn a tap?—No.

9161. Are you sure?—I am positive that he did not turn a tap.

9162. You know how the anæsthetic is administered there—from another room?—Yes?

9163. You know that the bottle is in another room?—Yes?

9164. With a pipe underneath the floor?—Yes. That is not the argument on this occasion. Dr. Bayliss says there was natural respiration. He admits that upon this occasion there was no artificial respiration, and consequently the anæsthetic could not come from another room. He says: "The anæsthetic was given from a Woulff's bottle with natural respiration." That would be quite a different sort of apparatus, and we did not see a Woulff's bottle (I know a Woulff's bottle quite well) upon that occasion, nor did we see Dr. Bayliss turn any tap. Moreover, the cat was placed in the corner of the room when we came in, on its little operating board, and afterwards it was moved on to the large table or counter before the students, and there was no bottle or anæsthetising apparatus moved with it.

9165. When the cat began to move, do you think there was any consciousness, or was it simply the reflex movements before returning consciousness came on?—I should say that it was entirely conscious. Dr. Bayliss says it moved; he does not deny that it moved.

9166. You think that that cat never had any anæsthetic?—Dr. Bayliss acknowledged later on that he had been experimenting on this cat earlier in the afternoon—that cat had been there a long time—and then he had gone out. And even supposing there had been the apparatus there, the cat was lying there all alone, and nobody attending to it. I think that is a strong point.

9167. It depends upon the nature of the apparatus. In some of these laboratories there is an automatic apparatus?—But Dr. Bayliss says it was not an automatic apparatus on this occasion. He says there was natural respiration.

9168. May I just read to you what Dr. Thane says, and ask you if you agree?—Yes.

9169. "This was an experiment on the production of heat in the sub-maxillary gland. The anæsthetic was being given from a Woulff's bottle with natural respiration. The lecturer left the theatre to get his notes." How long was he away?—I cannot say, because when we came into the room there was no lecturer there. I think we came first of all the students. There was only the struggling cat when we went into the room, and it was struggling very much when——

9170. "The cat began to move"?—Yes, Dr. Bayliss says "the cat began to move," but as Dr. Bayliss was not there when we entered, and as he acknowledges that he was not there when we entered, he cannot possibly know how much or how little that cat moved.

9171. But he can know how long he was away?—Yes, but he cannot know the amount of the struggles.

9172. "A student fetched him, and he come at once, and gave more anæsthetic"?—Yes, he came in, but I do not agree that he gave more anæsthetic.

9173. Have you made any inquiry from him about it?—Naturally not.

9174. "This required only the turning of a tap; possibly the reporter mistook the turning of the tap for tightening the strings"?—It is very strange. I cannot understand how it would be possible with natural respiration to produce more anæsthesia merely by turning a tap. It would be possible with artificial respiration when the air is being pumped continually from a pump by turning a tap. It is altogether beyond me, and I cannot find any explanation of such a thing.

9175. (*Colonel Lockwood.*) He gives the explanation Question 1767, in answer to Mr. Tomkinson?—Yes, I understand that it is possible with artificial respiration, but not with natural respiration.

9176. (*Dr. Wilson.*) Was there a tube in the trachea?—No, there was no tube in the trachea.

9177. (*Sir Mackenzie Chalmers.*) There was no tube in the trachea?—No.

9178. How was the anæsthetic applied in this case?—I saw no anæsthetic applied, so that I could not answer that question. Whether Dr. Bayliss had applied an anæsthetic to that animal before we came into the room I cannot say, but I do speak to this: That when we came in, and it was struggling in a corner of the room, there was no anæsthetic applied.

9179. Was Dr. Bayliss examined at the trial? Was that one of the cases at the trial?—No, that case was never touched upon.

9180. Then Dr. Bayliss says: "I see that the lady student, who is reported as saying, 'I am sure they have forgotten to give the cat an anæsthetic' has been advertised for. I have not heard whether she has been found"?—I do not know who advertised for her. I did not advertise for her.

9181. She was a stranger to you?—Yes.

9182. (*Chairman.*) Did I correctly understand you to say, in answer to a previous question, that you had never spoken to the lecturer, and that that was in

consequence, more or less, of what had happened when a lady student had spoken to him?—Yes, I saw the effect.

9183. What happened to the lady student?

9184. (*Sir Mackenzie Chalmers.*) She seems to have fetched Dr. Bayliss?—She sent for Dr. Bayliss. I meant to infer that it had very little effect that Dr. Bayliss knew of the distress of the young girl, who seemed very sensitive; in fact, she began to cry.

9185. (*Chairman.*) I thought the suggestion was that Dr. Bayliss treated her in some way that was extremely discouraging by his manner?—No, I simply meant that he took very little notice of it.

9186. (*Sir Mackenzie Chalmers.*) Are we to take it from you that your impression is that that cat, instead of having more anæsthetic given to it when it began to move, was simply tied down tighter?—That was my impression.

9187. That we must inquire into. After the experimenter's attention had been called to the fact, after somebody had gone out and told him that the cat was moving, instead of increasing or giving any anæsthetic, he merely tied down the cat more tightly?—He pulled the strings at the four corners and tightened them.

9188. What happened after that?—The cat was lying on its back.

9189. What happened after that to that cat?—After that the lecture began. Dr. Bayliss came in and explained that they were rather short of dogs in the laboratory, and this afternoon he could not have a dog. He also said that he had experimented on this cat earlier in the afternoon. We were sitting behind this girl who had shown so much distress, and we saw that it distressed her still further. She was then spoken to by a lady student at University College, who made light of it. I cannot recollect what actually was said, but she laughed, and probably reassured the girl that the cat was not suffering.

9190. How long did the lecture continue?—The lecture continued, I suppose, an hour and ten minutes—a little more.

9191. Was the cat killed at the end of the lecture?—No.

9192. No?—No, the cat was taken out before the hour was over.

9193. You do not know what happened to it?—No.

9194. Did the cat again make any motion; was there any sign of pain that you saw in the cat after what was done to it?—Yes, the cat struggled.

9195. Struggled still?—Struggled throughout the time we saw it; but, as often happens when you see an animal struggling, it did not show the same forcible movements—movements of the same strength—as it did at the beginning, which accounted for the girl who was so distressed becoming more calm afterwards. But it did move all the time distinctly.

9196. How far were you off?—I was very near; I was always very near. And Miss Schartau and I always walked up to the animal before leaving whenever the animal was not taken out before the lecture was over, and looked at it. Sometimes we spoke to the laboratory attendant.

9197. There we have an issue of facts which we must inquire into.

9198. (*Sir William Collins.*) Was Dr. Thane present at any of these experiments?—No, he was not.

9199. (*Sir Mackenzie Chalmers.*) Anyone can go in, I suppose; anyone about the place?—I think any medical man is allowed to go in.

9200. There are some other points which I dare say other members of the Commission will ask you about the investigation made by Dr. Thane. I want to ask you, generally, do you object to all experiments on animals? How far do you go in that?—Yes, I object to all experiments on animals.

9201. Irrespective of whether pain is caused or not?—I do not believe that vivisection can be made painless.

9202. You do not believe in complete anæsthesia. I want to take you to this case first. Supposing that an animal is properly anæsthetised, and is killed before the experiment concludes, before the anæsthesia passes off, do you object to that?—Supposing an animal could be properly anæsthetised, which I deny for vivisection operations as a rule, but supposing it could be, and supposing it were killed before it recovers consciousness, I should not object to that as an anti-vivisectionist; but I should object to it as a general humanitarian and for other reasons, but not merely as an anti-vivisectionist.

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9203. You do not object that the animals should be killed when they are unnecessary or for various reasons when they are a nuisance?—I object to the sacrifice of life.

9204. May I ask you this, for instance? You know that 20,000 dogs are killed every year in the Dogs' Home in London?—Yes.

9205. What I may call superfluous dogs?—Yes?

9206. Do you object to that?—Under present circumstances, and as the world is, I think it is a regrettable necessity, and as vivisection exists it is far better for the dogs to be protected and killed in that painless way. But I do not agree with what Professor Starling said before this Commission, that this is done to rid the streets of "the danger of stray dogs." I think it is done from a humane point of view.

9207. I want to know whether you object to that or not?—No, not at present.

9208. Then if a dog which had to be destroyed could be properly anæsthetised, do you think there would be any objection to increasing knowledge by means of its death?—I should think there is. Because I know that vivisection anæsthesia is always unreliable. I would trust a veterinary surgeon of skill and capacity to operate on an animal painlessly if he sought to do so; but I would never trust a vivisector in that capacity. Moreover, it must be remembered that vivisection is not only a few experiments on so-called "anæsthetised" animals.

9209. I will take you to the others afterwards. I want, first of all, to know whether you think it is wrong and objectionable, where an animal is to be killed, and assuming that you can give it complete anæsthesia, to use it for experimental purposes?—Yes; for the reasons I have stated.

9210. Then what it comes to is this: That you would not trust any lecturer who performs experiments, or any scientific man, to give complete anæsthesia?—No, from my knowledge of what vivisection is I cannot trust vivisectors with regard to complete anæsthesia.

9211. You do not agree with the experimenters when they say that in the case of an animal they give much heavier doses of anæsthetic than they would in the case of a human being, because it is somewhat important that the human being should recover from the operation?—I brought evidence last time to show that that is not so. I brought a considerable amount of evidence with regard to light anæsthesia to contradict that entirely, and also to show that fatal doses are not generally employed.

9212. Your opinion is that people who are performing what I may call vivisectional experiments prefer light anæsthesia, and do not give complete anæsthesia?—Yes; and in some cases they prefer no anæsthesia at all.

9213. Can you give me any case of that?—I said last time that I could give the Commission a considerable number.

9214. I have it all before me in your evidence. Can you give me any case in England?—There is a statement in a book that I referred to by Sir Lauder Brunton, saying that experiments with drugs cannot be made under anæsthesia.

9215. I have not come to those yet; those are what you may call feeding experiments?—Drug experiments, poison experiments; they cannot be made under anæsthetics, because the anæsthetic would interfere with the result.

9216. I quite agree with that. I want to ask you this. How far does your objection go on moral grounds to vaccination, the using of calves for the preparation of vaccine?—I object to vaccination for the sake both of the infant and of the calf.

9217. Have you ever seen a bad case of smallpox?—No, I have not.

9218. It is a pleasure some of us have had?—I have seen scars.

9219. I may take it therefore that if you object to vaccination you object to the whole principle of what

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is called serum therapy?—I do, both on humanitarian and on hygienic grounds.

9220. We have had a good deal of evidence about the case of diphtheria. You have been in hospitals and seen children dying of diphtheria, have you not?—I have, and I had diphtheria myself some years ago, very severely; I did not have the serum, and I recovered perfectly; whilst a friend of mine did have the serum, and got several complications, which taught me an object lesson.

9221. Are you not generalising too much from a single case?—I think not. I have plenty of evidence before me of very serious complications following on the use of the antitoxin. I have also a German book, written only for the medical profession, which gives no less than 13 diseases as the result of the use of diphtheria antitoxin. It is not an ideal remedy.

9222. No, I do not suppose that any remedy is ideal. But if there is a great concurrence of medical opinion that life is saved by using antitoxin in many cases in which it would be lost without, do you think it is unjustifiable to use it?—There is a considerable amount of medical opinion against the use of anti-toxin, both in this country and in others; I could quote many names; but even supposing that the majority of the medical profession claim that diphtheria anti-toxin is an invaluable remedy, that does not prove that it really is so. Medical history is full of discarded theories and superstitions that have gone. I have brought quotations from three works on medical history, but I will not trouble the Commission with them, in which it is specially emphasised that medical history is not least interesting because it always teaches you what not to do.

9223. I take it, then, that you object absolutely to the use of antitoxin?—Yes.

9224. If you were convinced that it was useful, would you still think it wrong?—I would, because I know from personal experience, from having had diphtheria, that there are other means of fighting the disease more natural and followed by less objectionable results.

9225. We had some very interesting evidence the other day about snake bite, and I want to ask you this. Do you think it is right to use animals in order to experiment on a new remedy for snake bite?—I should think it would be better to use other means. I do not believe in the scientific value of these experiments.

9226. Did you look at Colonel Rogers's evidence of the other day; have you seen it?—I am afraid I have not read it very carefully.

9227. What he told us was that he is able now to save a good many lives of people or animals who have been bitten by snakes, by scarifying the point and putting in permanganate of potash, and that he discovered that by a series of experiments, on, say, 20 or 30 unfortunate animals, it may have the effect of saving many hundreds of human lives and animal lives. Do you think it is justifiable or not?—There is no harm in using the remedy itself.

9228. Do you think he was justified in using animals to experiment on?—I think it would be better if he had used himself.

9229. The poor man has not got 20 or 30 lives. I am afraid he would have died 20 or 30 times. However, that is your answer. You think he ought not to have used animals?—Yes, I think, as in the case of other people who are devoted to either scientific or social or ideal questions of any kind, we all know that the highest thing is to sacrifice yourself, not others.

9230. Do you suggest that all experiments should be made first on the experimentor?—Certainly. I have no objection to vivisection, provided that the vivisectors experiment on themselves, and nobody could have. They are perfectly entitled to their opinions.

9231. A good many experiments have been tried by doctors on themselves?—A very small percentage of the whole.

9232. Do you draw any distinction yourself between causing pain to an animal in sport, and causing pain to an animal with the object of finding cures?—Certainly. I object to what is called blood sports, most seriously.

9233. All sports?—And I consider the anti-vivisectionist who does not object to them most illogical. I consider the anti-vivisectionist who takes pleasure in blood sports, which cause pain to animals, most illogical. In that view I believe I am not supported by the total number of anti-vivisectionists; but that is my opinion.

9234. I only wanted to know how far you go. Take, for instance, this case. Have you ever seen a good day's rabbit shooting?—No, but I can imagine what it is. I should think it hateful.

9235. But in what I may call your public agitation you do not include sports?—There must be a division of labour for everybody. I am a thorough humanitarian, so far as I can possibly be so. I have written against sport a good deal in Sweden.

9236. I do not know whether it is fair to ask you, which do you think is the most wrong: in rabbit shooting, to maim an unfortunate rabbit which crawls into a hole to die, or to perform an experiment on a rabbit which you think and hope may result in benefit to human beings?—That all depends on the way in which the two actions are done. My personal opinion would be that the vivisector (though I am a very strong anti-vivisectionist) who honestly thinks he works for the good of humanity, or the good of science, and who honestly tries to render his experiments painless, is on a higher moral scale than the man who seeks his pleasure from maiming and hurting animals.

9237. His pleasure is not in maiming and hurting; but in the pursuit of his pleasure it is incidental?—That alters the case a little; it depends on how far he is conscious of the maiming and hurting of animals, of course.

9238. (*Chairman.*) It is wicked in a bad shot, and not wicked in a good shot, you mean?—I do not mean exactly that. It depends very much upon the man's attitude of mind, does it not?

9239. (*Sir Mackenzie Chalmers.*) You agree that new remedies and new drugs must be tried, do you not?—Yes; but I think that the development of medicine is rather the decline of drugging; I think that is one of the signs of the time.

9240. You think, if I may say so, that it is wasted labour to try new remedies and new drugs?—I think it would be far better if we turned our attention to the preservation of health, and in a minor way to the prevention of disease, by strengthening the general health.

9241. So that you do not think it is justifiable, if a new remedy is brought in, to try it first on an animal, and then on a human being?—No, especially as experiments on animals are so useless. Professor Starling has told this Commission that "the final experiment must always be on man," and we have many statements against the danger of applying the results of experiments on the lower animals directly in the sick room.

9242. I may take it that you object to every experiment of every sort or kind for the purpose of advancing knowledge, believing that it is both wrong and useless?—Yes. There are experiments, of course, which I might quote which are altogether different from the ordinary experiments which are performed now. There is, for instance, one experiment given in Beddard's "Practical Physiology" (p. 151), with the object of studying the intestinal movements in a live cat under the Röntgen Rays. The cat is taken gently, it is not even anæsthetised, there is not even that discomfort to the cat, the movements of the intestines are studied by means of the Röntgen Rays, and it is specially said that the cat must be treated gently, as otherwise abnormal conditions will be set up. Nobody can object to an experiment of that kind, either on a human being or an animal. There is a certain class of physiological apparatus which is being used nowadays for experiments on human beings, by physiologists on themselves, and those experiments are not at all objectionable. I have brought plenty of pictures of that kind of apparatus, if the Commission care to see them. Now you can study the pulse, the blood pressure, and a great many physiological questions by means of this new physiological apparatus for man. And that is developing every day; and I believe that if humanitarian pressure is strong enough, physiologists will more and more have to apply this painless kind of apparatus to experiments on animals as well as on human beings.

9243. I take it that the general object of your evidence is that experiments on animals should be disallowed by law?—Yes.

9244. Without exception?—Yes.

9245. Assuming that that cannot be done, have you any suggestions to make as to how the existing law should be modified in any way?—I have. I would first of all suggest that demonstration experiments before students should be forbidden. Both in the "Shambles of Science," and later, in Sir Thornley Stoker's evidence before this Commission, it has been brought out very strongly that such experiments are demoralising; and, after my recent debate with Professor Halliburton a few days ago, where students came in large numbers, and behaved, as the Press characterised it, in a very brutal way, I feel still more strongly that those experiments ought to be discontinued.

9246. Have you any other suggestions to make?—Yes; that the use of morphia should be made illegal as an anæsthetic.

9247. (*Mr. Tomkinson.*) And curare?—Curare should be forbidden altogether.

9248. (*Sir Mackenzie Chalmers.*) Curare should be forbidden altogether?—Yes.

9249. And morphia should be forbidden as an anæsthetic?—Yes.

9250. You do not object to morphia being given previously to an anæsthetic?—No, not to help the induction of anæsthesia simply. But I believe when morphia is given in a fairly large dose it produces a condition which is deceptive, and that therefore, when vivisectors give a very small dose of a real anæsthetic afterwards, it may be misleading, but I do not think that that could be legislated against. But nobody can object to the use of morphia merely as an induction to anæsthesia.

9251. Have you any other suggestions to make?—Yes; that experiments on all the higher animals, dogs, cats, monkeys, horses, asses, etc., should be forbidden.

9252. With the exception of man, you think all other animals should be excluded?—I do not approve of experiments on men and women, except voluntary ones, and of those only when undertaken for the real good of the men and women—the patients; of course, all life is an experiment, if you extend the word.

9253. I do not want to play with words. Every dose of medicine is an experiment, I suppose?—Yes.

9254. Have you any other suggestion to make?—My whole object is that the Act should be repealed.

9255. Assuming that that is not possible, have you thought of any other course?—Yes, I have another suggestion to make, which I brought out last time, and which I feel very strongly about; that is, that students should not be allowed to experiment on frogs.

9256. Even pithed frogs?—Frogs are now experimented on by unlicensed people.

9257. After death, so to speak?—The question is, what is death? I have not quite finished my evidence on that yet, if I may be allowed to continue.

9258. You mean, that there are certain modes of pithing which, in your opinion, do not destroy consciousness—the sensation of pain?—I consider that a really pithed frog, in which the brain and spinal cord have been totally destroyed, does not feel pain.

9259. But you think that as long as there is any purposive movement the animal does feel pain?—Yes, as long as the spinal cord is intact.

9260. (*Chairman.*) You wish, I think, to add a note to your evidence with regard to Question 7430?—Yes.

9261. I think you wrote to the Secretary, and he suggested that you had better make it part of your evidence to-day?—Yes.

9262. It relates to the pithing of frogs; and perhaps you will just tell us what it is?—There were two notes, in fact three notes that I sent in, which I was asked to give in evidence instead. I was asked at Question 7315 to give a reference to a passage of Claude Bernard. The words which I quoted from Bernard's "Lecons de Physiologie opératoire," are preceded by the following sentence, relating to the one referred to, which I translate:—"The dose which we generally employ is 5 centigrammes, but one can give the dog, without any danger, a stronger dose, even double, of morphine, provided the salt is pure. The animal still remains sensitive, etc." Then I have a few words, if I may add them, on morphia; in fact, just a few sentences. I wish to add in extension of my evidence on morphia last time that the morphia doses generally recommended in text books are not fatal doses (I have a list of such doses here), and also that the survival of animals experimented upon under morphia is proof positive that the doses given are not always fatal ones. (I have another list of such instances.) Even Professor Starling does not assert in his evidence that all animals die after the so-called fatal doses of morphia to which he refers, but says that in most cases they do so; that was at Questions 3607-8. I have evidence here from Dr. William Murrell's "Manual of Pharmacology and Therapeutics," that even such a strong dose as 37 grains of morphia has been given to a dog without affecting it; and that doses of 17 grains have been given without producing serious symptoms; and Professor Starling mentioned a dose of 15 grains as being the utmost limit of a fatal dose.

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9263. What do you mean by the utmost limit of a fatal dose? If more was given it would be fatal, would it not?—Yes; but Professor Starling mentioned before the Commission that a fatal dose would be within one grain and a half and up to 15 grains. I have several extracts here to show that over 30 grains have been proved not to be fatal doses in dogs.

9264. (*Dr. Gaskell.*) Would you mind giving us the size of the dog?

9265. (*Chairman.*) I did not intend to interrupt the examination on this point; it is another point altogether; you were speaking about pithing frogs?—Yes. I can leave the morphia now until I am asked questions upon it; I have very little more to say about this point.

9266. If you have nearly finished it you might as well continue?—I was asked the size of the dog. This is an extract from a "Manual of Pharmacology and Therapeutics," by William Murrell, Lecturer on Pharmacology and Therapeutics to the Westminster Hospital, Examiner in Materia Medica and Pharmacy to the Conjoint Board of the Royal College of Surgeons and the Royal College of Physicians, London, 1896. Murrell says that "Recent observations show that dogs are but slightly susceptible to the action of morphia. H. C. Wood and David Cerna gave a dog weighing 15½ kilo. a dose of 2·4 gramò (37 grains) of morphin without affecting its respiration. Caldwell gave a dog 17 grains without producing serious symptoms; whilst to a number of other dogs he gave 12 grains hypodermically, with no bad effect" (p. 339). Then Claude Bernard refers to a dose of 30 grains, that is, twice the dose to which Professor Starling refers, and this enormous dose produced great excitement and convulsions of great violence. After four hours the dog was frightened, lay stretched out in a corner, but could walk. The dog was not dead the following day. That is from Claude Bernard's "Leçons sur les Anesthésiques et sur l'Asphyxie" (p. 201).

9267. Without any effect on what, does he say?—He gave this enormous dose. It produced great excitement and convulsions of great violence. "Without effect" was Murrell's quotation, which I quoted before.

9268. Without any effect on what?—On respiration.

9269. It was confined to that?—In both quotations it is shown that the dose was not fatal.

9270. (*Sir William Collins.*) Did the dog recover?—The dog recovered—both dogs recovered.

9271. (*Mr. Tomkinson.*) The dog did not lose consciousness?—Not in these two cases. Shall I go on to the next note that I have?

9272. (*Chairman.*) Yes, please; that is about the frog?—Yes. At Question 7430 I was asked to supply a note of the physiologists who attach different meanings to the word "pith." Thorough pithing of a frog is described in the following way by Dr. Augustus D. Waller in his "Exercises in Practical Physiology," part III., page 44: "Insert one blade of a strong pair of scissors into the mouth, as far back as it will go, and cut off the top of the head. . . . . Thoroughly destroy the brain, bulb, and cord by means of a stout wire pushed several times down the spinal canal. The frog is now thoroughly pithed, and should remain permanently flaccid and motionless; any movements of the limbs signify that the spinal cord has not been thoroughly destroyed." In "Essentials of Experimental Physiology," Dr. Brodie gives directions for thorough pithing, on page 16, as well as advice for incomplete pithing on pages 49 and 214. Professor Stirling includes mere decerebration as well as destruction of the brain and spinal cord, under the word "pith" (pages 219, 377, "Outlines of Practical Physiology"). In "Practical Physiology," by Beddard, Edkins, Hill, etc., the word "pith" is used in its proper and inclusive sense (page 14). Professor Busch says under the head-

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ing: "Pith a frog. This ordinarily means destroy the brain" (Laboratory Manual of Physiology, page 10). In his evidence before this Commission Professor Starling speaks of a brainless frog as a "pithed frog" (3467), and in the pamphlet, with which I have been favoured, Dr. Thane commits the same error. In the above-mentioned book, Dr. Waller states that "For certain experiments it is necessary only to decerebrate the frog—*i.e.*, to destroy only the hemispheres, or the hemispheres and optic lobes. In this case the scissor cut must not be made quite so far back, and the brain carefully removed." That ends my note on that question. May I ask a question? I have also added a list of those experiments which are recommended in text books to be made on only decerebrated frogs, but if the Commission do not wish to hear it——

9273. I think we had better go on with the examination now; I interrupted you before?—I was asked to give another reference.

9274. If it is only a reference, you might as well give it now?—I have a little comment upon it; so perhaps it had better wait.

9275. (*Mr. Ram.*) Will you tell me, please, what is your attitude with regard to the Act that we are acting under to-day? Would you entirely repeal it?—I would repeal it and substitute another Act prohibiting vivisection. I would not only repeal it ——

9276. You would enact in lieu of it an Act saying that vivisection was to be wholly illegal?—Yes.

9277. How would you define vivisection?—I would define vivisection as experiments calculated to inflict pain on living animals for scientific purposes.

9278. You would limit it to scientific purposes?—I should like to prohibit all painful experiments, scientific and otherwise; but if we are only dealing with scientific vivisection, I dare say it would have to have the word scientific.

9279. Would that not leave a very wide field open by which great pain could be inflicted on a large number of animals, not for scientific purposes, but for purposes which might be considered less worthy than scientific purposes?—Certainly, there is a great deal of other cruelty which should be stopped by other Acts, if possible.

9280. Would you stop all of it?—Personally, I would stop by legal enactment as much as possible, but I have not the slightest hope that in my lifetime all such cruelties will be stopped.

9281. Take the farmyard operations, which have been spoken to by other witnesses; would you stop them?—I think that it should at least be enacted by law that those farmyard operations are not permissible without anæsthetics.

9282. You would give anæsthetics in every case?—Yes.

9283. Every colt, every lamb, and every young animal of every sort?—I think it is most cruel to perform that operation without anæsthetics.

9284. And you would make it illegal to perform any one of those operations on any animal, at any age, without anæsthetics?—Yes, certainly.

9285. Meantime, if the Act is to continue, you make the suggestions that you gave to Sir Mackenzie Chalmers just now?—Yes.

9286. And you do not wish to add to them?—No.

9287. Do you know the practice in America?—I know something of the practice in America.

9288. Do you know that there is no restriction by law there of vivisection?—I know, nor is there in my own country, Sweden, or in Germany or France; and I know something of what is being done in those countries.

9289. Are the practices in those countries worse than they are here to-day?—I consider that they are. I consider that they are much worse in countries where every medical student has the right to vivisect any animal in whatever way he pleases.

9290. Therefore the Act that we are acting under to-day is of advantage in making things better than they are in countries where there is no such Act?—Certainly, I think so.

9291. Do you know anything of the writings of Dr. Leffingwell, an American doctor?—Yes, I know of him.

9292. I have here a book which was sent to me by Mr. Sydney Trist, of the office of the "Animals' Guardian." Do you know the "Animals' Guardian"?—Yes.

9293. Do you know the people connected with it?—I know some of them.

9294. Are they anti-vivisectionists?—Yes.

9295. They sent me this book, and I find in this book that what this gentleman, Dr. Leffingwell, desires is that there should be in America exactly the same Act as we have in England?—Yes.

9296. You are aware that that is what he desires?—Yes.

9297. Then I need not trouble you, as you know it, with extracts from his book. Do you agree with him that in America, and perhaps in Sweden, to have such an Act as we have would be a very great step in advance?—I quite agree; because here, at least, you have an opportunity of finding out something of what is being done, but you cannot in America or Sweden.

9298. And here, too, if any illegal case is brought to the knowledge of the Home Office proceedings can be taken against a person doing an illegal act?—Yes, it should be so.

9299. And inspectors exist for the purpose of finding out whether there are such illegal practices or not?—Yes, they are supposed to.

9300. In how many of the experiments which you have enumerated in the "Shambles of Science" do you consider the Act had been broken?—I think I have been asked that question before.

9301. If you have answered it before I am sorry I have asked you?—I think I was asked the question this morning.

9302. At any rate, I need not tie you down to ones and twos and threes. You do think that in several of those experiments, if not in all, there have been infringements of the Act?—I think that the Act, as it stands now, admits of a great deal of cruelty. There is no effective control over vivisection as the Act stands to-day.

9303. Whether there is control or not, in your opinion some one or more of these cases were infringements of the Act?—I believe, on the whole, it is possible to do a great many cruel things under the Act.

9304. Did any of these cases that you thought cruel strike you as being infringements of the Act?—That is a question that is rather difficult to settle. If a man performs an experiment under licence or under Certificate C, before students, which licence or certificate lays down that the animal should be anæsthetised, and if he employs an anæsthetic that is no real anæsthetic, or if he gives the anæsthetic in a manner that is not proper and is not conducive to total abolition of consciousness, is that infringing the Act? It comes to that question.

9305. (*Colonel Lockwood.*) Like "The Struggling Cat"?—Yes.

9306. (*Mr. Ram.*) You thought, then, that some, at any rate, of these experiments that you saw were conducted in the way you describe?—Yes.

9307. Did you make any representation to any Inspector or to the Home Office as to any one of these alleged breaches of the law?—No.

9308. Over what period do all these experiments extend?—The experiments described in this book extend, I believe, over a period of six months; I am not quite sure.

9309. And at no period during that time did you make any representation or any complaint?—I made representations, but not to the Home Office or to the Inspector.

9310. You made them, I think, to the public through this book?—Yes; I think we did.

9311. Did you ever apply to the experimenter to know whether you had misunderstood what was done or to ask for any explanation?—No, we did not.

9312. I will deal with one or two of those cases in a moment; but before I come to them I want to ask one or two general questions. I refer first to your answer to Question 7243. The question asked you was, "May I point out that the train of your argument really seems to involve a belief on your part that they intended to suggest that the anæsthesia should be so slight that the animals might be conscious; because

it arose in connection with a question of cruelty?" And your answer is this: "My whole evidence is in connection with cruelty"?—Yes.

9313. I take it that that is your position now?—Yes.

9314. Eliminate cruelty and you would not object to vivisection?—But my point of view is that cruelty cannot be eliminated from vivisection.

9315. But take a hypothetical case. Assuming that cruelty could be eliminated, and that there was and could be no suffering on the part of animals, I take it that you would not object to vivisection?—I have already answered that question, too.

9316. Would you mind answering me?—No, I will answer you in the same way: that supposing such an impossible thing were possible as that vivisection could be made painless—are you quoting drug experiments and inoculation and those things, or are you assuming only a surgical case?

9317. I am assuming for the moment a surgical case?—If it were absolutely painless, if it could be made so, and the animal killed immediately after, so that there was absolutely no return of consciousness, I said that I should not object to it merely as an anti-vivisectionist, but I should object to it from a general humanitarian point of view.

9318. I am afraid I do not quite follow that. You as an anti-vivisectionist would not object because there would be no suffering. Will you clear it up?—It is very difficult to answer a hypothetical question.

9319. I will put a question which is not hypothetical. What is the distinction that you draw between the anti-vivisectionist and the humanitarian?—The anti-vivisectionist may be a person who sees very acutely the cruelty and selfishness in vivisection, but has not yet had his eyes opened to other cruelties in the world which may be just as bad, or nearly as bad.

9320. That is your definition of an anti-vivisectionist?—But the general humanitarian is a person who sees other forms of cruelty and selfishness, and is anxious to remedy them, too.

9321. I do not quite follow the distinction?—As a humanitarian, I should object even to the painless vivisection (supposing vivisection could be made painless) of a dog.

9322. Why?—Because, as I have also said in my previous evidence before this Commission, I am not an anti-vivisectionist solely on the ground of pain; I am an anti-vivisectionist on the ground of objecting to the general exploitation of animals.

9323. That is the reason why I referred you to your evidence in which you said "My whole evidence is in connection with cruelty"?—Yes?

9324. Do you wish to modify that?—That I can easily stand by, because my whole evidence had to do with bringing proof that vivisection cannot be made painless.

9325. Do you wish now to go further, and say that you now object to what you call exploiting animals for any purpose?—I am very anxious to be allowed to stick to my position. My position before this Commission is simply that of an anti-vivisectionist, and I am an anti-vivisectionist because I know that vivisection, including drug experimentation, inoculation experimentation that involves keeping an animal alive for a long time, extirpation of organs, and things of that kind, never can or will be made painless. That is my position here.

9326. I am reminded that you said, in answer to Question 7622, "I should object to an experiment on a living animal, supposing it was unconscious . . . because I take my stand against vivisection, not only and entirely on the ground of pain, but on the broader humanitarian ground of objecting to the general exploitation of animals for our use"?—Yes, and I was asked a good many questions last time as to what I considered to be exploitation of animals.

9327. I do not want in the least to go over ground that you have already covered, or to put anything to you which might be an extension of what you said; but do I take it that in your view any pain inflicted on an animal otherwise than for the animal's good is wrong?—I consider it to be so.

9328. How about breaking a horse to harness?—That may be for the animal's good. I consider that the domestication of animals is distinctly for their good, because it trains their minds and helps their general mental development.

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9329. I understand that it is lawful to break a horse to harness with whip and spur, and so on?—I have read a book by a very famous Norwegian trainer of horses who does not do it with whip and spur, but does it in a most humane way; it is a most interesting book.

9330. I wish it was more universal, but I am confining my question to what takes place to-day?—I think that any cruel training of horses and breaking in of horses is most objectionable, but I do not think that the process need be cruel.

9331. Supposing you take the best process that you can conceive, the horse is ultimately broken in, and you make him drag a carriage or a cab in London, not to an excessive extent, you do not injure the horse by doing it, but you make him drag it about contrary to his wishes. Is that right or wrong?—It depends entirely upon the driver, and on the amount of driving that is done with the horse and the amount of load that the horse has to pull.

9332. If it has a moderate load and is driven for an hour or two and is made to go uphill when it would rather not, and is made to go in a cab when it would rather be in the field, is that right or wrong?—I do not think it is cruel to be made to do what you would rather not do. We are all of us made to do what we would rather not do.

9333. Should you call it exploiting the animal?—No, no more than I consider myself exploited by having to give evidence before this Commission. I do not like doing it.

9334. (*Chairman.*) I think there is a little difference. I do not think you have been compelled to come here; you have come willingly. The horse is compelled to go in a shaft and to be fastened to the pole of an omnibus?—But I am compelled by my own moral feeling with regard to vivisection.

9335. There is a great deal of difference between being compelled by your own moral feeling and being compelled by somebody else?—But a father and mother have to compel their child to do a thing very often both mentally and physically. You would not call that cruelty.

9336. We do not like it applied to us when we grow up.

9337. (*Mr. Ram.*) What I really want to know is your definition of exploiting animals. It would be very helpful to us when you are telling us what view you wish us to take?—I could go on for days telling the Commission what I consider to be exploitation of animals.

9338. (*Chairman.*) Then I am sure Mr. Ram will withdraw the question?—It is too broad, I think.

9339. (*Mr. Ram.*) In mercy to the rest of the Commissioners I will withdraw it. It is a fact, is it not, that in consequence of the exploitation of animals—that is, use being made of animals for the convenience of mankind—thousands of animals lead a very comfortable life which they otherwise would not lead; do you agree?—Some animals lead a very comfortable life, but the majority do not.

9340. Supposing that we did not eat sheep and oxen after they were dead, do you suppose that we should keep them with the utmost comfort for three or four years, feed them in winter, and house them in bad weather?—No, but the journey to the slaughter-house and the present slaughter-house methods are anything but comfortable.

9341. Do you think it is not worth the animal's while to have three or four years of a very happy life because at the end it will be killed by a blow?—I would certainly prefer to be a wild sheep than a domestic sheep.

9342. Supposing that mankind did not exploit animals for their own good—horses, cows, and so on—for use in life and for food after death, how many of those animals do you think we should have in this country?—The supply is generally found in nature to meet the demand, and those who have entered upon this very wide question (and many competent people have done so) have come to the conclusion that supposing we gave up eating sheep and oxen the supply would naturally regulate itself, because at the present moment there is a great deal of artificial breeding. For instance, we do not eat dogs and cats in this

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country, and yet that matter can be managed without our being cemeteries for them.

9343. Do you agree with me that if it were not for the use of these animals a great many of them would not exist, or there would be many thousands fewer?—They would not exist in the same quantity.

9344. Why should a man keep a horse at all if he never uses it?—I have not said that a man may not keep a horse provided that he uses it in a humane way.

9344a. Why should a man keep a herd of oxen unless he meant ultimately to eat them or to sell them to others for food?—He might keep oxen as the peasant in Sweden keeps them, to plough his soil and draw his carts and to work.

9345. Do you think that oxen kept to draw carts and till the soil in this country would not have a worse time of it than if they are kept for four years and then knocked on the head and eaten?—Very probably they would at the present moment. It depends whether we are arguing from the point of view of greater humanity or lesser humanity than exists at the present moment.

9346. I will pass from that. Now I want to come to your answer to Question 7645. I will read the question if you desire it, but it is rather long, and the answer is what I want to deal with?—I know the point.

9347. You say: "I should not dream of denying that that method"—that is, the method of immunising animals for cattle plague—"has been discovered by experimentation on animals; in fact, that is something that we all know; but the point is whether some other method might not have been employed, and could not have been employed with equal advantage. I believe, on the whole, in natural immunity versus artificial immunity, and I am not at all inclined to admit that the principle is a true or right one." Could you tell me what other method you had in your mind then?—As relating to that special case, I believe I had in my mind the method which has been referred to before the Commission of isolation and slaughter; that, I think, in the first instance. And, secondly, I had in my mind an objection to the whole system of immunising by means of sera, the various substances drawn from an artificially diseased animal, which I do not believe in.

9348. Then what you would do would be to isolate and not to attempt to immunise at all?—I do not believe in those methods of immunisation. I believe that they are unhygienic—dangerous—blood poisoning.

9349. And you would substitute for them simple isolation?—I would substitute for them sanitation first, prevention of disease and isolation. I believe immensely in the natural powers of the blood of slaying bacteria, in the so-called bacteriacidal powers of the blood.

9350. You are opposed, then, to the use of any sera at all?—I am, from the hygienic point of view as well as the moral.

9351. Some witnesses have told us, and I want to know whether you agree with them, that they object to the use of sera because it is the importing of a substance from the body of an animal into the body of a human creature; is that your objection?—That would not exactly be my objection. If I am to speak on the subject from the scientific point of view, I object to it because I know that the serum of one animal has the power of destroying the red corpuscles of another animal if the serum be injected into that animal—what is referred to as the globulicidal action of the blood; and that wonderful capacity in one's own blood is a proof that we are not meant to lend and borrow blood from one another, and especially not from different species. I also object to it because all artificial immunity which can possibly be induced by these sera is of very little value compared with that of strengthening these normal and natural powers of the blood which, when we have them, are an absolute protection against bacteria and disease.

9352. Then you do not think that there is some artificial immunity which can be obtained?—In some cases no doubt there is a certain amount of truth underlying the whole system of serum therapy, as, for instance, we know that in the case of measles and certain other diseases if you have an attack you are liable to be protected for a certain time. But I believe that the principle is wrong and its application is wrong.

9353. May we not hope that further experiments, the principle being right, might show a way to applying it more accurately and more beneficially than at present?—I do not think so. There is a paper by Dr. Brodie on "The Immediate Action of an Intravenous Injection of Blood Serum,"* of which I have brought an extract, but I will not trouble the Commission with it, showing from his experiments with serum that serum is by no means an innocuous substance, but is always more or less injurious.

9354. Now one or two questions, please, with regard to some of the other cases alluded to in the "Shambles of Science." At page 37 you deal with the incident of the marmot. You speak of a marmot the spinal cord of which had previously been divided?—Yes, I have a short statement on that marmot, which I brought with me last time, in answer to Mr. Thane's statement on the marmot, and perhaps I am allowed to read it now.

9355. (*Chairman.*) That is just what Mr. Ram is going to elicit from you if you will answer his questions instead of reading your statement.

9356. (*Mr. Ram.*) It may be that my questions will save your reading?—Yes.

9357. You observe that the statement which you make is, "We once saw a marmot the spinal cord of which had previously been divided"?—Yes.

9358. On that Mr. Thane says: "I knew this marmot well. It belonged to Dr. Pembrey, of Guy's Hospital Medical School. I have often seen it. It was never experimented upon, save for having its temperature taken. It is the only marmot that has been shown to physiologists in this country. To the general regret it died a short time since. I have since examined the body. The spinal cord has not been divided." Do you accept that statement of Mr. Thane's?—I have prepared my answer here.

9359. I will not preclude it if it is necessary, but will you just tell me first do you accept that statement of Mr. Thane's?—If I may give it in my own words——

9360. (*Chairman.*) The only difficulty is that we have a little essay read in answer to a question?—It is not an essay.

9361. It looks rather formidable?—No, it is not really.

9362. It means that a great deal is introduced which really is not evidence at all very often; it is information furnished to you by others who are not here?—This is a direct answer to the statement which Mr. Thane made in his evidence on the marmot.

9363. But you never saw the marmot except on this one occasion?—I never saw that marmot, except on this occasion.

9364. I think you had better follow the questions.

9365. (*Mr. Ram.*) If you will just answer my question first, you may find that your statement is not necessary. Do you accept that statement of Mr. Thane's as to this marmot, that it is the only marmot that has been shown to physiologists in this country; that it died, and that he examined the body and found that the spinal cord had not been divided?—Very well. I will answer your question so far as I can. The statement that that marmot had never been experimented upon save for having its temperature taken is altogether incorrect. According to a paper by Dr. M. S. Pembrey——

9366. (*Chairman.*) Now this is the very thing——?—I cannot answer the question unless——

9367. Will you allow me just to say this. We are not here for you to disprove what Dr. Thane may say about this marmot by saying that Dr. Pembrey has said something else. If you think it necessary, Dr. Pembrey could come here. We cannot try the issue as to whether that marmot was experimented upon before or had had its spinal cord severed?—But, my Lord, it is an answer to a personal attack on me, and I understood from what you said last time that I was to be allowed to answer anything which had been brought into the evidence and which was in the nature of a personal attack.

9368. I do not call this a personal attack. Here it is

* *See "Collected Papers for the Research Laboratories of the Royal College of Physicians, London, and the Royal College of Surgeons, England." Edited by T. G. Brodie* (1903). L.L.-af-H.

said that the statement which you make in that book about this marmot is not accurate?—I bring evidence to show that Mr. Thane's criticism upon our book is incorrect.

9369. (*Mr. Ram.*) Will you forgive me for saying that I am not going into his criticism, and I certainly am not making any attack upon you. Is there any evidence which enables you either to disprove or accept Mr. Thane's statement that he examined the body of that marmot and found that the spinal cord had not been divided?—I can prove first that experiments had been performed on this marmot before.

9370. We will leave that for the moment, because I am dealing with the dead marmot?—In the second place we never laid claim to having seen the actual division of the spinal cord of this marmot. It is very difficult to answer such a complicated question if I am not allowed to give my answer fully.

9371. (*Chairman.*) It is a very simple question?—The marmot, according to Mr. Thane's criticism, was in a state which resembled that in which the spinal cord has been divided. That has been acknowledged by Mr. Thane. He said that it was in a state which resembled paraplegia. We drew that inference from seeing the marmot and from my experience of other marmots, because I have lived a great deal in the Swiss Alps, and as a humble student of natural history I have observed marmots, and I may say that if the spinal cord had not been divided that marmot was very different from other marmots. I have not, of course, made a post-mortem on this marmot.

9372. (*Mr. Ram.*) But you see Mr. Thane has?—Mr. Thane says he has.

9373. I want to get it quite concisely. You state here as a fact that you once saw a marmot the spinal cord of which had been previously divided. Mr. Thane says that he examined the body of that marmot after death and found that its spinal cord had not been divided. Do you admit that you made a false deduction from what you saw?—I do not admit that we made a false deduction from what we saw, because I have no evidence sufficiently strong to convince me that we did. I have not seen that marmot myself after its death.

9374. Will you not accept the evidence of Mr. Thane?—I should hesitate to accept the evidence of Mr. Thane because he had just made a most inaccurate statement by saying that that marmot had never been experimented on before save for having its temperature taken.

9375. Then I am to take it that this distinct statement of fact by Mr. Thane is one that you decline to accept?—I decline to accept it on the ground that he had just made an inaccurate statement.

9376. Will you admit that the state of the marmot was one which might lead you to make a wrong deduction?—I need not admit that, because the marmot looked as if it had had its spinal cord divided.

9377. And therefore you inferred that it had?—Yes.

9378. And you still stick to that inference?—I see no reason not to stick to it.

9379. Then I leave that.

9380. (*Chairman.*) Do you think that because in your opinion Dr. Thane has made a misstatement about the number of experiments on that marmot it justifies you in dismissing the whole of his evidence?—I think that when Mr. Thane says that that marmot was never experimented upon save for having its temperature taken, and it had actually been subjected to fasting experiments—starvation experiments—I am justified in not accepting the second part of his statement.

9381. But supposing we thought that you were mistaken in your view about this marmot, you would think it very unfair that we should not accept any of the statements you have made in the "Shambles of Science"?—I am sure that the Commission will carefully scrutinise anything that I have said.

9382. We certainly shall, and, I hope, fairly.

9383. (*Colonel Lockwood.*) You want to bring Dr. Pembrey in evidence?—I have got the actual paper by Dr. Pembrey,* who owned this marmot, showing that starvation experiments had been performed on this marmot in the autumn time lasting 52 hours, 50 hours, and 42 hours at a time, when the marmot is in the greatest need of feeding, being an hibernating animal.

9384. (*Mr. Ram.*) What you saw was capable of one of two explanations, either that the animal was hibernating or that its spinal cord was divided, is that so?—No, it was not capable of that first interpretation which you refer to, because if it had been hibernating it would not have bitten the demonstrator, and Mr. Thane does not deny that it bit the vivisector, which really is the chief point in our bringing in that marmot.

9385. Then you ask us on this evidence, which we have to balance, to say that this statement of Mr. Thane's is and must be to his knowledge untrue?—I do not ask the Commission to accept that. I only beg the Commission to excuse me from accepting Mr. Thane's statement.

9386. What alternative can you suggest to us that we can find?—I can only leave the question with the Commission to draw their own conclusion between the two statements.

9387. In that same chapter which begins about the marmot you give an account of an experiment on what you call "The Quiet Cat"; that is at pages 37 and onwards?—Yes.

9388. Do you suggest, or do you wish the reader to believe, from this account, that that cat was under anæsthesia the whole time of the operation or not?—May I just look at the chapter.

9389. If you please; it is only three pages?—That cat was quiet all the time, and artificial respiration was, according to our observations, maintained. As no operation had been performed which necessitated artificial respiration (because sometimes it is necessary or the animal will die) we drew the conclusion that the animal was under curare, because artificial respiration would then be necessary, and, of course, if the animal was under curare it would be absolutely quiet.

9390. Is the conclusion which you desire the reader to draw that the animal was under curare and not under an anæsthetic?—The conclusion might be drawn that the animal had had both curare and an anæsthetic, and when curare has been given an anæsthetic is highly unreliable, and the animal would lie quiet without further doses.

9391. Will you show me anything in this chapter on "The Quiet Cat," which gives any indication to the reader that there was any anæsthetic employed at all?—No, I do not think anything is said about an anæsthetic.

9392. You have just told me that the animal, in your opinion, might have had both an anæsthetic and curare administered to it?—We do not say anything about an anæsthetic for the reason that nothing was said to the students.

9393. That is what I complain of. You have just said that, in your opinion, the appearance of the animal was such that both an anæsthetic and curare might have been administered?—I said that they might have been; but, if I might continue, our inference was that no anæsthetic had been given because we did not see any administered; there was no smell of ether or chloroform, and nothing was said by the lecturer about an anæsthetic having been given.

9394. (*Colonel Lockwood.*) Who did this experiment?—Dr. Bayliss. All the experiments at University College described in "The Shambles of Science" were by Dr. Bayliss.

9395. (*Mr. Ram.*) Then I understand that you draw the conclusion yourself, and that you wished to communicate it to the reader that this cat was not under any anæsthetic at all?—We drew the conclusion that it was curarised only.

9396. (*Chairman.*) Did I catch rightly what you said just now that all the operations described in the "Shambles of Science" were by Dr. Bayliss?—All the operations referring to University College were by Dr. Bayliss. May I add to my answer in conclusion that we do not state definitely that the animal was not anæsthetised.

9397. (*Mr. Ram.*) So I gathered. I asked you whether there was anything to indicate that it was?—It was not sufficiently indicated to us to enable us to state such a thing positively.

9398. In the case of the "Dog that Escaped" which begins on page 67, was that dog, in your opinion,

* "*The Respiratory Exchange during the Deposition of Fat*," by *M. S. Pembrey, Journal of Physiology, Dec.*, 1901.

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under anæsthesia the whole time that it was under the experiment?—That dog we describe as struggling until curare was injected, and I note that Mr. Thane does not deny that curare was administered.

9399. He says that it was?—Yes, and so do we in the "Shambles of Science," and after the injection of curare the animal became absolutely quiet.

9400. Do you know that Dr. Thane also says "anæsthesia was kept up the whole time by pumping air charged with a known amount of anæsthetic through the lungs"?—Yes, that is a distinct contradiction of what we say in the "Shambles of Science."

9401. It is?—We say that no arrangements for artificial respiration had been made. And there again I must stick to what we have said for this reason, that after that particular demonstration we went up to the animal and stood near it for some five minutes after the other students had gone, talking to the laboratory boy, and I had ample opportunity of observing at that time whether artificial respiration had been maintained or not.

9402. Do I rightly understand that it was your belief then, and is now, that that animal was under curare only and had never had any anæsthetic at all?—That is my belief.

9403. Did you ever make any complaint to the Home Office about it?—We never made any complaint.

9404. There was, as you believed, a very gross violation of the law and horrible cruelty?—Yes.

9405. And you made no representation about it?—No, and our book was not written for the purpose of showing how the Home Office administers the law in England.

9406. (*Chairman.*) I do not think Mr. Ram's question referred to the book, but to what you saw. Whether you had written the book or not you made no complaint?—I see. I beg your pardon.

9407. (*Mr. Ram.*) You made no complaint at all?—No.

9408. At page 147, at the end of what you describe as "A Grand Demonstration," you say: "We meet the boy carrying the miserable white rabbit into the lecture room again"—did you at the time when you wrote that believe that the animal, the miserable white rabbit, was alive or dead?—When the animal was brought in again we had no opportunity of observing whether it was alive or dead; we simply put in the book that it lay absolutely still. It had been taken away from the room because it struggled. It might have been alive or it might have been dead. We do not insist on its being alive; the description is merely general, and gives no details to that effect.

9409. What was the inference which you intended the reader to draw from that sentence?—Either that the animal at that time was properly anæsthetised or it may have been that it was dead. I say that it was impossible for us to draw any inference; you cannot if you merely see an animal carried in when you are passing out of a door and you see it is lying absolutely still.

9410. You notice that in this case again Mr. Thane says: "An anæsthetic (ether) was used the whole time; it was given just in the same way, and under the same conditions, as in an operation on man"?—Yes; and the whole chapter "A Grand Demonstration," in our book describes how carefully ether was being given. We begin by saying "There is a strong smell of ether meeting us as we walk up the stairs."

9411. Do you think that the animals referred to in this "Grand Demonstration" suffered anything, or that they suffered nothing?—I believe that the animals, as we have endeavoured to show, continually came to, and I see that Mr. Thane does not deny that. He says, "There was probably some movement now and then as the effect of the anæsthetic began to fall off. More ether was at once given." That is exactly the same as we describe.

9412. He also says: "The so-called 'violent struggling movements' of the rabbit are greatly exaggerated. As explained by the lecturer (see p. 143), they are the reflex working of the muscles, excited by the injection of any depressor substance; they occur, as happened here, in deep anæsthesia"?—Yes, that would imply the purely reflex effect of such injection, but that is not what we referred to.

9413. Did you see anything there which enables you to say that it was not purely reflex action?—The suggestion throughout this chapter——

9414. Please answer the question?—May I say that throughout this chapter, so far as I can remember at the present moment, we do not suggest that everything was not done to anæsthetise the animals.

9415. Do you suggest that the animals suffered any pain?—We suggest that the animals kept coming to, and that everything was done to put them under the influence of the anæsthetic again.

9416. Is it your suggestion that in the course of that experiment they suffered, or that they did not suffer?—There may have been some suffering on return to consciousness.

9417. (*Chairman.*) Do you make any suggestion that in that experiment the requirements of the Act were not complied with?—No, that suggestion is not made in that chapter. The suggestion is rather made of the difficulty of anæsthetising these three animals, and the nature of a vivisectional experiment versus a surgical one.

9418. (*Mr. Ram.*) I want to read to you one expression in your book, and to ask you to what it applies. To whom do you intend this sentence on page 68 to apply: "The tormented long for their freedom, but the tormentors fight the merciful death. They are artists in the black art of producing the utmost agony, while the gateways through which death could enter are carefully watched; but sometimes they are not clever enough"; that is in "The Dog that Escaped"?—That is not meant to apply to any special vivisector; it applies to the whole system.

9419. And to everybody who practises vivisection?—The style of this book is not like a law report or anything official, like an inspector's report; we say things which have a general application, and not only things which have a special application.

9420. Then you deny that it has a particular application to anybody whom you could name?—I think it is quite clear to anybody who reads it that it refers to vivisectors generally, and to vivisected animals generally.

9421. Have you ever in England seen anybody, any operator, who, in your opinion, endeavoured to produce the utmost agony while watching the gateways through which death could mercifully enter?—I have read of many, as, for instance, the experiments of Mantegazza in Italy.

9422. May I take it that these words are not, in your opinion, applicable to anything you have ever seen in England?—They are applicable to the whole system of vivisection.

9423. In England as practised to-day?—In England, too.

9424. As practised to-day?—Of course, if you read it very literally word for word, and take it to pieces as if it were, as I say, the report of an inspector, perhaps, I cannot say that I have ever seen any vivisector who is an artist in the black art of producing the utmost agony; but from reading the whole literature of vivisection I know that there have been such.

9425. I am asking you whether here in England, under the law as it exists to-day, you can give us any instance of any person whom you have either seen in England or read of in England, who has produced the utmost agony, and carefully seen that death did not occur?—I am sure that I can give an instance of that.

9426. Will you give us one, please?—Firstly, all vivisectors generally——

9427. Please leave the vivisectors generally. I ask, Can you give us any instance in which in England since this Act came into operation those words could be justly applied?—Then I must say that the method of vivisection in itself is an attempt to stop death because death would interfere with the experiment.

9428. (*Chairman.*) Surely you can answer the question a little more directly?—Then I can say that there are vivisectors at present licensed and holding certificates in England who, I think, would be quite capable of exercising the "black art of producing the utmost agony, while the gateways through which death could enter are carefully watched."

9429. (*Mr. Ram.*) Will you give me the name of one person?—I can give you the names of several. I give you the name of Dr. E. Klein, who in 1906 held three

certificates exempting him from the obligation to use anæsthetics.

9430. Any other?—I can give the name of Dr. Schäfer, who in his last evidence before the Royal Commission exhibited the greatest callousness with regard to frogs.

9431. Any other?—I can give you the name—I hope I do not give offence to the Commission—of Sir Lauder Brunton. I quoted some experiments of his in my former evidence which I considered callous and brutal.

9432. Any other?—Am I supposed to go on?

9433. I am asking you for any that you desire to give. You assert that that is, in your opinion, the state of things to-day under our present law here in England?—If the tendency of vivisectors be to do cruel things, I do not think there is any power of stopping them in England, neither in the law as it stands nor in its administration as it is now administered; there is any amount of liberty under the administration of the law in England.

9434. You believe that these people whom you have named are all capable of doing this act—of producing the utmost agony while the gateways through which death could enter are carefully watched?—I trust I have not mentioned any name without giving a reason for so doing.

9435. Now, can you give me, please, any instance in which any one of these persons—take Dr. Schäfer, for instance—has been guilty of anything like this?—Certainly, if I am allowed to read his evidence before the last Royal Commission.

9436. What is the date of that?—It is the last Royal Commission.

9437. That would be before the present Act was passed?—Yes, the last Royal Commission on Vivisection.

9438. After which the Act was passed?—Yes.

9439. You recollect that my question was under the Act to-day; still, I do not want to exclude anything?—I did not quite catch that question before.

9440. What I asked you was: Can you give me instances either of people or of operations since the present Act was passed which justify this definition of yours?—I think I should be able to do it if you will allow me a few minutes to think.

9441. Perhaps you would like to do it after the adjournment.

9442. (*Chairman.*) Will you let me ask you, before we pass from these words in this chapter, "The Dog that Escaped," do you consider that in the case of the dark-coloured dog, which you call "The Dog that Escaped," the operator and demonstrators were "artists in the black art of producing the utmost agony while the gateways through which death could enter are carefully watched, but sometimes they are not clever enough"; and then (after three dots) you go on, "There is a dark-coloured dog somewhat resembling the Dalmatian," and you describe the experiment. Did you consider that those who were operating upon this dog were such artists as you describe there and guilty of what you describe there?—I think that in writing that passage ——

9443. Would you kindly answer the question?—Yes, I am answering it, and you must allow me to say that I feel the responsibility of what I am saying, and therefore I cannot answer very quickly; I must think before I answer. In writing that passage we had in our minds a number of vivisectors generally, and also—allow me to finish—the two operators upon that occasion.

9444. But do you consider that what happened to the dark-coloured dog described in this chapter is an illustration of that "black art of producing the utmost agony while the gateways through which death could enter are carefully watched, but sometimes they are not clever enough"?—Yes, it is an illustration, because they sought to prevent death and failed.

9445. You tell us this about the dog as an example of the present treatment of animals?—It can be taken as an example.

9446. Do you mean it as an example?—Yes.

9447. You do?—Yes.

9448. I thought you told us before that you did not think that in this case the Act was not complied with?—No, not in this case; that was in relation to "A Grand Demonstration."

9449. Then you do mean to say that this was a case in which—Dr. Bayliss, was it?—Dr. Bayliss and Professor Starling.

9450. In which they were acting the part, in treating this dog as they did, of "artists in the black art of producing the utmost agony, while the gateways through which death could enter are carefully watched, but sometimes they are not clever enough"?—Yes, it may be taken like that.

9451. (*Mr. Ram.*) There is one other quotation that I want to ask you about. On page 152, under the heading of "Vivisection Bankrupt," you say this: "In the pain-tortured bodies, trembling under the sharp steel, bathing in their own blood, and in vain trying to tear the straps that fasten them to their crosses of agony, twitching under the piercing currents of artificial electricity, there is no trace left of the normal life in the nerves, in the blood, in the wonderful co-ordinated mechanism of the internal organs." Does that relate to animals which are not under any anæsthetics?—That chapter relates to vivisection generally.

9452. Do you intend in that paragraph to allude to animals under anæsthetics or to animals not under anæsthetics?—That paragraph should be read, as I pointed out in my former evidence, in connection with the paragraph that follows. The two paragraphs were written to show that vivisection is always performed under abnormal conditions—(1) pain and (2) anæsthesia.

9453. But do you believe that these "pain-tortured bodies," of which you speak in that paragraph, were under anæsthesia or not?—We do not speak of any special experiments. This is a general chapter on vivisection in England, vivisection throughout Europe, vivisection all over the world.

9454. That is what I am trying to get at. Do you present this as a fair and true representation of vivisection as practised in England to-day, that animals have their "pain-tortured bodies, trembling under the sharp steel, bathing in their own blood, and in vain trying to tear the straps that fasten them to their crosses of agony"? Is that a true representation of vivisection as practised in England to-day?—Yes, it is a true representation according to other descriptions of vivisectional experiments given in other chapters.

9455. Will you tell me of any experiments that you have seen in which any one or all of these different ingredients of pain were present?—I can refer you to "The Dog that Escaped," which we have just been discussing.

9456. Is this passage, then, intended to represent and sum up your opinion of what you yourself saw?—Not only of what we saw; that added to our general experience through reading of what vivisection is and the experience of others.

9457. And you believe that that goes on to-day in England?—Yes, I believe that that goes on to-day in England.

9458. And you believe that what you have described in this paragraph is a truthful representation of certain experiments which you saw?—Yes; and, added to that, my further experience and knowledge of vivisection.

9459. And you made no complaint to the Inspector or to the Home Office?—No, I was not there to make complaints to the Inspector; I wished to see how vivisection is practised in England under the Act of 1876. It is supposed to be more humane here than in other countries.

9460. And you represent that as a fair description of it?—Yes.

9461. (*Dr. Gaskell.*) I want to ask you a question with respect to what you have just told Mr. Ram about "The Dog that Escaped." Am I to understand that curare only was given and no anæsthetic?—According to our impression curare only was given.

9462. And there was no artificial respiration?—There was no artificial respiration according to our observation.

9463. The object of the demonstrator was to kill that dog, I presume?—No, because a slight dose of curare does not kill a dog. Dr. Bayliss said, "The animal is now slightly curarised."

9464. Have you ever seen or heard of anyone who was going to slightly curarise a dog who would not put in

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a tube for artificial respiration first?—I have frequently read that a slight dose of curare does not stop respiration.

9465. But would he not put in a tube first, on the possibility of respiration being stopped?—I think it is quite possible that that would not be done in the case of a very slight dose. Our contention here is that Dr. Bayliss meant to give a very slight dose and gave a stronger dose than he intended, and therefore respiration failed.

9466. You did not see that tube which was in the trachea?—No, we saw that there was no tube in the trachea.

9467. Then Dr. Thane's statement there is absolutely untrue, is it? He says, "Anæsthesia was kept up the whole time by pumping air, charged with a known amount of anæsthetic, through the lungs—*i.e.*, artificial respiration was carried on. Curare was administered, and the artificial respiration, the air still charged with anæsthetic, was kept on as before"?—Yes, that is a distinct contradiction.

9468. Contradiction of what?—Of our description in the "Shambles of Science."

9469. But is it a contradiction of fact?—Yes, it is a contradiction of fact.

9470. Then you say that that is a deliberate statement on the part of Mr. Thane and whoever it was he consulted—Mr. Bayliss—to deceive this Commission?—Mr. Bayliss may not have remembered, because no notes are kept of demonstration experiments, according to the evidence given at the trial.

9471. Mr. Thane goes on further to say: "The apparatus for artificial respiration broke down. The professor went to put it right. The experiment was continued, but the apparatus gave way again. Respiration was stopped, and the animal died of asphyxia." It is a very circumstantial account—invented, I presume, according to you, by Mr. Thane and Mr. Bayliss?—I can pass no opinion as to whether it is invented. I simply say that I do not agree with it.

9472. I will leave it at that. You have quoted Professor Claude Bernard with respect to the action of morphia, and I presume that you have read various papers of his, have you not?—I have read a good deal of his work.

9473. He says, in talking of the action of morphia, "However, the sensibility persists, for if we pinch the animal it moves and cries"?—Yes.

9474. That is what you saw?—Yes?

9475. And from that you conclude that Claude Bernard considered that the animal felt pain?—Yes.

9476. But he goes on to say: "But these are only movements due to a very obscure kind of perception. In reality he does not dream of getting into a rage, or of defending himself; his intellectual faculties are completely in abeyance"?—Yes, that is no contradiction.

9477. That does not look, does it, as though he considered that the animals suffered pain?—As a supplement to that I put my quotation from Claude Bernard on the effect of morphia, where he says that the animal "feels the pain but has lost the idea of self-defence."

9478. In this case he is defining what he means by feeling pain—that is, movements are due to a very obscure kind of perception?—But he says distinctly that it "feels the pain." He cannot say anything clearer.

9479. This is a further statement of Claude Bernard in his "Lessons on Anæsthetics and on Asphyxia"?—That is the book I have brought with me to-day, and that book has a great many statements which I can show you proving that Claude Bernard did not consider morphia an anæsthetic for dogs in any sense. I have carefully read through this book before coming to-day.

9480. On page 219 he says distinctly: "Morphin sends to sleep pain which has its seat in the sensorium"?—He says, on page 219, that he injected a young dog with 5 c.c.'s of a solution of morphia hydrochlorate, and that resulted in—yes, you are right—that is an exception.

9481. An exception?—Yes, that is an exceptional case, and entirely different to that given on p. 222, and on other pages; but even then he shows that "Si l'on reveille le chien ou seulement si on le pince" physiological functions at once become more normal.

9482. I did not understand it to be an exception. Again, as illustrating the action of morphine, you will find on page 235: "After the injection of 1-2 centigrammes of hydro-chlorate of morphia into a man, the first effect of inhalation of chloroform given in the usual manner is to produce a state of analgesia, with retention of the intelligence, the sense and the power of voluntary movement"?—Yes, but I do not propose to deal at all with the effect of morphia on man. My evidence last time was rather to show that dogs are extraordinarily insusceptible to morphia, and I have here another quotation from Claude Bernard.

9483. I wanted to ask you whether you did not realise that Claude Bernard says there that the action of morphia with a very slight dose of chloroform is to remove all pain although consciousness persists and the sense and the power of voluntary movement?—Neither am I dealing at the present moment with morphia and chloroform; that makes all the difference in the world.

9484. But the chloroform is so slight, according to Claude Bernard, that consciousness still persists with retention of the intelligence, the sense and the power of voluntary movement, but that the action of morphia *plus* a slight amount of chloroform is to remove all pain?—Then my answer is, that if that be the case in man, man is far more susceptible to morphia than a dog would be.

9485. But I was simply stating that Claude Bernard's argument is that morphia removes pain?—I cannot accept that statement of what morphia does with regard to man as being equivalent to what it does with regard to a dog.

9486. Do you not believe—it is not a question of belief; do you not know that in these cases of anæsthetics being given, the sensation of pain is one of the first things to go?—That is not always the case. The sensation of pain may persist. I have quoted Professor Flint to show his opinion upon that point, and I believe that is very often the experience of people who have been under anæsthetics.

9487. Is it not a very common experience to find that pain does go even although apparently consciousness still exists?—It is a common experience; but, on the other hand, it is a common experience that pain does not go.

9488. Have you ever had laughing gas?—I have; I was just describing that when you came in this morning. I had laughing gas once for the purpose of extracting a number of pieces of a tooth which were left in my jaw, and although I was unconscious to a certain extent, I felt the pain of the operation all the time; though when I came to perfectly I very soon forgot all about it.

9489. And then when you were asked the question whether you felt any pain you said "No"?—I did not. I said that I had felt the pain.

9490. Is it not often the case that patients under a slight anæsthetic like laughing gas, not only cry out but make violent movements when the tooth is drawn, and afterwards assert that they felt no pain at all during the operation?—That is often the case; but it is equally often the case that people do feel pain and yet are in a kind of subdued state; as, for instance, when slight doses of chloroform are given to women in cases of labour. I know of one instance of a woman who felt pain all the time. She lay in a state of subdued hushed consciousness, but was afterwards able to state definitely that she felt all the pain, though she was unable to give any expression to it. That is a personal acquaintance of mine. There are such cases.

9491. That meant that a very little more chloroform might have been given with advantage, did it not?—She could not ask for more, because the people round her thought that she was unconscious and talked of her as being unconscious, and she heard them talking of her as being unconscious.

9492. (*Dr. Wilson.*) She was in a cataleptic condition?—She was in a cataleptic state.

9493. (*Dr. Gaskell.*) Do you not notice that Claude Bernard speaks of the persistence of sensibility in the case of morphia; it is the persistence of sensibility that he speaks of, and then later on he defines what he means by sensibility?—Yes.

9494. And he goes on to point out the difference in the use of the term sensibility by philosophers and physiologists?—Yes.

9495. He says that "according to the former sensi-

bility is the sum total of the psychical reactions provoked by external stimuli. For the latter, for us, it is the sum total of the physiological reactions of every kind provoked by external stimuli"?—Yes.

9496. Do you not understand by that that he does not consider that the retention of sensibility after morphia means necessarily the retention of consciousness?—I think, from reading Claude Bernard's work on this subject carefully, one comes to the conclusion that he does mean by his *sensibilité* what we mean by sensitiveness. I remember I was once attacked in a book by a Swedish vivisector on this very subject, and in my answer I pointed out the various meanings attached in French to the expressions *sensibilité, irritabilité sensitive*, or *excitabilité sensitive;* and there is no doubt that by the first Claude Bernard means the persistence of conscious sensation. For instance, here on page 222 he says: "Though in a state of torpor the animal is excitable and sensitive. But if the nerves have not lost their *sensibilité* they are at least dulled." And he adds that the animal is sensitive to pain if it be pinched or subjected to stimulation. Besides, there is the distinct phrase "signs of pain"—*les signes de la douleur*. What could be clearer?

9497. But in this other case he says "*sensibilité.*" "However the sensibility persists, for if we pinch the animal it moves and cries." And later he states that "for the physiologist in addition to the conscious sensibility there exists also an unconscious sensibility"?—But if he has mentioned *les signes de la douleur*, there can be no doubt about it.

9498. Afterwards he distinctly asserts that morphia sends to sleep pain," as I showed you?—I do not think it has that meaning, for he has previously said (p. 211): "L'action de la morphine diffère tout à fait de l'anesthésie."

9499. Now, I would like to go from Claude Bernard to your own evidence?—My printed evidence, or the book?

9500. The evidence in the book. Will you turn to your chapter headed "An experiment that 'is not supposed to be useful'"?—Yes.

9501. You tell us, "The animal is now deeply under morphia"—and then throughout the whole of the experiment you further speak of it as "absolutely still." Then you say, "We can be almost certain that the dog has been also curarised, because it is absolutely still with the stillness that is characteristic of the 'hellish oorali'"?—Yes.

9502. And therefore all we know from this statement of yours is that the dog was under morphia, and in your opinion had curare as well?—Yes.

9503. But you will see from Dr. Thane's evidence that there was no curare?—Yes, I have seen that he says so.

9504. Then does not your evidence show that morphia alone kept that animal still?—Yes. All the evidence that I have given on morphia has gone to show that morphia is a drug which has the power of making an animal lie very still—not a cat, but a dog—and that the animal may be sensitive all the time, and that it would respond to painful stimuli.

9505. But these were painful stimuli and it lay still—it did not respond?—Yes.

9506. Therefore, according to your own evidence, morphia was very distinctly an anæsthetic in that case?—According to my evidence, if we were certain that the dog had not also been curarised it would show that it had lost its power of self defence.

9507. But you find now that that is not so?—I hear that Mr. Thane mentions that it is not so; I do not find that it is not so.

9508. Then, may I ask, do you still stick to it that the animal was also curarised?—I stick to all the things that we have written.

9509. It was under curare?—Certainly.

9510. And this is not evidence of the anæsthetic properties of morphia?—No.

9511. This dog had curare?—Yes, because it had every sign of having curare.

9512. Although we are told definitely and absolutely that no curare was given?—Yes.

9513. Take another case, "Scarcely any Anæsthetic"?—May I add that our reason, and a very strong reason, for concluding this, is Dr. Brodie's own statement in his book, where he distinctly states that he prefers curare because it is essential to the only satisfactory method.

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9514. Which are you referring to now?—"The only completely satisfactory method."

4514A. I was speaking of "Scarcely any anæsthetic"?—But the physiologist was Dr. Brodie on both occasions, and he has written in his book for students very clearly on his preference for curare.

9515. "Not supposed to be useful" was the one. Was that Dr. Brodie?—Yes.

9516. In this case "Scarcely any Anæsthetic," at page 49 in the old edition, you say: "'The animal is so profoundly under morphia we have scarcely had to give it any anæsthetic at all,' says the lecturer"?—Yes, those were the words of Dr. Brodie.

9517. I understand that to imply that the main narcotic given there was morphia?—Yes, according to Dr. Brodie's own words; he has not denied it.

9518. No, I do not want to deny it?—I believe he has not denied it through Mr. Thane.

9519. I only want to point out to you that you say on the previous page: "The strings which hold the dog's legs are loosened. The assistant demonstrator lifts up first the hind legs and then the body several times, whilst the lecturer holds down the animal's head." Would not the dog have made any movement, if it had not been anæsthetised, under those circumstances? The limbs are free, the dog is capable of moving them as much as it likes; it is pulled up and down in this way; if morphia was not an anæsthetic would not the dog have moved?—I have never denied that morphia has the power of keeping a dog quiet. Morphia can keep a dog quiet.

9520. What, when it is in pain?—If it were in a state of narcosis, it is quite possible that it would be quiet for that moment when the legs were loosened from the board.

9521. But it was in pain all the time, according to you. Do you mean to say that it would not demonstrate its pain if it was conscious and was capable of doing so, by the legs being moved?—How could it demonstrate its pain by moving the legs when the assistant demonstrator held both hind legs? How could it struggle with its hind legs when he held them both for the moment, if it had wanted to do so? If I take up a dog which is fastened to an operating table and lift up its hind legs and hold them together with my hands, that dog cannot struggle.

9522. Let us take "Long Experiments." This again is a case of morphia and a little chloroform?—Yes, according to Professor Halliburton.

9523. And you say throughout that the animal was quiet. It is not an instance that you give of any evidence of pain?—No, we do not give that as an instance of evidence of pain; we could but see that the animal was quiet, and we honestly put it down and recorded it.

9524. And yet what was mainly given was morphia?—I do not say that the animal did not suffer pain. I do not know. I could only record that it was quiet.

9525. Then I understand that in these three cases of morphia which I have quoted to you, although the animal was in pain, it remained remarkably quiet as though it was properly anæsthetised, and did not show any sign of that pain by struggling and so on?—I do not know whether that is so in the three cases you have quoted.

9526. I understand from your own statement that in these three cases the animals were very quiet?—Then it must be so. I cannot at this moment recollect which three they were. If that be so, it is just because morphia does make a dog quiet. In two of the experiments I believe chloroform was given as well.

9527. I have a question to ask you on "The Only Completely Satisfactory Method." Here again your remark (on page 134) is: "The director of the laboratories is anxious to make the most of the dog that is so nice and quiet to experiment upon"?—Yes, the director was Dr. Waller. He is the director of the laboratory.

9528. Do you mean to say that that quietness was not due to properly given anæsthetics?—We inferred that it was due to curare, because Dr. Brodie was the lecturer, and he has expressed his preference for curare. He said nothing about having given an anæsthetic, and there was no smell.

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9529. Here again I suppose you would not agree with Dr. Thane's statement: "The assertion that curare has been used is false. The animal was anæsthetised with chloroform throughout, under the superintendence of the director of the laboratory"?—The director of the laboratory did not superintend any giving of chloroform in our presence during the hour and a half, and if you give chloroform you have to give every five minutes, or more frequently. I do not think Dr. Waller would say that he did do that, because that I should think would be impossible.

9530. Dr. Thane goes on to say: "'Artificial respiration is being kept up'; quite true; but the writer omits to state that the air pumped in was passed through a bottle containing, and so became charged with, the anæsthetic"?—That is his statement.

9531. Is not that a possible method of giving chloroform?—Yes, and we stated that artificial respiration was kept up.

9532. That you omitted to see, apparently?—But no chloroform was given in our opinion.

9533. Because you could not smell it?—We could not see any bottle.

9534. No, because the bottle was in another room?—Nor could we smell it.

9535. It is the same as in the case on which the action was. There, again, because you did not see the chloroform you stated again and again that there was no anæsthetic given. In each of those cases anæsthetics are given in the same way, with chloroform, the air rising through it?—But on other occasions, in the University of London, anæsthetics were given in the usual way through dropping the anæsthetic on to a cloth, and then everybody could smell it. On this occasion there was no such indication.

9536. And you made a mistake?—I do not acknowledge that I made a mistake. I saw the pump; we describe how we saw the pump and heard the hissing sound, but no bottle.

9537. If you do not acknowledge that you made a mistake, I say no more?—Certainly, I do not.

9538. There is one more question that I should like to put to you on this book. You speak in one sentence, I think it is in that same "A Grand Demonstration," on page 144, of the rabbit moving its paws: "When the table with the operation board is now lifted up, and the rabbit carried to the place in the middle where the dog had been, the little animal again stretches up its fore-paws with claws thrust out"?—Yes.

9539. Are you aware that in physiological laboratories that movement of extension of the paws is a sign of asphyxia?—There was no asphyxia then. The animal was, according to Mr. Thane, asphyxiated later on in another room.

9540. Will not asphyxia take place if chloroform is pushed too much?—It was not pushed too much, because no chloroform was given. It was ether.

9541. Ether is still more restricted?—According to Dr. Thane, and, I think, it is admitted, the animal had to be taken out to be killed because it came out of the anæsthesia. There was no question of asphyxia at all, neither according to our account nor Dr. Thane's.

9542. Do not you know that when a rabbit is under an anæsthetic, either chloroform or ether or A.C.E. mixture, as soon as the experimenter sees that movement of the paws he gives less chloroform, because he knows that he is overdoing it?—That is quite possible; but that was not the case here.

9543. It is the beginning of asphyxia in consequence of an overdose of chloroform?—It would entirely contradict Mr. Thane if what you say is true; because he acknowledges, I believe, in his pamphlet that the rabbit was taken out to be given more anæsthetic.

9544. But that is afterwards?—He does not say that there were not violent struggles and movements of the rabbit. He says that they were "greatly exaggerated" by us.

9545. I was only referring to that movement of the paws. I wanted to know whether you recognised that movement of the paws does not necessarily mean returning consciousness?—Yes; but I also contend that upon this occasion it could not mean asphyxia or any danger from too much anæsthetic, because, according to Mr. Thane, the animal was sent out to be killed by an overdose of ether. If it had already had an overdose of ether, it would not have been sent out to be killed.

9546. (*Dr. Gaskell.*) You spoke of physiologists using light and deep anæsthesia?—Yes.

9547. Are you acquainted with Hooper's experiments on the recurrent laryngeal?—I am not.

9548. Do you not know that with ordinary anæsthesia, when that nerve is stimulated you get closing of the glottis, and that when anæsthesia is pushed deeply you get opening of the glottis. You have not heard of that?—I am not acquainted with those special experiments. I do not know to which you refer.

9549. I only wanted to know whether you were aware that the peripheral nerve alters its effect according to the condition of anæsthesia. That the recurrent laryngeal opens the glottis when the anæsthesia is deep?—Of that, I think, I am generally aware—not of those special experiments—because that is one of the reasons why I have stated that physiologists prefer light anæsthesia, as deep anæsthesia sometimes interferes.

9550. This is the peripheral nerve?—Yes.

9551. That has nothing to do with the central nervous system?—Do you mean when the nerve is cut off?

9552. I mean the nerve cut off and the muscles stimulated by excitation of the peripheral end?—Yes.

9553. You do not also know of Bowditch's experiments upon the action of anæsthesia upon the sciatic of the frog?—No; of course I am not a vivisector, and even a vivisector cannot be acquainted with all experiments.

9554. I thought you had studied a certain amount of physiology?—I have studied a certain amount, but not in the way that a vivisector would.

9555. There again the evidence is that stimulation of the nerve will cause extension when the anæsthesia is slight, and the opposite effect when the anæsthesia is deeper. And do you know the difference between pressor and depressor effects upon the stimulation of an afferent nerve, according to whether the anæsthesia is deep or not?—Yes, I have some knowledge of that.

9556. And also the difference in the effect upon the respiratory centres?—Yes.

9557. How inspiration takes place in one case and expiration in the other; you know that?—I have some knowledge of that, too.

9558. Do you not see then that the reason why physiologists make a distinction between light and deep anæsthesia is on account of the reversal of effects which take place in nervous actions?—Certainly, I have seen that all the time, though, of course, not only from the vivisector's point of view.

9559. It has nothing to do with the question of pain?—It has to do with the question of pain. Thus, for instance, Professor Starling in one place refers in his book "Elements of Human Physiology," to incomplete anæsthesia, and speaks of "Pain, as evinced by the movements of an animal not fully under the influence of an anæsthetic." That would prove, in my opinion, that incomplete anæsthesia has to do with pain.

9560. But I was using your own terms, light anæsthesia and deep anæsthesia—not incomplete anæsthesia?—That is the same thing, light or incomplete, so far as I understand.

9561. That is exactly what I want to get you to see. It is not the same thing?—I have never heard that before.

9562. Do not you know that the reason why a distinction is drawn between deep anæsthesia and light anæsthesia is to emphasise the deep anæsthesia because of its reversal of the effect?—I quite see that.

9563. That is all I want to bring out with reference to that. Now, might I ask you with regard to your theory that the spinal cord of the frog is capable of feeling pain. I think I am right in saying that you rather hold that view?—Yes.

9564. I suppose I shall not be wrong in understanding that you hold that view on account of the apparently purposive activities which take place on stimulation of a nerve when the spinal cord is isolated?—In holding that view I am in very distinguished company.

9565. I only wanted to know whether that is your reason for holding it?—I have many reasons, but I cannot answer you with a single sentence. I could give a considerable amount of evidence on that point if I were allowed

9566. I thought you said that it was merely because of the purposive activities?—That is one of the reasons. I have more; I have a great deal of knowledge of these experiments.

9567. I was only asking you that because I wanted to know whether you would also hold the same view with regard to the spinal cord of a dog?—I do.

9568. That there purposive reactions take place showing that there is pain?—Yes, and I based my statement last time——

9569. Do you also hold the same view with regard to a monkey?—Certainly.

9570. And do you hold the same with respect to man?—No.

9571. You draw a distinction between a monkey and man?—Yes, I do. I could not offer an opinion about the anthropoid ape, but I do think that a monkey is sufficiently low for the spinal cord to be exceedingly highly developed in comparison with the cerebral hemispheres.

9572. Then according to you man's spinal cord is not capable of feeling pain, but you think that a monkey's is; there is that difference in the purposive activities of the spinal cord of a man and of a monkey?—Yes, I think there is a difference. I cannot be absolutely dogmatic on that point.

9573. If consciousness of pain is established because of the purposive activities upon stimulation, may I ask why you confine yourself to the spinal cord; is there not another system in the body, the sympathetic system?—Yes, but I do not think that the sympathetic system plays the same part as the spinal cord.

9574. Is there not any evidence of purposive reflexes from the sympathetic system?—Yes, there is. I do not think that any physiologist has ever put forward that the sympathetic system would be capable of any kind of consciousness; but a great number of physiologists have put forward that the spinal cord would be capable of a kind of hushed consciousness. Thus Huxley himself in 1870 read a paper entitled "Has the frog a soul, and, if so, of what nature is that soul," based entirely on these strange phenomena exhibited by the spinal cord of the frog.

9575. Are you not aware that the sympathetic system used to be looked upon as entirely independent of the central nervous system, and that they went so far as to speak of the coeliac ganglion as the brain and the soul of that system. You did not know that?—I think there has been a great deal of theorizing with regard to the solar plexus generally, and is still; I am quite aware of that.

9576. I thought you said that no physiologist had ever put forward the view that the sympathetic system was independent or capable of reacting like the central nervous system. That was the view that was always held?—I am not aware of any physiologist who would be acknowledged as a physiologist at the present day, who has put forward that theory.

9577. One great physiologist I know of was a man named Bichat?—Yes?

9578. Did he not put forward that view?—Not that I am aware of.

9579. Do you know the experiments that have lately been made on Auerbach's plexus in the intestines?—I am acquainted with them.

9580. There is evidence in those experiments that the ordinary peristaltic action of the intestine is due to nervous stimulation, owing to Aeurbach's plexus?—Yes.

9581. Would you not call that of the same character as the purposive activities that you can get from the spinal cord of a frog?—No, I would not, because they would be of an entirely different nature. The purposive activities which you get from the spinal cord of a frog, and of animals higher in the scale, present certain, what I might call mental or psychic phenomena, which I am sure would not be presented by any plexus of any kind.

9582. The one is the ordinary movement of the intestines and the other is the ordinary movement in the striated muscle; I do not see quite where the distinction lies?—The distinction lies in this, that the theories I hold with regard to the spinal cord, spinal automatism, and its power of independent action, are based not on mere movement, but on the deliberate choice in action, and on signs of consciousness which have been again and again manifested by such animals. I need only refer to Steiner's well-known experiments on frogs *contra* Goltz and to Pflüger's experiments.

9583. But upon this question of peristalsis in the intestine, there are two effects occurring: one is the contraction of one part of the intestine, and the other is inhibition of another part?—Yes.

9584. That is as purposive as the purposive movements of the muscles. It is not a simple contraction of the muscles but a purposive movement?—You are now using the word purposive in the wider sense in which it can be applied to every function of the body; there is nothing in the body which does not have a purpose; but I am using the word purposive in the sense of conscious purpose in a living creature. It is quite a different thing.

9585. I do not understand where the word conscious comes in.

9586. (*Mr. Tomkinson.*) May I take it that your general position towards the question is this: that while you are strenuously opposed to every kind of cruelty, and would do your best to minimise it, the special branch that you have taken upon yourself is the branch of this anti-vivisection?—That is so, because I consider it the worst, perhaps, of cruelties, though there are others that can be compared with it.

9587. And in regard to its practice you are, in the first place, sceptical about the possibility of total anæsthesia?—Entirely sceptical.

9588. And even if you were not so, you would think it inevitable that, either from carelessness, or omission, or indifference, at times great cruelty would be inflicted by operations?—Yes, I think so.

9589. Those are your general grounds of opposition to the system altogether?—Yes.

9590. You are not altogether opposed, if you could not obtain a complete prohibition of it, to taking instalments?—No, I am not opposed to it, but I should continue to demand abolition.

9591. In the direction of its limitation to certain animals?—No. If I had no choice I should, of course, gratefully accept the limitation of the practice.

9592. And you would also support further restrictions in regard to the prohibition of morphia and curare as anæsthetics?—I should, as preliminary measures.

9593. Did I rightly understand that you have witnessed yourself personally many operations?—Yes, I have witnessed a certain number of operations on living animals.

9594. And did you say that in two cases only you thought that complete anæsthesia was established and maintained throughout the operations; there is some confusion about the "Two cases only"?—Yes.

9595. (*Chairman.*) Does that mean two of those in the book or two of the 50 you spoke of?—All the experiments that we saw are not described, but the severer ones are described in this book.

9596. But I only meant that Mr. Tomkinson's question seemed to assume that you were speaking of only two out of the 50 operations which you had seen properly anæsthetised?—It would be an exaggeration to say two out of the 50, because during many of those 50 there was nothing which might be called real vivisection. But among those described in this book there are only two. That is the answer.

9597. I understood that was what you did say. I only wanted to make it clear.

9598. (*Mr. Tomkinson.*) You have no suggestions to make as to increase of inspection?—No, I do not believe in increase of inspection.

9599. You think that inspection is a mere formality?—As inspection is practised now it has been of no avail at all, I think, either in checking the increase of vivisection or in enforcing compliance with the Act.

9600. On the ground of its infrequency?—Both on the ground of its infrequency and other matters associated with it. I do not think the Inspectors (but perhaps that is outside the inquiry) are men we have confidence in as Inspectors.

9601. Did you notice what Professor Starling said with regard to experiments in the way of starvation, of depriving animals of food?—Yes.

9602. Did you notice that he said that he thought the mere fact of going without food was not a painful experience?—Yes, I noticed that with some surprise.

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9603. And instanced the Italian faster Succi?—Yes.

9604. Who was supposed to have fasted for 40 days?—Yes.

9605. But who was afterwards found to have received something?—Was he? I did not know that.

9606. In the way of a phial, I think he did. What have you to say with regard to that? It is rather a novel idea that starvation could take place without cruelty and pain?—I should certainly consider starvation generally, forced starvation, painful. Of course a professional faster might by power of will reduce that pain to a minimum; but I saw the Italian faster Sacco come out of his fast at Hengler's Circus at the very moment he broke it, and I think he was in a very weak and bad state.

9607. Undoubtedly he was?—But I do not think that case can be at all applied to the artificial starvation of an animal that is craving for food and has no will power to put against the pain. Professor Starling gives several examples in his book of animals starved for experiments, and especially one case where an animal is starved and then made to do hard work afterwards, drawing a milk cart, which certainly would be cruel. I do not know whether he would declare that to be a painless procedure. It is at page 491 of his book. He says: "Much more efficacious than starvation in causing disappearance of the hepatic glycogen is muscular work. If a dog be starved for a day and be then made to drag a heavy milk cart about all the next day" (that is rather hard lines, I think), "there will be found only the merest traces of glycogen in its liver." A dog is to be starved one day, and then made to drag about a heavy milk cart all the next day.

9608. You described how you witnessed some operations which you thought extremely cruel on account of the imperfect anæsthesia, and you were asked whether it had occurred to you to make any representation, and you said it had not. You did not think of laying an information?—We were not there for that purpose.

9609. Were you there under any obligation of loyalty or secrecy not to do that?—No, we were there entirely as independent students for the purpose of seeing what actually took place, and we were there for the purpose of seeing how experiments are performed under the present Act. It was not for us to remonstrate with the vivisector or to make any kind of official declaration, because the result would probably have been that we should have been turned out, and the experiments would have gone on just the same. We were there to see how they were carried out without any interference from us, and it cost us very much not to interfere many times.

9610. Do you think it would be a good thing if there was a freer right of entry, by people of accredited position, to go in and pay surprise visits at times and witness operations?—I should think so, provided that those people were chosen from among those who have humane views and not among those who are in favour of vivisection, or who only advocate its scientific utility and care nothing for its moral value.

9611. I am speaking of ordinary laymen without marked opinions upon either side?—Yes, I should think that would be useful.

9612. You are aware, of course, that in foreign countries the whole practice is infinitely less humane than in this country?—I am aware of that.

9613. That is to say, there are unspeakable cruelties and horrors in France and the United States and different places?—Yes.

9614. Therefore I suppose you admit the possibility, if the practice was totally prohibited here, of driving it more abroad where they are more inhuman, and probably the sum of animal suffering might not be diminished?—That is a very difficult question to answer. You mean that English physiologists would emigrate wholesale to foreign countries?

9615. No, I do not say that; I only meant that there might be more experimentation abroad by people there already?—I do not think that. The only danger would be of a wholesale emigration of all English vivisectors to foreign countries; otherwise I think it would diminish the total sum of animal suffering.

9616. But the example of this country?—Yes.

9617. You would like this country to set an example?—Yes, I think the example would do a great deal of good.

9618. I think you said you were a vegetarian?—Yes, I am.

9619. You no doubt then appreciate very deeply a good deal of the cruelty in our slaughter-houses?—Yes, and I am very anxious that such avoidable cruelty should be prohibited by law.

9620. And also in many other ways; for instance, the trapping of wild animals?—Yes.

9621. Some people hold it to be an absolute necessity to prevent a plague of rabbits; thousands of rabbits are trapped in a very cruel manner?—I think it might be possible to destroy rabbits in a humane way, at any rate in a much more humane way than is done at present.

9622. (*Dr. Wilson.*) I think you said at a previous sitting that you had not witnessed any experiments at all on the Continent before you came to this country—in Sweden?—No, I had not witnessed any experiments in Sweden. I said I had seen the results of many kinds of experiments at the Pasteur Institute, but that I had not witnessed any actual experiments.

9623. But you held strong views on this question before you came to this country?—Yes, we did.

9624. Was that from your reading as to how experiments were carried on in Sweden and other Continental countries?—Partly from seeing the Pasteur Institute and partly from reading.

9625. And you had preconceived views as to how vivisection was carried out in this country, I take it?—I do not think we had very preconceived views as to how it was carried out in this country in particular; in fact, we expected it to be much more humane than it was, because of the Act of 1876—because of England being one of the few countries where there is any restriction. There is some restriction in Denmark and some in Norway.

9626. And when you entered at the London School of Medicine for Women, were there any experiments carried on in the physiological class there?—No, I believe not. The place is not registered for experimentation.

9627. They are not allowed there?—No. I believe Professor Cushny made a mistake in his evidence, no doubt unintentionally, in saying that the place was registered.

9628. Then did you join the advanced course of lectures on physiology at your own request, or were you invited to attend?—No, we joined them at our own wish. We sought permission in the usual way. Any medical student, any student at the London School of Medicine for Women, has the right to apply for permission to attend these courses, and we did that in the usual way.

9629. You had no intention then of writing about what you had witnessed?—No, we had no intention at the time of writing.

9630. Now, just a few questions with regard to this book, the "Shambles of Science." I take it, from all you have said, that you honestly and truthfully maintain that what you state in your book represents your impressions of what you saw, and of what you listened to?—Yes.

9631. And even as regards that experiment in that chapter, which was eliminated from the first edition, you still adhere to your views there?—I still adhere to our views there.

9632. Perhaps this is not a question which I should put, but with regard to what I may call the somewhat cynical headings of the various chapters, and somewhat scornful and sarcastic remarks that are made throughout the text, you used those, I take it, to emphasise in the strongest fashion your honest convictions as to what you saw and listened to?—The book was written under a great deal of indignation, and the scorn and sarcasm probably came out of that.

9633. But when you used the title "Fun," you had quite another feeling, which you wished to express. You did not mean that to apply to the students, did you?—Yes, in a certain way. The title was suggested by the, in our opinion, unnecessary hilarity and joking which went on whilst the dog was being experimented on.

9634. Some questions have been asked about the "Struggling Cat"; you said there was no tube in the trachea?—Yes.

9635. Dr. Thane, I think, maintains that there was

artificial respiration carried out?—No, Dr. Thane acknowledges that there was no tube in the trachea on that occasion.

9636. But does he state that there was artificial respiration?—No, he states that it is as we say, that there was natural respiration in the case of the struggling cat.

9637. And, of course, there was no tube inserted into the mouth?—No.

9638. So that anæsthesia could only have been administered in the ordinary way, by a mask?—Or by dropping it on a cloth.

9639. And you did not see anything of that?—Nothing of the kind.

9640. And the cat was muzzled?—The cat was muzzled in the usual way, and tied down on its back.

9641. And it struggled?—And it struggled, and the movements are not denied.

9642. I think all these points were brought out, but I wished to make this clear about the respiration?—In that case it struck us that as there was no artificial respiration, the ordinary apparatus for anæsthetising ought to have been at work. It is not suggested that there was artificial respiration, with a pump blowing in the anæsthetic, and therefore I may well ask who is supposed to have administered the anæsthetic on that occasion.

9643. You did not see anyone?—No.

9644. You did not see any anæsthetic administered during the whole period of the demonstration?—No.

9645. (*Dr. Gaskell.*) May I ask, did you notice a bottle on the table?—No, we did not. And may I add that even if we had noticed a bottle upon the table, the bottle could not administer the anæsthetic; you would have to have somebody to deal with the bottle.

9646. Then you deny the entire statement that "The anæsthetic was being given from a Woulff's bottle, with natural respiration. The lecturer left the theatre to get his notes, and the cat began to move; a student fetched him. He came at once, and gave more anæsthetic; this required only the turning of a tap"?—We cannot deny that the anæsthetic may have been given through a Woulff's bottle before we were there, but we deny that it was being done in our presence.

9647. You deny the turning of a tap?—Yes.

9648. "Possibly the reporter mistook the turning of the tap for tightening the strings"; that again is not true?—No.

9649. You assert that Mr. Thane and Mr. Bayliss have made misstatements there?—Yes.

9650. Then that is another untrue statement.

9651. (*Dr. Wilson.*) Now with regard to the marmot, your impression was that the spinal cord had been divided?—Yes.

9652. But, of course, we are all liable to make mistakes. Can you not imagine that that marmot was suffering at the time from paraplegia, from some cause or other; animals often do, as you know; is not that possible?—I think, as Mr. Thane acknowledges, that the animal was paralysed in its hinder parts and looked like one that had had its spinal cord divided; I cannot go further than to say that that was our impression. I have said that before, and that we have not seen it after death, and had no opportunity of verifying whether it was so or not. I can only say that if it had not had its spinal cord divided, and if I had been allowed to read my statement, which I was not, I should have been able to put in some evidence to show why it was such an abnormal marmot compared with other marmots; because it must either have been in a state of hibernation, and then it could not have bitten the vivisector; or it was in the awakening stage, when it is well known that a marmot begins to shiver, and the temperature rises exceedingly rapidly, as much as 12 degrees or more in one hour, and that when that shivering has once begun nothing can stop the marmot from becoming absolutely awake, and then from running about. This marmot lay still, and was carried about, shown to the class in this peculiar state of what looked to us as paralysis, and was neither in a state of hibernation nor in a state of awakening, nor in a state of being really awake. According to the usual knowledge that one has of marmots, neither of those three states was represented.

9653. But do you not think it is quite possible that as this marmot was retained to demonstrate this extraordinary rise of temperature under certain conditions, there would not have been any object in dividing the spinal cord?—The spinal cord of marmots is very often divided. There is a great work written on the marmot alone by a well-known French physiologist, Professor Dubois, of Lyons, which is the best known monograph on the marmot, and a considerable number of his experiments are concerned with dividing the spinal cord of the marmot in various places. Under the circumstances, therefore, seeing the state of the marmot and having this knowledge of experiments on marmots, it was the most natural and in fact the only conclusion we could draw.

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9654. I will not say the only conclusion, but I admit that it might have been the natural conclusion under the circumstances. But you see Dr. Thane has made a post-mortem of the marmot, and do you not think that, after his positive statement that he found no evidence whatever of the division of the spinal cord, you must really admit that your impression must have been wrong?—I gave as my reason for not admitting that, that Professor Thane made a seriously inaccurate statement almost in the same breath as he corrects us, and I cannot feel justified in accepting his statement two lines lower down when he has just made himself guilty of such a great inaccuracy as he has done.

9655. Can you not admit this much, then—that Professor Thane may have been wrong in his inference after making the post-mortem; but surely you cannot say that he did not honestly believe, when he made that post-mortem examination of the marmot, that the cord had not been divided?—But I do not think I have said that.

9656. Yes?—I do not think I have.

9657. That was my inference—that you doubted his truthfulness, not the accuracy of his statement?—I do not think I have said that he did not honestly believe that the spinal cord had not been divided when he made the post-mortem. But I simply begged to be excused from accepting this one statement as accurate when he has just made an inaccurate one.

9658. I wanted to make that a little more clear if possible. Now about anæsthesia, you contend absolutely that as a rule light anæsthesia must be more or less incomplete anæsthesia?—I do.

9659. And in anæsthetising animals so as to ensure complete anæsthesia you would insist upon the same agents being used and the same care being taken as in anæsthetising human beings for operations?—Certainly, if the anæsthesia is to be complete or reliable.

9660. And from all you have read and seen of experiments, you believe that morphia and urethane and these other narcotics are unreliable as so-called anæsthetics?—I do, and I believe I am supported in that by veterinary surgeons of much experience.

9661. And as to the struggle of animals under operations, you have seen operations, as you have stated, on human beings under anæsthetics. Whenever a patient begins to struggle under an anæsthetic, does or does not the surgeon take that as an index or indication that the patient is coming out of the anæsthetic?—He does, and measures are taken at once to ensure deep anæsthesia by adding to the anæsthetic given.

9662. So that any movements of the limbs on the operating table, whether they may be more or less purposive or not, or reflex, are indications that the anæsthesia is incomplete, and they at once take steps to give more?—Certainly; that is always so.

9663. You have seen animals operated on?—I have seen animals operated on by veterinary surgeons, and that has been the case.

9664. With animals the same care is taken?—Yes; I was referring to an operation by Professor Hobday under those conditions.

9665. A question was asked you about permission being granted to outsiders, as it were, to witness some of these experiments. Do you think that if medical men belonging to one or other of the Anti-Vivisection Societies were allowed to be present, that would be a satisfaction to the Societies? Supposing these men were accredited by the Secretary of State, after being nominated by their Societies, to be present at any operations on animals, do you think that would give more satisfaction to the Societies?—I certainly think

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that would give a good deal of satisfaction and more guarantee that those Societies have at present.

9666. If they were medical men then there could be no objection to their being present?—No, I do not think that physiologists would have any objection; at any rate they would object less than to lay people.

9667. An ordinary layman, of course, could not judge so fairly as one who had some knowledge of medicine?—No. May I add that an Association of Medical Anti-Vivisectors is being formed called the International Medical Anti-Vivisection Association; there are already some 200 members.

9668. They are not all anti-vivisectionists, are they?—Yes, they are all anti-vivisectionists.

9669. (*Colonel Lockwood.*) In London, is this?—In England and in France. There are about 100 members in England and 100 in France; it is only at its beginning.

9670. (*Dr. Gaskell.*) With respect to that statement of Dr. Thane's, which you characterise as inaccurate, on the question of the marmot, if Dr. Thane intended to imply that no cutting experiment had been made upon the marmot before, would that have been inaccurate?—He did not say that.

9671. If he had intended to imply that, would it have been inaccurate?—It would not have been inaccurate, but he said that no experiment had been made save the taking of the temperature.

9672. My question was as to a cutting experiment, cutting the spinal cord?—Yes, but he did not say that.

9673. But that might have been in his mind?—Then he did not express himself correctly.

9674. But his statement in that case would not have been inaccurate?—No, then it would not have been inaccurate.

9675. (*Sir William Church.*) I gathered from an answer that you gave to Sir Mackenzie Chalmers that you do not think that antitoxin is of value in the treatment of diphtheria?—Yes.

9676. And then you gave him a considerable number of names of, as you said, persons who had written or spoken against its use as being dangerous?—Yes.

9677. Would you kindly give the Commission the name of any person of repute, who is held in good repute by his professional brethren and the public at large, either in this country, or in Germany, or in France, or in any other country, who, having had large experience of diphtheria, both before and after the introduction of the antitoxin, is not convinced of its usefulness?—I should be most obliged if I could be allowed to send that as a note. I will prepare a list of names of people. It is rather too wide a question to answer in a minute, because there are a great many. I have the names of a great many opponents of vivisection in general, who are also opposed to serum therapy.

9678. They must be people of repute, both in their own profession and in the eyes of the public?—If you mean by that, repute among pro-vivisectionists and vivisectors, I do not think I can supply it, but if you mean men of repute among humanity at large I am sure I can supply it.

9679. (*Chairman.*) You cannot supply any name at present?—Yes, I can.

9680. You might do that now?—I can supply the names of the men at the discussion that took place at the Chelsea Clinical Society last year, I believe in March. Professor Wright, I believe, spoke against serum therapy to some extent.

9681. (*Sir William Church.*) Who is he?—Sir Almroth Wright.

9682. Where did he speak?—At a meeting of the Chelsea Clinical Society, and also Professor Hewlett. But, if I am asked to do it now I cannot do it with the same accuracy and care that I would if I supply a note of it. Then I believe Dr. Rosenbach, a German physician, has written against it, and Dr. Boucher, and Dr. Deswatines in France.

9683. I do not think it is any use mentioning the names here now, because what I have asked you to refer the Commission to is the names of persons of high professional and general repute who hold the view that you hold of the uselessness of antitoxin in diphtheria.

9684. (*Chairman.*) I should have thought it better, if you can mention them now, to do so, because if a note is sent in a list might be sent which we should have no opportunity of questioning as regards the eminence of the persons, and so forth. If you can give us any information now it would probably be more useful to us?—I could mention the name of one man of eminence who has previously expressed those views, but I believe I am precluded from mentioning his name at the present moment.

9685. (*Colonel Lockwood.*) Why?—I think it is one of the Commissioners whom I should mention.

9686. (*Chairman.*) You might as well let us have the name?—If I may mention Dr. George Wilson, I believe he has expressed such views; and Dr. Boucher, in France, the secretary of the French branch of the International Medical Anti-Vivisection Association, Dr. Lutaud, Pasteur's eminent opponent. From Germany I could supply names by the hundred.

9687. (*Sir William Church.*) We only want one, the most distinguished?—Dr. Lahmann, in Germany; and I do not know whether homeopaths in England would be considered to be men of repute or not, but I believe the homeopaths stand as one man against serum therapy. I can mention the hospital staff of the Homeopathic Hospital.

9688. If you had noted my question, I asked you for the names of those who have had large personal experience of the treatment of diphtheria, both before and after the introduction of antitoxin. People who have not had large experience of diphtheria both before and after the introduction of antitoxin I do not want you to refer us to, but I want you to refer us to those who have had large practical experience of the treatment of diphtheria both before and after the introduction of antitoxin?—That condition is scarcely a fair one to impose upon me, because the men who would oppose the use of diphtheria antitoxin would be men who oppose it on principle, and on various principles, and therefore would not try it; they would rather take the experience of other men who have failed than try it themselves.

9689. That is just what I want to avoid. Now one or two other questions, if I might be allowed to ask you. At Question 7423 I asked you a question about pithing: "You understand what pithing is? (A.) Yes, of course," and you have been kind enough to-day to give us a list of names of physiologists who use the term in various ways, you say; but do not they all use it in this sense, either that they mean total separation of the spinal cord from the brain or destruction of the cerebral hemispheres or of the whole brain?—I do not think they all use it in those senses. I have already given evidence to show the different senses in which it is being used.

9690. Yes, you have given us a number of names, and I ask you whether all those authorities to whom you have referred do not use the term either as expressing the severance of the spinal cord from the brain or the destruction of some part of the brain itself, either the cerebral hemispheres or the cerebral hemispheres and something more?—Then I must answer "No," because some people use it in the proper sense, the inclusive sense, the sense to which I would agree where the word pithing would be applicable—namely, the destruction of the brain and spinal cord.

9691. That is what I say?—No, I beg your pardon, I do not think you said that; you did not include the destruction of the spinal cord in your question.

9692. No, of course I exclude the spinal cord, that is going still further; but I say: Do any of the physiologists to whom you have referred us to-day make use of the term pithing in any other sense than either the separation of the spinal cord from the brain or the destruction of either the cerebral hemispheres or the cerebral hemispheres and other portions of the brain?—I am afraid I must give again the same answer, unless I misunderstand you entirely, that some use it in the real sense of destroying the spinal cord as well.

9693. I am perfectly well aware of that; that is a different question. I will not trouble you any more on that. I understood you to say, in answer to Dr. Gaskell, that you are of opinion that the spinal cord is sensitive to pain. I do not know whether the head also feels pain when it has been decapitated?—I think Sir Lauder Brunton gave some evidence to that effect about Ludwig holding that view. (Question 7106.)

9694. (*Sir William Collins.*) Do I rightly understand that the word "pithing" is ever used to denote the condition of mere separation of the brain from the

spinal cord?—A mere cutting—a mere cut, yes, it is used in that sense.

9695. That is one sense?—Yes, but it is very rare.

9696. Is it used in the sense of mere destruction of the contents of the cranial cavity?—Very often.

9697. That is another sense?—Yes.

9698. Is it used in the sense of destruction both of the contents of the cranial cavity and of the spinal canal?—It is used in that sense, too, and my contention is that that is the proper sense.

9699. Is it your opinion that only in the third and last sense the term pithing ought to be employed?—Yes, that is so.

9700. After I concluded my questions on the 1st of May you answered Question 7627 in these words: "From my own point of view I am prepared to say that nothing which I should call useful has been obtained by experiments on the lower animals"?—Yes.

9701. If that be so, should I correctly understand you to contest the assertion of those many witnesses who have appeared before us, who have stated that various drugs and other modes of treatment as well as other useful discoveries have been made by experiments upon the lower animals?—I answered partly in that answer that it was a question of relative terms, and that my personal opinion was that nothing indispensable or in the higher sense useful had been discovered by such means.

9702. Then in that sense anything useful could not have been gained by experimentation on the lower animals?—I look upon it from the moral point of view, and I do not believe that the lesser and immediate utility which might be claimed as the result of doing something against a moral law is of real advantage. I believe that it is a greater advantage on all planes, moral and physical, to do the thing which is morally right, and that it will ultimately also be found to have been intellectually right.

9703. To take the case of Dr. Cushny's evidence, would you contest the assertion that various soporifics, such as chloral and sulphonal, have been discovered by experimentation on the lower animals?—I should not contest that.

9704. Then do you contest that they are useful?—I contest that they could not have been discovered by experiments on human beings who offered themselves voluntarily. I contest that the method of experiments upon the lower animals is indispensable. From the experimental point of view they might just as well have been discovered by experiments on voluntary human beings who are anxious to find out the truth about a drug which they believe will be of value.

9705. When you say that they might have been so discovered, it is easy of retort that they might not; and it is alleged that as a matter of fact they were discovered by experiments upon animals?—As a matter of opinion I do not think that that answer could be given, because nobody could deny that they could have been discovered by a few experiments on voluntary human subjects, provided that the experimenters had been a little more unselfish.

9706. But you do not allege that they were not so discovered?—I do not contradict it.

9707. Then as regards local anæsthetics—cocaine, for example—do you say that that is not useful or that it was not discovered by experiments upon the lower animals?—I should say that cocaine is exceedingly useful.

9708. Do you say that it was not discovered by experiments upon animals?—I do not think I should say that; but I have the same answer to give in relation to that, that it might perfectly well and better have been demonstrated on the human being.

9709. Does the same answer apply to the case of nitrite of amyl, which has been frequently quoted in evidence?—I think it applies to all cases. If I were keenly interested in the testing of drugs I would certainly offer myself for testing them. I would not do it on dogs, cats, and rabbits.

9710. Whereas they might have been discovered by testing them on human beings, you do not deny that they were discovered by testing them on animals?—I do not deny that you can discover things by experiments on animals; I think I would be very foolish to attempt to deny it, you are bound to discover something. The question is, first, whether the discovery is really valuable; and, secondly, whether the discovery could not have been made through other means.

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9711. But when you make the statement that nothing which is useful has been obtained by experiments upon the lower animals, it seems to me that it is incumbent upon you to show, when it is alleged that anything useful has been so discovered, either that the thing is not useful or that it has not been so discovered?—Then I ought to have added, nothing useful which could not have been discovered by other means; because if an experiment be considered absolutely necessary, my contention is that it should not be a vivisection experiment on an unwilling victim.

9712. You also stated that you had been at the Pasteur Institute. The treatment of rabies or hydrophobia has been often alleged as an example of a useful discovery as the outcome of experiments on animals; have you anything to say in regard to that?—I have to say that I have seen long and sad lists published, amounting, I believe, to thousands of people, who have died in spite of the Pasteur treatment for hydrophobia. They are published in the "Annales de l'Institut Pasteur" and other journals.

9713. In that case do you allege that what has been discovered is not useful?—I am most strongly against the Pasteur treatment, certainly.

9714. (*Chairman.*) That is not quite an answer to say that you are against it; do you allege that it is not useful?—I do not consider it useful.

9715. (*Sir William Collins.*) In the case of drugs I understand you to say that nothing useful has been discovered by experiments on animals, although something might have been discovered by experiments on man?—Yes, the decisive experiment must be made on man.

9716. With regard to the treatment of hydrophobia, I understand you to say that it is not useful in itself?—Certainly.

9717. And with regard to the treatment of typhoid fever, plague, and cholera, do you say that useful things have not been discovered, or that they have not been discovered by experiments on animals?—In that case I should say that useful treatments have not been discovered by experiments on animals wherever the treatment takes the form of a serum.

9718. And with regard to snake poison, what answer do you make there?—I am not aware of any successful treatment that has been brought out for snake bites which necessitated experiments upon animals.

9719. With regard to the treatment of myxedema by thyroid extract, do you allege that the discovery is not useful or that it has not been made by experiments on animals?—I think that it has partly been made by experiments on animals, but the principle might just as well have been discovered by other experiments—in fact, an equivalent of thyroid extract is being made very largely, I understand, in chemical laboratories and prepared synthetically.

9720. As regards adrenalin as a mode of arresting hæmorrhage and for other purposes, do you allege that that is not a useful discovery or that it was not obtained by experiments upon animals?—I do not think that the use of adrenalin is absolutely necessary or that I should include it among that which is useful. That again is a matter of opinion whether it is useful or not. I do not consider it useful, if I answer the question plainly.

9721. In therapeutics adrenalin serves no useful purpose?—No, I consider that it is not useful.

9722. Do you think that it was discovered by experiments upon animals?—I am against all the organo therapeutic system of such drugs and the applying of drugs which is now in vogue.

9723. Do you contest the allegation that it was discovered by experiments upon animals?—I think it was discovered by experiments upon animals. A great number of experiments have been performed indeed in my presence with adrenalin.

9724. One more thing—the treatment of anthrax and the prevention of anthrax; do you think that something useful has been discovered there?—No, certainly not. I think there has been a great deal of failure. Only yesterday the papers contained an account of a new process which has been brought out by a technical chemist at Dewsbury, a purely chemical one, which promises much for the future.

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9725. (*Chairman.*) You prefer your judgment to that of Sir John Macfadyean, with his large experience upon a question of that kind, as to whether this treatment has been found a useful remedy for anthrax?—I have not heard his opinion.

9726. (*Colonel Lockwood.*) Would you not say that the discovery of the X Rays has done more for science than any discovery which has been made by the use of living animals?—I think there are infinite possibilities in the discovery of the X Rays, both for physiological investigation and the healing of disease; and in the light treatment and in all the new treatments, more than in any vivisectional treatment.

9727. And will be of more value than any results of vivisection?—Yes.

9728. (*Chairman.*) I think that concludes your evidence?—My Lord, I am not going to take up too much time of the Commission, but there are two things which I have not been allowed to answer; I will do it as shortly as I can—in five minutes.

9729. If you have not been allowed to answer I presume it was for some reason?—The first matter is with regard to a note that I was asked to supply on the paper by Dr. Brodie and Miss Cullis, and then I have an answer to Mr. Thane's statement about the dog that he referred to in his evidence on the fourth day, which is described in the "Shambles of Science"; and, thirdly, a few words about "the brown dog." There are two statements that I am very anxious to bring out on "the brown dog" in answer to Professor Starling and Mr. Thane.

9730. I hope you will give it very shortly, because these are matters which we have discussed at considerable length?—I will do it as shortly as I possibly can; I will only take a few minutes, and I will not read what I have got. The first note that I was asked to give is this: A further reference to the paper by Dr. Brodie and Miss Cullis to which I referred in my evidence shows that the animals were by no means pithed in the proper sense of the word, the spinal cord and the medulla oblongata having been left intact. Nor was it an immediately fatal operation, for such animals can breathe spontaneously and "survive for some four to eight hours." Artificial respiration was carried out during the actual experiments, not for the purpose of maintaining life, but in order to get a more uniform aeration of the blood. Then I have some comment upon that, which I do not think I will trouble the Commission with, but perhaps I may just mention what it is, as it is rather a question that touches the legal aspect of such a case. Either these animals are dead or are supposed to be dead, and then anybody can experiment upon dogs deprived of their brains—any man in the street; or they are alive, and then they ought to come under the Act. Miss Cullis, I see, had no licence when she performed these experiments, and I presume that she considers the animals to have been dead, and therefore that these were not illegal experiments. The question is of the utmost importance at the present moment, as the Act relates to experiments upon animals which are alive, and it seems that we must reconsider what it means to be alive. Then the point in Mr. Thane's evidence was in what he said at Question 1270. He said that the look of utmost agony in the eye of the fox-terrier was the result of our imagination, as "emotions are not expressed in the eye itself, but in the surrounding parts." In answer to this, I must point out that the "surrounding parts" of the eye were quite visible to us, and that it is immaterial to this particular case whether we prefer to localise the expressions of different emotions to the eye itself or its "surrounding parts." That is all I will trouble the Commission with on that point. I have now the last, and that is the so-called "brown dog." That was brought before the Commission several times, especially by Mr. Thane, at Questions 1551 to 1568, and by Professor Starling at Questions 3696 to 3705. I do not know that I shall be able to make myself intelligible now, as I leave out the greater part of my statement on this point; but there are principally two points of view that I wish to bring before the Commission; first, that Professor Starling has said before this Commission that this dog was "quite well" and "quite healthy," and that during the trial—in fact, the whole trial turned on that issue, which was presented by him and Mr. Bayliss—he said that it suffered from a disease, and its state was referred to as "morbid," and even the failure of the experiment was attributed by Mr. Bayliss to this "morbid" state. That is a contradiction to which I feel I ought to draw the attention of the Commission. Now comes the last, and then I have done, and that is that in operating upon this dog, in subjecting it to a third vivisection, a contravention of the Act was made, for the Act does not say that you are allowed to perform more vivisection experiments on an animal under Certificate B, provided that animal is still in a state of anæsthesia; the Act says nothing of the kind, it says that when the operator has finished, when his object has been attained, he is under an obligation to kill that animal. Now Professor Starling has acknowledged that he had attained his object with that dog, and yet he handed it over to Mr. Bayliss, and it is a significant fact that Mr. Thane, the Inspector under the Act, has told this Commission that he is not quite clear whether this is legal or not (Question 1558)—it is rather a "legal point," and, further, that he does not like it quite (now I may quote a word wrongly, because I do not read it), and that is not a thing one would desire to be done (Question 1561); and, further, that if there had not been this trial he would not have known of this procedure (Questions 1563 and 1564); and that, as far as he knows, there may be cases of a similar kind which would not appear in his report (Question 1563). Those things have been told to the Commission by Mr. Thane, and I have brought it in from the point of view of what has been said on the "brown dog" before the Commission.

9731. (*Sir William Church.*) With regard to the discrepancy between the statement that Professor Starling made here, that the dog was quite well and healthy, and the statement at the time of the trial that the dog was said to be in a diseased state, what was said to be the matter with the dog at the trial?—It was said to be suffering from chorea.

9732. Canine chorea?—Yes.

9733. What is that?—It is a disease.

9734. How does it manifest itself?—It manifests itself in twitching.

9735. And it was suggested at the trial, I think, that some of the movements which might be thought to be due to pain, and which moved the board, were due to this twitching of the muscles?—Quite so, that was the defence of Mr. Bayliss and Mr. Starling.

9736. What is the condition of a dog which has canine chorea?—I have seen a dog with it—Mr. Sewell showed it to me.

9737. What is its condition?—It is a dog that is in a state of disease. You cannot refer to it as being quite healthy.

9738. For instance, if it is a sporting dog is it able to be used for sport just the same as any other dog?—I should think not.

9739. You only think that?—I have seen a dog suffering from chorea, and I am sure that such a dog could not be used. That was not the "brown dog," for I do not admit that it had chorea—it was a bulldog.

9740. Speaking of dogs which have canine chorea, are they what you would call unhealthy; they have this twitching, but are they unhealthy?—The dog that I saw was diseased and in a very diseased condition.

9741. Was that the brown dog?—No.

9742. I am not saying that a dog might not be unhealthy?—I have seen a dog suffering from chorea at Mr. Sewell's establishment.

9743. Then you think I should not be speaking the truth if I told you that I had frequently shot over a dog which had twitchings?—That dog may have suffered from a very slight form of chorea, but, as I understand, it was inferred that the form of chorea that this brown dog was said to have suffered from, according to Mr. Bayliss and Professor Starling, was a severe form; but the dog I saw certainly could not have been used for sport; and what Professor Starling implied here before the Commission was that the dog had recovered from the operation, and that it was in the same condition as it was before the operation. He did not say that it had not canine chorea, but he said during the trial that it developed canine chorea, I believe after the first operation, between the first and the second.

9744. The dog had canine chorea before it was ever operated upon at all?—No, I do not think that was stated.

9745. I beg your pardon, it was?—I beg your pardon. I think Professor Starling stated distinctly

during the trial, and I think I know that trial, that he observed the chorea in January, and he had operated upon the dog in December—he performed the first operation in December.

9746. You assert that it was the result of the first operation?—I cannot assert that. I can only assert that it was declared to have come after the first operation.

9747. Of course we can ask Professor Starling himself, but that was not the case; it was not the result of the first operation. The dog had this twitching before ever it was operated upon at all?—That is not so according to Professor Starling's evidence at the trial.

9748. That I do not know anything about.

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9749. (*Chairman.*) It does not seem material to the question that we have to decide whether this operation upon the brown dog was an illegal operation or not?—I only brought it out as a contradiction on the part of Professor Starling.

9750. It is a contradiction that does not bear much on our particular inquiry; it may have done so on the trial; but you cannot ask us to disbelieve all Professor Starling's evidence, because you allege that there is a discrepancy between his saying that the dog was well and that the dog had chorea?—I think it is rather of some importance.

9751. I do not attach much importance to it myself.

TWENTY-SECOND DAY.

Wednesday, 12th June 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.B.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. (now Sir) HENRY R. SWANZY, F.R.C.S. (Ireland), called in; and Examined.

9752. (*Chairman.*) You are a Doctor of Medicine?—Yes, I am a Doctor of Medicine of the University of Dublin.

9753. And you are President, I think I am right in saying, of the Royal College of Surgeons in Ireland?—I am President of the Royal College of Surgeons, and I am sent as representing the College.

9754. Will you tell us, before you give us their message, a little about the Royal College of Surgeons in Ireland? It is a large body?—The Royal College of Surgeons in Ireland consists of a Body of Fellows; we have 466 Fellows, and we have also a large number of Licentiates—2,908 Licentiates. The Fellows elect the Council of the College, which consists of 19, and they also elect the President and Vice-President annually.

9755. Is it one of the qualifications that they should be practising in Ireland?—No, not at all. A great many of our Fellows practise in England and in the Colonies, and elsewhere—large numbers of them.

9756. But their headquarters are in Dublin?—Their headquarters are in Dublin.

9757. And you yourself, besides being President of the Royal College, are Ex-President of the Ophthalmological Society of the United Kingdom?—Yes.

9758. And Surgeon to the Royal Victoria Eye and Ear Hospital in Dublin?—Yes.

9759. And I presume from that that you are to a certain extent a specialist in regard to the eye?—I am.

9760. You are aware that we invited the Royal College of Surgeons in Ireland, to give evidence?—Yes.

9761. And it is in accordance with that request that you come here?—It is in accordance with that request.

9762. Would you just tell us what their views are generally on the question of experimentation on animals?—It is considered that it is necessary for the advancement of medical and surgical science, in that it has introduced great improvements and advance on our knowledge and our methods in the past, and that we may look forward to its doing similar work for the good of medicine and surgery and of humanity in general in the future. That is in a general way the position which the College takes up.

9763. Those are the views of the Council?—Those are the views of the Council.

9764. Which is the ordinary mouthpiece, I suppose, of the Body?—Yes.

9765. Is that the unanimous view of the Council?—Yes, that is the unanimous view of the Council.

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9766. And your own view?—And my own view, most distinctly; I should not have come unless I was prepared to back it up.

9767. As regards the use of anæsthetics and the avoidance of pain, have your Council expressed an opinion upon that subject?—Certainly. So far as it is possible, and so far as it is compatible with the success of the operations, it is necessary and desirable that every means should be adopted to prevent animals having pain, or inconvenience even, so far as it can be managed.

9768. You say so far as it is compatible with the success of the operation?—Yes.

9769. There are certain operations, as we know, which are provided for by the Act, in which the direction to kill the animal before it comes out of the anæsthesia is suspended?—Yes.

9770. What do you say to that provision; do you think that it is necessary?—Yes, certainly I do.

9771. And when you say, so far as is necessary for the success of the experiment, I suppose you are alluding to experiments of that nature?—Yes, the experiments are with the object of obtaining knowledge, and the animal should be protected from pain and inconvenience in so far as that is compatible with the obtaining of the knowledge which is proposed to be derived.

9772. Have your Council formed any opinion as to whether any animals which are now subjected occasionally, or frequently, to experiments should be excepted from the list?—It is held that there should be no exceptions from that list.

9773. That is to say, that the law should remain as it is at present?—Yes, there are no animals excepted now.

9774. There are exceptions. There is certainly exceptional treatment of horses, for example—of horses, dogs, and cats?—Yes; they require a special certificate; and there can be no objection to that; but some people propose that certain animals should be excepted, and that is what we do not agree with.

9775. That is what I understood, but I wished to have it quite clear whether you raised any objection to the present limitations as regards horses, dogs, and cats?—No, I do not think so.

9776. But you would not, as I understand, extend those limitations?—No, we would not extend those limitations.

9777. Then what do you say as to experiments upon

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animals for the purpose of enabling surgeons, or future surgeons, to acquire skill?—That is a point upon which I have heard opinions expressed; I have made it my business to inquire among general surgeons upon the subject, and perhaps when some general surgeons come before you you may get from them more direct information, because I am not a general surgeon, as you understand; but all those with whom I have spoken, including some of the most distinguished men with us, are clearly of opinion that it would be greatly to the advantage of surgeons, and of those learning to be surgeons, if operations upon animals for the purpose of acquiring skill were permitted.

9778. Would you apply that to students, or only to those who have taken degrees or already acquired and given proof of having acquired a certain amount of skill?—I would extend it to students towards the end of their course, when they were almost surgeons, and when they were about to enter upon practice, where they would have to perform operations very soon upon the human animal.

9779. Would you have surgeons who were permitted to experiment for those purposes do so under a special licence for the purpose, or would you let it be one of the incidents of their being qualified surgeons?—I presume that it would require a special licence in this way: That the animals ought to be protected from pain and inconvenience as much in operations for skill as in operations for the ascertaining simply of knowledge; and therefore I presume it would be necessary to have a special certificate. But that is a point I have not considered; I am merely speaking in a general way.

9780. You mean that you would apply all the protections which are applied to animals as to freedom from pain to the cases of those who were experimented upon for the purpose of acquiring skill?—Yes, I would protect animals to the utmost in that case, as I would also in the case of experiments for obtaining knowledge.

9781. But I was rather asking you, would you require a person who was to get leave to experiment for purposes of that sort to have a special licence or certificate for that purpose from some authority?—I suppose it would be necessary.

9782. (*Sir Mackenzie Chalmers.*) It would not be allowed under the existing law at all; therefore it would be a change in the law?—I do not know what measures would be necessary, but in a general way it is held that it would be a most desirable alteration in the law if it were permitted.

9783. (*Chairman.*) You have not considered the question whether the authority to grant such a licence should be the Home Office or a medical authority, or both?—No, I have not at all.

9784. But you think that the law should permit experiments to persons having certain qualifications already for it?—Yes, I do. I agree with a witness who was here—I forget now who it was—who made a very striking observation, which was that he regarded the present law as immoral, because it did not permit these operations upon animals for the acquirement of skill.

9785. (*Colonel Lockwood.*) Was he a medical witness?—He was a physiologist.

9786. (*Dr. Wilson.*) It was Professor Starling?—Yes, Professor Starling. He said that he regarded the present law as immoral, because it did not permit operations for the acquirement of skill.

9787. (*Chairman.*) Because it sent out persons as being qualified for surgery who had not had an opportunity of acquiring that skill?—Yes, because it excluded animals from being used for the purpose of obtaining skill.

9788. Then another point upon which we have had a great deal of difference of opinion from witnesses before us is as to whether experiments should be permitted for the purpose of teaching students by demonstration. What is the view of your Council and yourself with regard to that?—The view of my Council is that it is desirable that students should be taught when necessary by means of experiments upon animals. There was one member of our Council—I wish to be perfectly candid—who seemed to be a little doubtful on this subject; but he seems to have altered his mind about it.

9789. I asked you whether that was the view of your Council and yourself. Did you agree with the majority of the Council, or did you agree with the doubts of the other gentlemen?—I agree with the majority of the Council.

9790. Have you considered at all the question of what limits, if any, you would put upon demonstrations to students, or would you leave the law as it is?—I would not limit it any more than it is limited.

9791. So much as to the general views. You have told us that your practice and experience lie principally in matters of eye disease?—Yes.

9792. And I think you wish to illustrate your views about the value of experiments upon animals from your experience in eye treatment?—I should like to endeavour to do that if you will allow me.

9793. If you please?—Of course, the question of inflammation, and what inflammation is, has been one of the most important questions, upon which a great deal of light has been thrown within the last 50 years or so, and some of the pioneer knowledge in this matter was gained by experiments upon the eyes of animals. The cornea of the eye is peculiarly suited for observing the processes of inflammation going on, as shown by the experiments of Cohnheim and Leber. Cohnheim was one of the first who showed what pus was, and a great deal of his experiments were made upon the cornea of animals.

9794. Of living animals?—Of living animals. Experiments on the origin of pus corpuscles.

9795. Was that before the days of anæsthetics?—These experiments were done in Germany, and they were not probably quite so particular there. I do not advocate some of the methods which have been used there, but these experiments would be done under anæsthetics, the animal would be anæsthetised, of course, during the whole time, and then not killed afterwards, because you could not see what went on afterwards unless you allowed the animal to live.

9796. These are German experiments that you are speaking of?—They are.

9797. Merely for the purpose of giving us the history?—I am only giving you the history. I want to point out in what way, by experiments upon animals, we have gained knowledge concerning inflammation which is of the utmost importance to medicine and surgery in general. Other cases which I have to bring forward are more specially connected with disease of the eye; but that is a general question, and a most important one, a great deal of knowledge of which has been obtained by experiments on animals' eyes.

9798. That is to say, the discovery of what pus is and how it is formed?—Yes.

9799. What would be about the date of those experiments?—Cohnheim was in 1873, perhaps rather earlier. His first work was published in 1867, but he wrote a monograph in 1873 on the subject. I was in Berlin myself at the time, and I saw his experiments, not upon animals' eyes, as it happened, because his very earliest experiments were done upon the mesentery of frogs and the tongues of frogs, and I saw those. Then as regards the function of the ciliary body, it is very important to know what the function of the ciliary body is.

9800. Will you explain to us what is the ciliary body?—I will try to do so. The iris is the part of the eye which gives colour, when a person has a blue eye or a brown eye, and that is the only part of a certain coat of the eye that one can see. There are two other parts, the ciliary body and the choroid.

9801. Are those two films, as it were, under the colouring one?—Yes, they are behind the white of the eye. The iris you can see in front through the cornea. It was not known what part of this uveal tract, whether the whole of it or what parts of it, secreted the fluids of the interior of the eye; there are two fluids, the vitreous humour and the aqueous humour. We knew pretty well, or suspected, at any rate, that these fluids come from the uveal tract, or from some part of it, but it was not until the year 1880 that it was definitely ascertained that the ciliary body was the part of the eye which secreted these fluids, and then we immediately began to understand certain diseases of these two fluids.

9802. Were those fluids discovered by experiments?—No, they were always known, I suppose, from any remote period. When we found that it was the ciliary body which secreted these fluids—and that discovery was made by experiments upon animals—we then began to understand certain diseases of the eye which we had not understood the nature of at all before. There was

one disease of the eye which was called keratitis punctata, which we thought was a disease of the cornea; but from the time when we ascertained the meaning of the ciliary body we learned that this keratitis punctata was merely a symptom of inflammation of the ciliary body, and consequently we then went about treating the keratitis punctata in quite a different light. Then, also, there are opacities in the vitreous humour, which sometimes appear, and one wondered what they meant; and there were various ideas as to what they meant. We now know that for the most part these opacities, too, are due to inflammatory processes in the ciliary body, and consequently we now proceed to deal with them in a different way.

9803. About when would that discovery be made?—That discovery was made definitely by Deutschmann in 1880.

9804. You say it was discovered by experiments upon animals; was it discovered wholly by that, or was it assisted by clinical observation or by observations on the human eye in cases of disease?—It was discovered entirely by experiments on animals. There was no other way by which it could be done.

9805. You have something to tell us, I think, about the corneal endothelium?—The cornea is the clear part of the eye which covers the iris, between you and the iris; if you look at somebody else you see yourself in that person's cornea—it is the glittering, bright part of the eye. On the back of the cornea there is a very delicate membrane called the endothelium, and between the cornea and the iris there is a little water called the aqueous humour, of which I have already spoken. We did not know what use the endothelium was, but it was discovered by Leber in the year 1873 that the use of the endothelium is to protect the cornea from the aqueous humour—to prevent the aqueous humour filtering into the cornea. If the endothelium is damaged or injured the aqueous humour percolates into the cornea and makes the cornea opaque; and, of course, opacity of the cornea interferes with vision. That has been a most important discovery, because now when we are operating we are most careful to avoid injuring the endothelium, for if we injure it—suppose that in extracting a cataract by any chance we injure the endothelium either with our instruments or by using some antiseptic, a weak sublimate lotion getting into the anterior chamber and injuring the endothelium—the aqueous humour percolates into the cornea and makes it opaque.

9806. That might produce total blindness, I suppose?—At any rate, it would make the eye a very imperfect eye, a useless eye, and damage the sight. That is knowledge which we could not possibly have gained in any way except by experiments on animals. Previously we used sometimes after operations to have opacity of the cornea; and we did not know why it happened. Now we not only know why it happens, but we know what we should do to prevent its occurring. I have seen this thing happen myself and I did not know why it happened. It does not occur now with me, because I carefully avoid those things which would produce it.

9807. By experienced oculists I presume that is perfectly understood, and those effects are avoided now?—Yes, certainly.

9808. Could you explain any further why experiments on animals were essential to this discovery?—I am at a loss to understand how it could be ascertained in any other way.

9809. You say that it was ascertained, in fact, by that means?—Yes.

9810. You gave us the name of the experimenter?—Leber.

9811. In what country was it?—In Germany.

9812. Has he published accounts of his experiments?—Yes.

9813. I am not asking you to give them, but I suppose you would give the same answer here as you gave as regards the function of the ciliary body, that mere clinical observation could not have discovered it?—It could not have discovered it. One might have guessed, but guessing is not any use; we must have definite absolute, downright knowledge.

9814. What would be the nature of the experiment for making this discovery as regards the corneal endothelium?—There were a series of experiments, of course, but the one which was conclusive, or, at least, one of those which was fairly conclusive, was that an incision was made in the cornea, a sharp instrument was passed into the anterior chamber of the eye, and the back of the cornea was deliberately injured—in fact, it was injured in the form of a cross; then the little wound in the cornea healed, the aqueous humour re-formed after a few hours, and soon one saw in the cornea a little grey cross corresponding with the injury which was done on the back of the cornea; so that obviously the condition of opacity in the cornea must have arisen from the wound or injury on the back of the cornea.

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9815. As you say, that is only an illustration of the kind of experiments. I suppose there were a series of them?—Yes, no doubt.

9816. Then you wish to deal with transplantation of the cornea?—As I have just been saying, the healthy cornea is clear, and when opaque it interferes with vision. A great many diseases cause opacity of the cornea, whilst the rest of the eye is perfectly good for vision, so that if there were only a clear cornea the patient would be able to see very nicely. For probably fully a century or more there have been attempts made to transplant a clear cornea from the animal's eye to the human eye in such cases, with the object of providing the patient with a clear cornea and good vision. Those experiments began so long ago as the year 1831; at any rate, Reisinger in the year 1831 operated and used a rabbit's cornea for the purpose. He wanted to transplant the whole cornea from a rabbit; he took away the opaque human cornea and took the rabbit's clear cornea, and put it on instead, and healed it on, but it soon became opaque. Stilling, in 1833, tried the same thing, and there were a great many other experiments of that sort, modified by different people as they tried to think out what could be the reason of the transplanted cornea becoming opaque; but always, although it healed on, it became opaque, so that the operation was no use. Although it healed on there it became opaque. I have not thought it necessary to bring every name here; but, at any rate, it was early in the last century that those experiments began. The question seems to have been taken up again in 1872 by Mr. Henry Power, who still lives, although he does not practice in London any more, who read a paper at the Ophthalmological Congress in London that year. He had been making experiments upon dogs and cats and rabbits, with the object of discovering a good method, and he, too, found that although he could do the operation successfully, yet, when the cornea had healed on, it always became opaque. That was the great trouble, that although you could do a successful operation as such, yet the new cornea always became opaque.

9817. You mean by successful operation, one in which the human being adopted the transplanted cornea?—Yes, a surgical success, but not a success in giving the patient any advantage. Therefore the great puzzle was to do this operation in such a way that the cornea would not become opaque; and the knowledge that was gained by Leber's experiments, which showed that it was the endothelium on the back of the cornea which protected the cornea from the aqueous humour, and that when the aqueous humour percolated into the cornea the cornea became opaque; it was that knowledge which helped us to transplant the cornea from a rabbit or from a freshly enucleated human eye on to another human eye with retention of its transparencies. What was done was then the opacity was trephined off down to a certain depth and the posterior layer of the cornea and the endothelium were retained, and then a piece of the same size was taken off the other cornea with the same trephine, and put into the little hole in the cornea, where it healed on and it remained clear. This was first done by Von Hippel in the year 1877.

9818. What does trephining there mean?—The little trephine is a little circular knife—a little punch, you might say almost. A piece was punched out of the opaque corner and a piece of the same size punched out of the clear cornea, and the clear cornea put on to the opaque cornea, and then, in consequence of the endothelium on the back of the cornea not having been interfered with, as it was in previous attempts, the new clear flap that was put on remained clear. I do not say that that is an operation which is in every-day use; it does not happen in many cases that you can do it, because, unfortunately, there are not a very large number of cases where the cornea is not opaque right through. If it be opaque right through there is no use in taking away a piece of its surface, and the operation is only good

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for cases where the opacity does not go right through the whole thickness of the cornea; there is that limitation to it. But I give it as an example of the valuable progress made in surgery for the good of people who suffer from blindness of a certain kind, and as the result of knowledge gained by an experiment upon an animal. It was the same experiment, in fact, which I referred to before as so useful in letting us know that we must not injure the back of the cornea in operations.

9819. Has serum therapy been utilised for diseases of the eye?—Yes, we are beginning now to use it a good deal. I have put down in my *précis*, as my fifth item, serum therapy; but I would ask you to take that number along with No. 7 and No. 14, because they really are all more or less serum therapy. There is an ulcer of the cornea which is called ulcus serpens. It is an exceedingly severe form of ulceration, and eyes are very frequently lost by it; at any rate, if they are not lost as eyes, they are lost as seeing organs, because it causes opacity of the cornea; it is one of the diseases of the eye which causes severe opacity of the cornea. Our methods hitherto have been unsatisfactory; except in the mildest cases we are seldom enabled properly to save the sight of an eye in this disease. The organism which causes this ulceration is the pneumococcus, and recently a pneumococcus serum has been prepared by Römer, which has been used for treating this disease; at any rate, it ameliorates its severity to such an extent very often that it enables the other measures which we have been hitherto using to be used with greater benefit. The experiments were first made upon rabbits by Römer. That is a recent thing—1905 is the date of it. Then with regard to tubercle of the eye, which is a very prevalent and very devastating disease of the eye, we find that treatment with tuberculin is exceedingly valuable. It is useful both in treatment and in the diagnosis, because, although it is a common disease and in many instances you can diagnose it perfectly well, and anyone seeing it is able to make a diagnosis at once, yet there are cases where one is in very considerable doubt, and you have to resort to other measures than your knowledge of appearances; and by using tuberculin in a rather high dose you get certain reactions which help you to make the diagnosis. There is also a method of diagnosis which is not possible in this country at present in the ordinary way in hospitals, but which can be done, I suppose, if we get some friendly physiologist to help us, namely, if we take a little of the water out of the aqueous humour in a case where tubercular disease is suspected and introduce it into an animal's eye, say, a rabbit's eye, it produces tuberculosis of the eye if the disease which the patient is suffering from is tubercular.

9820. That would be for the purpose of discovering whether the tubercle was present in the eye of the human being?—Yes. The method of treatment of diseases of the eye with tuberculin was introduced by Von Hippel in 1904. There is another instance, No. 14 in my *précis*, which refers to a very valuable remedy called jequirity. Jequirity is the active principle of the paternoster bean, a Brazilian seed. We have in these countries, especially in Ireland, a great deal of a disease called trachoma, which is a most destructive disease to the eye. In some cases it quite blinds people, and in order to cure it we have many remedies; but one of the most valuable is this jequirity. It is in my experience a most valuable remedy, but there is one drawback to it, and that is that sometimes the reaction which it causes gives rise to a danger in the eye; it acts in some people with such violence that it endangers the cornea; and therefore we would like very much to have some means of controlling its action. It was found by Ehrlich that it was possible to immunise animals from jequirity. He devised or discovered, I do not know which word to apply, an antitoxic serum which, when introduced into the animal, absolutely prevents your being able to produce a jequirity reaction in its eye at all. Then further experiments by Römer elaborated this point, and Römer found that in human beings if this serum was introduced into the blood the individual became immunised, and that you could not produce jequirity reaction in the eye; and, further, that even if the serum was dropped into the eye without being introduced into the general system, it would prevent the patient getting a jequirity reaction; and, again, if the individual has a very violent reaction from the jequirity and you want to moderate it, you can do so by dropping the serum into the eye. It is therefore really a very valuable remedy in that way.

9821. What is jequirity; is it a drug?—Yes, it is a drug; it is a little black bean with red spots; it is called the paternoster bean; it comes from Brazil.

9822. I think we have had some evidence that it was used simply in the ground bean?—That is true. What is done is, the skin is peeled off, and then the bean is pounded up, and you make an infusion of it, and it is this infusion which is applied to the conjunctiva, the lining of the eyelids.

9823. I think we had one case given to us where the dry jequirity itself was said to have been rubbed into part of the eye?—I suppose that probably the dry preparation would do, but clinically I always use the infusion.

9824. It was not an experiment in England; it was an experiment, if I recollect rightly, in Canada; in some place, at any rate, where our present law as regards vivisection does not apply?—No doubt.

9825. (*Sir Mackenzie Chalmers.*) Does the injection cause great pain?—In the human eye, yes, it is very painful; the temperature goes up, and there is considerable feverish reaction in the individual.

9826. When it is used as a remedy?—When it is used as a remedy.

9827. Still, the result is worth it?—Oh, yes, it is well worth it.

9828. (*Dr. Wilson.*) It is to allay that pain that the serum is given?—Of course, the serum would allay the pain, no doubt, but that is not the great object. Unfortunately, jequirity, when it gives rise to a violent reaction, may produce ulceration of the cornea. That is the danger of it.

9829. Is this serum used in Ireland?—No, we have not got it yet; it is not very easy to get.

9830. You have no personal experience of it?—I do not think you could get it in this country at all.

9831. (*Chairman.*) There are some other experiences of yours that you wish to give us. Sympathetic ophthalmitis is one, I think?—Yes, sympathetic ophthalmitis is a very important disease. As you know, when one eye is injured the other eye, in certain kinds of injuries, is liable to become very seriously inflamed, and both eyes may be lost. It has been the object of oculists and pathologists for many long years to ascertain what this sympathetic ophthalmitis is and how it comes about. It is, I think, an example, perhaps, of experiments which, while their results were negative, yet had their use; because I think it is a greatly mistaken idea in some people's minds, that when experiments are made and fail to prove what is wanted to be proved, the experiments have been thrown away. But really that is a wrong point of view. If an experiment fails, or a series of experiments, then it is shown that that, at any rate, is not the meaning of the thing, and that we must try in some other direction. Sympathetic ophthalmitis, it was thought at one time, was caused by the irritation of the nerves in one eye giving rise to irritation of the nerves in the other eye, and that then, as the result of irritation merely, inflammation resulted, just the same as when you get a foreign body into one eye the other eye waters very much. But this was found not to be so. Experiments upon animals were done with the object of deciding this point, too; I cannot really give the details, because it is a long time ago now. Then there was an idea that the inflammation, the bacteria or the toxins, travelled round from one eye through the optic nerve of that eye, across the structure we call the optic commissure, down the optic nerve of the other eye and into that eye, and then set up the inflammation in the latter. Many experiments were made in this direction with the object of finding that out; and, of course, the reason why so many experiments were made was because one man made an experiment and he used a certain kind of infection, and that infection did not answer, and then somebody else used another kind of infection, and so on, and the experiments were not all carried out exactly in the same way, or there would not have been any use in repeating them; they were all done in different ways by different men in the course of years, and they all failed to show that the inflammatory process extended round from one eye to the other through the optic nerve and commissure. Now we have begun again in recent years, largely by reason of the generally held

view of the infection of the blood in disease which we have nowadays; and the idea now is that sympathetic ophthalmitis comes about really as a metatastic disease; that is to say, when one eye is injured micro-organisms set up their residence in it, and in the course of time—a few weeks— they extend into the general system; but they do no harm to any organ in the general system except the second eye if they happen to go into it; and they appear to have some special attraction for the other eye, which is a very remarkable thing. If we knew for certain that this view were correct, and if we knew, which we do not yet know, what the specific micro-organism is, we could then perhaps get a serum which, when an eye was injured, we could introduce into the patient's system and immunise him and prevent him from getting sympathetic ophthalmitis. And many a person who is going about blind now, if we had such knowledge, would now be seeing.

9832. It is a very common disease, is it not?—It is a very common disease; it occurs in shooting accidents and all kinds of perforating wounds; we see it every day. The best thing we can do for people nowadays when they come to us with an injured eye, is to advise them to have it removed. But very frequently they say that they would rather not; and they go off and come back again in a few weeks with serious inflammation in the other eye; and they go about with the help of a dog and a stick for the rest of their lives.

9833. Does taking the other eye out prevent it?—Yes, it does, because it takes away the primary focus where the poisonous toxins and the poisonous bacteria develop.

9834. That is a case in which many experiments have been made with a view to discovering a remedy, but they have none of them been successful?—That is an example of a disease in which for years and years experiments were made in one direction, because it seemed so probable that the disease extended round from one eye to the other; and those experiments all resulted in a negative way. Now the new light which has been thrown upon general pathology, and so on, has led us to direct a great deal more attention to this other way of infection of the second eye; and it is by experiments upon animals that this other theory must be followed out and shown whether it is right or wrong.

9835. But all that has been done at the present time has been to narrow the field of research to show that certain fields of research are useless?—Yes; efforts are being made to prove or disprove this theory.

9836. Is glaucoma another case in which experiments on animals have been of use?—Yes, glaucoma is a very prevalent disease and a very serious one. The nature of glaucoma is that the eyeball gets too full. When there are just enough of the fluids within the eye and not too much, they keep the eyeball at a certain tension, and if you put your finger upon your eye you can feel that it is slightly soft. In glaucoma the eyeball gets so full that if you put your finger on such an eyeball it feels as hard as a marble; in fact, the intra-ocular pressure is greatly increased. The idea at first was that there was too much fluid secreted, that glaucoma was a disease of hyper-secretion; or, perhaps, if there was a low form of inflammation, it was the fluids given out by inflammation which caused the increased tension. Von Graefe, about the year 1867, discovered a cure for glaucoma, and he discovered this cure, not by experiments upon animals, but by clinical observation, because, after all, every advance that we make is not made by experiments on animals. Although experiments on animals teach us a vast deal, yet other methods are also not forgotten; and it was by means of clinical observation that Von Graefe, about the year 1867, discovered that iridectomy cured glaucoma (iridectomy is taking away a little piece of the iris), and the way in which it acted, he thought, was because it took away a little piece of the secreting surface of the eye; because at that time we did not know, as we know now, that it is the ciliary processes which secrete the intra-ocular fluids, and it was thought that it was by reducing the amount of the secreting surface within the eye that the tension of the eyeball was reduced. Deutschmann's experiments showed, of course, that the iris secreted very little, but the ciliary process secreted the main portion of it. But it was in the year 1873 (and to this I particularly wish to draw attention) that Leber discovered, by experiments on animals, that the way of exit of the intra-ocular fluids was at the angle of the anterior chamber, the part that we call the ligamentum pectinatum; and he discovered that this is where the intra-ocular fluids make their escape; because in our eyes, normally, these intra-ocular fluids are always being secreted, and they must be also getting away, or the eyeball would become too full. It is obvious that they must be getting away at the same rate as they are being secreted; and fresh fluids being secreted, when they become used, are discharged from the eye, and the place where they are discharged is the angle of the anterior chamber, as Leber pointed out in the year 1873. A few years after that it was discovered that in the glaucomatous eyes of individuals, which had been removed for one reason or other, the angle of the anterior chamber was blocked, and then it became at once plain that it was the blocking of the way of exit of the intra-ocular fluids which caused glaucoma, and that therefore it was not a disease of secretion, as was formerly thought, but a disease of retention.

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9837. Do these fluids escape, then, at the surface of the eye?—They do practically escape at the surface of the eye.

9838. (*Colonel Lockwood.*) Can you cure glaucoma now?—Yes. I was just going to talk about that. The cure of glaucoma previously had been, and continues still, for the matter of that, to be mainly iridectomy, but, as I said before, it was thought that the way iridectomy acted was in consequence of taking away the little piece of the iris. Now that we know that this is not the way the operation acts, and that it is by freeing the angle of the anterior chamber, we make an incision into that very angle, and we know that we must make this incision in a certain way; for it is not the taking away of a piece of the iris that is the important part of the operation, but it is the incision, and the way in which the incision was made. This discovery of the blocking of the anterior chamber as being the cause of glaucoma, which followed on the knowledge that that was the way of the fluids escaping has led also to the use of a very important remedy, or, at any rate, a palliative, in these cases—that is eserin, the active principle of the Calabar bean. Eserin contracts the pupil. It was immediately thought that if the angle of the anterior chamber is blocked, then if you put eserin into the eye, and contract the pupil, that will drag away the root of the iris from the angle of the anterior chamber, and in that way free the angle of the anterior chamber, and then the fluids will be able to get out again instead of being pent up in the eye. And this is a remedy which we use very much. It can hardly be called a remedy, but, at any rate, it postpones the evil day ,and it is a very great aid in many cases.

9839. (*Chairman.*) Is it used successfully?—Yes, successfully. The use of eserin in contracting the pupil was discovered by Fraser, of Edinburgh. I do not know how long ago, but, at any rate, it, too, was discovered by experiments upon animals. So that here you have a number of experiments upon animals all bearing towards the cure or alleviation and understanding of one very important disease of the eye. I might say perhaps that in this country very important work has been done by Parsons, Henderson, Starling, and Paterson regarding the normal secretion and excretion of the eye; that is the branch of knowledge improvements in which led us to understand so much about glaucoma.

9840. Were those experiments of pure research, or were they addressed particularly to the cure of glaucoma?—For the most part they were experiments of pure research. Those I spoke of, by which Leber discovered the way of exit at the angle of the anterior chamber, were pure research experiments.

9841. Then I see that you propose to make some observations on two diseases, namely, cataract and detachment of the retina?—Of course, it would be very desirable to know the cause of cataract—senile cataract; and investigations by experiments upon animals are now being made on this subject. Römer has investigated the question of lentotoxins, or cytotoxins, and the protection of the lens against them in normal conditions. Hess's experiments lead him to suspect that the cause of cataract is some auto-intoxication. These are quite new; they are going on now, in fact. Römer's experiments were published in the 60th volume of von Graefe's Archives, and that is quite a

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recent volume; but I mention these experiments because they are quite new; and they have not resulted yet in anything, but there is every reason to think that we will gain knowledge, and the experiments have to be continued. Detachment of the retina is also a very serious disease of the eye. The retina becomes separated off from the choroid coat of the eye by the intervention of fluid between the two. It was discovered by Leber in 1882 that the primary cause of detachment of the retina is shrinking of the vitreous humour. The disease is an exceedingly difficult one to treat; it is one of the most hopeless that we have, and attempts are being made to replenish the vitreous humour, by introducing the vitreous humour from animals, and some of those cases have resulted with advantage.

9842. As regards the next three matters—cocaine, adrenalin, and argyrol—I think we have had a great deal of evidence upon that class of subject, and unless you think there is anything very special regarding them, we need not perhaps go into them in detail?—I do not think that I need trouble you in that case.

9843. (*Sir Mackenzie Chalmers.*) We have not heard, I think, before of argyrol?—Argyrol is a preparation of silver, and it is used very largely for external diseases of the eye—diseases of the conjunctiva.

9844. Has animal experimentation had anything to do with it?—Yes.

9845. (*Chairman.*) You might tell us about it?—Experiments upon rabbits showed that when argyrol was introduced into the anterior chamber of a rabbit's eye it did no harm; it did not set up any inflammation or anything of that sort; and as it was known that it was good for certain bacterial diseases of the conjunctiva, a friend of mine, Dr. Mooney—one of the junior surgeons in the hospital that I belong to in Dublin—thought he would try what good it would do in a very severe case of inflammation extending into the interior of the eye in a case of injury, and he introduced in this almost hopeless case some drops of argyrol into the anterior chamber, with the result that in the course of twenty-four hours the eye was a great deal better, and in the course of twenty-four hours more was quite well. I regard that as a very brilliant result from the use of a remedy which had never been used in that way before, and which he would not have used if he had not learnt that it, when introduced into a rabbit's eye, did no harm there.

9846. It was a painless experiment on the rabbit, I presume?—I suppose so. I cannot give details. Dr. Mooney had read of it somewhere, and I asked him before I came away where he had read it, and he said he would look it up and tell me, but he had not done so before I came away.

9847. It does not bear so much upon the question of painful experiments on animals?—No.

9848. (*Sir Mackenzie Chalmers.*) But he dared not have used it if it had not been for experiments on animals?—No.

9849. (*Chairman.*) But it does not seem to involve anything in the nature of an experiment. What have you to say with regard to the Finsen light?—The Finsen light, as you know, is a very valuable remedy in certain serious superficial cancers, lupus, and so on; it is a bacteriacide in those cases, and it seemed that it might be useful in certain bacterial diseases of the eye. But the Finsen light, as used in the usual way for lupus, could not possibly be used for the human eye, because it would do it a great deal of harm. The eye has to be carefully protected in using the Finsen light, I believe, for lupus of the face. Yet it seemed so likely that it might be of use if its intensity could be subdued, that experiments were made in the first place to see if it would kill any of the bacteria that infect the human eye (there would not be any use in using it if it did not), such as the streptococcus, the pneumococcus, and so on. Hertel has tried upon animals' eyes whether it would kill these bacteria if used in a modified form.

9850. Without destroying the eye?—Without destroying the eye; and he has found that that can be done; consequently, perhaps, we will come to use it in this modified way for the human eye.

9851. That, I suppose, is a thing which would involve, in perfecting experience, painful experiments on a certain number of animals?—Certainly, because you must infect the animals' eyes with these bacteria in order to perform the experiments.

9852. That is a case of painful experiments which have had some useful result?—Yes, which promises to have some useful result.

9853. (*Colonel Lockwood.*) You hold, of course, your own opinion on this subject of painful experiments on living animals, apart from that held by your Council, naturally?—Yes.

9854. Do you trace your own success as an eye specialist entirely, or the greater part of it, to experiments made on living animals?—A great deal of it.

9855. Did you witness many experiments?—No, I did not.

9856. Or did you conduct yourself any experiments?—No; I am a practitioner; I merely utilise in practice the knowledge which is gained by these experiments.

9857. It was more from what you read of experiments than from anything you witnessed yourself?—Certainly. It is not necessary to witness them in order to know how to utilise the results obtained from them.

9858. You think that reading about an experiment is quite as useful to the profession as witnessing experiments?—I do not know quite what sort of experiments you mean.

9859. Experiments upon living animals?—The experiments I have spoken of here have been for the most part pathological. There were some, as the meaning of the endothelium on the back of the cornea, and the use of the ciliary body, which were physiological. But I think, of course, that it is not necessary that in every discovery in physiology which is made by means of experiments upon animals those experiments should be always seen by every person who is a doctor; that would be quite impossible.

9860. I understood you to say that you were in favour of demonstrating to students by means of experiments on living animals?—Yes, certainly.

9861. And you would be sorry to see them put a stop to?—I would, indeed.

9862. Do not you think that that knowledge could be arrived at, as it was in your own case, as much by reading as by witnessing experiments?—The experiments which are shown to students relate to the great functions of the body, some of the greater functions of the body, such as the action of the heart, peristalsis of the bowels, and such things as that. They certainly are things which ought to be taught to students in the most demonstrative manner possible.

9863. But I may take it, indeed, it is well known, that you yourself have arrived at considerable eminence in your profession, and yet you say you have witnessed no experiments?—It would have been a very good thing for me if I had.

9864. You think you would have been a still greater man?—I am sorry I did not see still more of them.

9865. Did you see any in Germany?—I saw the beginning of Cohnheim's experiments, and they certainly were something which impressed the nature of inflammation upon my mind more than anything I ever saw in my life. I only wish I had seen more. When I was a student these things were not to be seen, and therefore, of course, I was at a disadvantage.

9866. You said, of course, that you were, and I take it naturally that you are, anxious that the animals during these operations should be insensible to pain?—Yes.

9867. But you rather qualified that statement, because you said it must be consistent with the success of the operation?—Yes, that is, within the four corners of the present Act of Parliament.

9868. Therefore I take it—I do not want to put an unfair question—that your view is that the success of the experiment on the animal is of more value than that the animal should not suffer pain?—Yes, I do most distinctly think so.

9869. And you also think that these operations which you have described to us as being very successful, and which have been derived from experiments on living animals, could not have been arrived at by any clinical examination?—No, I cannot imagine them to have been arrived at by any clinical examination.

9870. Or by any operation on a dead body?—No, oh, no, certainly not.

9871. Only and solely by experiments on living animals?—Certainly.

9872. You did, I know, and very rightly, draw our attention to the fact that the use of iridectomy in glaucoma was by Von Graefe stated to have been not discovered by experiments on animals?—Yes.

9873. Which do you consider, if it is a right question to ask you, are the most skilful operators on the eye, the Germans or ourselves. I do not wish to press it. Would you say that on the whole we are on a par with the Germans. You understand why I ask the question, of course?—I cannot imagine why you ask it.

9874. I will tell you why. Because what I am going to suggest to you is that the Germans are under no restrictions, and we ourselves are under very strong restrictions; and if we are equally as good as the Germans, these restrictions on experiments on animals have not been hurtful?—But are you asking me this with regard to what I suggested, that surgeons should be permitted to operate on animals for the purpose of obtaining skill?

9875. Partly that; but partly I want to know this: Do you think that the regulations laid down by the Home Office to prevent suffering in animals have hampered our science in England?—Well, I really do not think I can say that they have. I think that they are sufficient, except in regard to that one restriction, that our surgeons should be allowed to operate for the purpose of obtaining skill.

9876. But under licence always?—Yes, I do not mind about that.

9877. But otherwise you would say that we were equally in line with the Germans as regards operations?—I do not think we can say that our restrictions have interfered, but if they were made any more strict than they are I have no doubt that they would.

9878. You think that would be a distinct disadvantage to us?—A distinct disadvantage.

9879. And you could not for one moment entertain the idea of leaving out of experiments which are allowed to be made, animals such as dogs, cats, or horses?—Or monkeys, certainly not.

9880. You think they are absolutely necessary?—I do.

9881. (*Sir Mackenzie Chalmers.*) I have not many questions to ask you. Your evidence here to-day represents, I understand, the practically unanimous opinion of your Council?—Yes.

9882. And going outside your Council, among the Fellows is there any minority, or a considerable minority, opposed to experiments on animals?—I am not acquainted amongst our Fellows with any views contrary to experiments on animals, except those of Sir Thornley Stoker upon one particular point.

9883. Those are demonstration experiments?—Yes.

9884. But we want to get evidence on both sides, of course, and I was wondering whether you could suggest to us any eminent Fellow who would give evidence on the other side?—I do not know of any of our Fellows who could give you any evidence of that sort.

9885. Practically, then, your evidence is the unanimous opinion of the Council and the unanimous opinion of the Fellows of the Royal College?—So far as I know it, and I think I would have heard if there had been any other view.

9886. Sir Thornley Stoker confines his objection to doubting the necessity of experiments before students as part of their general training?—Yes.

9887. Was he the dissentient that you referred to?—Not if you mean the gentleman who I said I thought had changed his mind. I said that there was one member of the Council (Sir Thornley Stoker is not now a member of the Council) who rather inclined to Sir Thornley Stoker's view at one time, but that I believed he now holds that opinion no longer.

9888. Now, then, as regards the point where you suggest an amendment of the law, you suggest that students who are about to become practising surgeons should be allowed to acquire manual skill by experiments on animals?—Yes.

9889. Would it satisfy your requirements if the animal was anæsthetised and then killed before it recovered from the anæsthesia, or would you require the animal to be allowed to recover?—It would not be desirable to let the animal recover in every case. For instance, if a surgeon wished to try some new method of suturing when a portion of the intestine had been excised, and he wished to see whether it was a good form of suture, and in many other cases. I simply give that as an example. There must be plenty of other operations where it is necessary that the animal should be allowed to live and should not be killed until an interval had taken place, in order to see what the actual result had been in the method of healing.

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9890. That, if I may say so, would be a scientific experiment and not an experiment for the purpose of acquiring manual skill. As regards a young surgeon acquiring manual skill, would it satisfy your requirements if the animal were anæsthetised and killed before it recovered consciousness?—I presume that it would; but, really, that is a question which would, perhaps, be better addressed to a general surgeon.

9891. However, so far as you know, it would be sufficient?—Yes, so far as I know.

9892. I suppose eye surgery requires very delicate manual operation?—Yes.

9893. May I ask this question: Does skill become progressive?—Yes.

9894. One of the witnesses told us—I will not ask you if you agree with him—that before an ophthalmic surgeon became a really good surgeon he had to destroy a hatful of human eyes?—That is not an original saying of his; I fancy it was Dr. Mackenzie, of Glasgow, who said that long ago. But it is not true, of course.

9895. But there is still this element of truth in it, that a man's skill is progressive?—Yes.

9896. Do you think that a student, by operating on an anæsthetised animal which was killed before it recovered from the anæsthesia, would acquire skill in operating on the eye?—No, I do not. I do not think, with regard to the eye, there would be anything gained. I am not aware of any operation upon the eye, except the transplantation of the cornea, that I can think of where it would be of any advantage to the student or of an ophthalmic surgeon to operate on a living animal's eye.

9897. You think that he had better begin straight away on the human subject?—No, not straight away, but he can operate upon dead eyes. But I do not think that anything would be gained by operating on a living animal's eye. I think the operations I spoke of are those of general surgery, and more particularly those upon the intestines.

9898. How far are diseases of the eye common to human beings and to animals?—I am told by Professor Mettam, of Dublin, who is head of the Veterinary College there, that all the diseases of the human eye, except certain infectious diseases peculiar to man, also affect the eyes of animals.

9899. You have a common run of disease between animals and human beings?—Very much, it seems.

9900. I ask the question, because it has been suggested to us by certain witnesses that experiments upon animals are useless, inasmuch as an animal is so different from a human being that you can draw no useful inference. You do not agree with that statement?—I do not. One of the diseases that I have spoken of at considerable length here—glaucoma—is not at all uncommon in animals—especially horses are very liable to it; I have seen a horse myself with glaucoma, and I have used eserin for it, because it was in great pain, and relieved its pain in a few hours.

9901. Has iridectomy ever been performed upon a horse?—I do not know. I think if a horse is blind of an eye it is hardly worth doing any operation upon it except removing it. That is mostly what the veterinary surgeons do. A blind eye in a horse, or a nearly blind eye, is of no use.

9902. Horses suffer from cataract, of course?—Yes.

9903. Have we obtained any information, from observations on horses or experiments on horses, with regard to cataract?—I do not think we have.

9904. Is transplantation of the cornea a recognised operation now?—It is, but it is not very often done, because the cases where it suits are not very frequent. It is still in the developmental stage. We hope after a time to be able to use it more extensively.

9905. That, of course, does not come under the Vivisection Act; it is not done for the purpose of knowledge. It is a common operation on human

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beings?—Yes, but then the preliminary knowledge of it had to be gained. We could not do it on the human being until we had tried it on an animal.

9906. As regards what you were asked about yourself, I take it that your knowledge of the functions of the eye has been largely obtained by experiments on animals, and you apply that knowledge in practical surgery to the cure of human disease?—Yes.

9907. May I ask you, is trachoma a common disease?—Yes.

9908. What is the nature of trachoma? It is highly infectious, is it not?—It is very infectious; I would not like to say highly. It is a disease of the conjunctiva, the lining of the lids, and it is little bodies which form in the conjunctiva, called trachoma bodies, which destroy the particular spot in which they sit, and then the whole conjunctiva becomes destroyed and the cornea becomes opaque, too—through a growth on it called pannus—and it is for clearing the cornea that jequirity is used.

9909. Do animals suffer from trachoma?—I believe pigs do very much; I do not know about other animals; I understand that pigs do.

9910. Is it known what the source of the infection is in trachoma?—No, it is not; the micro-organism has not been found.

9911. Are there any experiments on animals at present being carried out with reference to the treatment and the source of trachoma?—I am not aware of them.

9912. Still, if it is a disease that animals can suffer from, information might be obtained, of course, from experiments on animals?—Yes.

9913. Do you operate chiefly under cocaine or under general anæsthetics?—Mostly with cocaine. I put drops of cocaine into the eye, and then the eye becomes anæsthetised, and you can operate without giving the person any pain.

9914. (*Mr. Tomkinson.*) That is a purely local anæsthetic?—Yes.

9915. That is quite effective, is it?—Yes, in cataract operations and iridectomy and such operations.

9916. (*Sir Mackenzie Chalmers.*) I only ask you for the purpose of a further question. In experiments on the eyes of animals it has been said by previous witnesses that the experiments are of a very painful character. But can the experiments which have been of use to you be carried out under cocaine; can the animal be anæsthetised?—The animal would require general anæsthesia; because it is not merely to do away with the pain, but it is also that it does not like to be restrained in any way. It is like a child. In many things that one has to do for a child one has to give general anæsthesia; whereas if you do the same thing for an adult, if you simply use local anæsthesia, the intelligent sense of a grown person will make him not resist. But a child will not be restrained, even to hold it without doing anything to it; it does not like it.

9917. Do you think that in experiments on animals cocaine ought to be allowed as an anæsthetic; or ought animals always to be put under general anæsthesia?—I would think that they ought to be put under general anæsthesia.

9918. Chloroform or ether?—Yes.

9919. (*Dr. Gaskell.*) I presume that in your student days, when you learnt physiology, there were practically no demonstrations, no experiments on animals at all?—I never saw them and never heard of them.

9920. So that naturally you would not have witnessed any in your student days?—I would not.

9921. There is one question I was wondering whether you could give us any information about—that was, with respect to certain experiments performed by Dr. Cecil Shaw for sympathetic ophthalmia. Do you know those experiments?—I know that he did perform some experiments some years ago, and they were published in the "British Medical Journal"; I think they were in connection with sympathetic ophthalmitis.

9922. They were?—I think he used jequirity in some way, jequirity beans.

9923. Were those done in Ireland?—They were done in Belfast.

9924. You had no personal knowledge of them?—I had not any except what anybody can read in the "British Medical Journal."

9925. I presume that experiments of that kind must necessarily be somewhat painful to the animal?—I certainly would suppose that they would be painful to the animal.

9926. In setting up inflammation in the eye and keeping the animal alive to see whether sympathetic ophthalmitis resulted?—Yes, it would be painful.

9927. Do you know whether the results were valuable?—No, I cannot say that I remember them well enough to say anything about that.

9928. Still, there can be no doubt that, even at the expense of some pain to the animal, it is well worth while, most important in fact, to find out the meaning of sympathetic ophthalmitis?—Most important indeed. Dr. Shaw desired by this means to find out whether or not the inflammatory process extended round by the optic nerve and commissure; but I forget now really what the result was.

9929. (*Mr. Tomkinson.*) I have hardly anything to ask you. In addition to what Colonel Lockwood asked you with regard to your qualification, as to the extent to which you subordinate the pain of the animal to the results derived from it, would you push that theory to an extreme in your opinion? For instance, supposing it was impossible to give an animal anæsthesia at all, would you place any limit upon the suffering which you thought it justifiable or right to inflict in order to promote research?—I think that the ascertaining of knowledge is to be put before the suffering of the animal; that is my own individual opinion. But I would always endeavour to the utmost to safeguard the animal from any pain, or even inconvenience, which could be avoided.

9930. So far as you know, that is practically the custom that is carried out so far as possible?—Yes, it is. In fact, so far as I know, animals do not suffer any pain during operations; and after operations which are done aseptically, as they all are, the animals also do not as a matter of practice and as a matter of fact suffer pain or inconvenience, I believe.

9931. I think that is very much the experience of human beings after a mere incision; that under aseptic and healthy conditions the healing up is accompanied by hardly any pain?—Yes.

9932. Have you any doubt in your own mind as to the possibility of keeping animals absolutely in a condition of insensibility for a prolonged period by anæsthetics?—I have no doubt in my own mind, because I know it on good authority; but I have no experience of my own to tell you.

9933. I think you have witnessed very few experiments on animals?—Yes.

9934. Is that from choice?—Not at all.

9935. Would you have any shrinking from it?—Not the slightest—no more shrinking would I have from seeing an animal operated on or experimented upon than I would have in seeing an operation in the theatre of a hospital.

9936. Then you do not look upon it at all in the light of being a cold-blooded and cruel practice?—Not at all—by no means; certainly not, because it is necessary. It would be very different if I saw an animal being stoned by little boys out in the street; I should think that a very beastly thing to witness; but I see nothing at all to shrink from in scientific experiments, carefully carried out in a physiological laboratory, with every precaution for the animal and for the success of the experiment.

9937. (*Dr. Wilson.*) You said that experiments on animals had assisted largely in the cure, or treatment, I should say rather, of glaucoma?—Yes.

9938. Is that disease ever cured?—Yes, it is.

9939. Even if it is far advanced, can it be cured?—No; when it is far advanced, if you allow a certain time to go past, you cannot do anything for it.

9940. It is only in the early stages that it can be cured?—Only in certain rather early stages.

9941. In diagnosing tubercle of the eye, I think you said that it was of great value to be able to inject the aqueous humour from a suspected eye into the eye of an animal?—Yes, for the purpose of diagnosis.

9942. But it is believed, is it not, that the tubercle bacillus is the only infecting agency, that the tubercle bacillus must be in the aqueous humour which you inject into the eye?—Yes.

9943. Could it not be developed, as it is ordinarily, and diagnosis made without having recourse to that experiment?—I do not quite understand.

9944. I mean that the tubercle bacillus, for example, in the sputum, is developed in artificial media?—Yes.

9945. And the bacteriologists never resort, so far as I know, to inject it into the eye?—Yes.

9946. Why could not the same test be made with regard to the aqueous humour which you suspect?—Because experience shows that it is not always successful. The tubercle bacillus in tubercle of the eye is very often in a very attenuated form, and you cannot cultivate it in the same way as you do the more active bacillus which occurs in pulmonary tuberculosis.

9947. It cannot be diagnosed in the ordinary way?—Very often not. I do not mean to say always, but it is often so with what we call attenuated tuberculosis of the eye. Those are the cases where you are likely to have doubts sometimes.

9948. Now with regard to the transplantation of the cornea, is it not the case that only one single instance of cure has been recorded by Zirm in 1905?—That is a very recent case. Zirm has published that case very recently, but Von Hippel published two cases on the human eye.

9949. But these are the only recorded instances of success?—Yes, but they are quite sufficient to show that the thing can be done.

9950. But have not oculists tried the experiment very often on human eyes?—I do not think so, because the cases in which it is applicable at present are not many. That is probably the reason why there are so few cases. We hope to develop it as time goes on. We have made a considerable advance in it now.

9951. The eye, of course, is a very sensitive organ to pain?—Yes.

9952. Would not the insertion of foreign substances into the eye for experimental purposes be likely to produce a great deal of pain?—Yes, they would, as a rule.

9953. And, of course, in experimenting on animals, although for cutting operations cocaine or anæsthetics may be used in the first instance, they could not be used continuously while the animal was under observation?—No.

9954. So that the animal must be more or less in a suffering condition while under observation?—No doubt there would be suffering in many instances.

9955. Then with regard to antiseptics, I suppose you have to be very careful about the use of them in operations on the eye?—Yes.

9956. So that what was called Listerism, or antiseptic surgery, never applied really to those operations. You could not operate under the spray on the eye?—No, you could not.

9957. Did you take any part in that controversy about Listerism some 25 years ago?—I did not take any active part.

9958. But operations on the eye must all along have been aseptic operations to be successful, clean operations?—Yes. We used also at the beginning to use antiseptics at one time, but they were not satisfactory; and then when the aseptic system came more to the front we were delighted to be able to abandon the other method, because the lotions which were used were often deleterious to the eye in themselves.

9959. So that what is called aseptic surgery really was not the direct outcome of antiseptic surgery as introduced by Lord Lister?—I am not so sure about that.

9960. Did he not himself own that he was rather ashamed and, with rare candour, regret that he ever used the spray?—I am afraid those are questions it is better not to ask me so much about, for I am not quite so familiar with them as perhaps a general surgeon would be.

9961. I only want to show, if possible, that eye surgery all along had to be clean surgery to be successful, even before what is called the aseptic treatment came into vogue?—Yes.

Mr. (now *Sir*) *H. L. Swanzy*, P.R.C.S. (Ireland).

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9962. Of course, when you refer to serum treatment, you refer only to it in connection with eye diseases?—Yes, that is all I refer to, only in connection with eye diseases.

9963. You are not commissioned to go further?—I have a perfectly free hand; but I consider myself to be as a witness more useful when I speak of what I know in my own branch than of what you would learn better from somebody else.

9964. Is it your experience that tuberculin is valuable in cases of tuberculous eye disease?—Most distinctly. I have had a good deal of experience of it in that disease.

9965. And it is used sometimes for testing when you are not certain?—Yes, I used it so the other day.

9966. Injecting it into a patient?—Yes, I did it the other day.

9967. Has it not failed as a test with human beings; has it not been given up?—I know that it was very much out of repute for some time, but it is obviously coming back into use again now—used not quite in the way it was at first; and I know from my own knowledge and experience that for tubercle of the eye it is a most useful remedy.

9968. Then in referring to operations for acquiring skill as regards students, you refer to general surgical operations?—Yes, I do.

9969. You say they are of no value as regards eye operations?—I do not think that practically they will be of much use as regards eye operations.

9970. Then you do not agree with Professor Starling in that respect?—I do not think I can say that they would be of use in regard to eye operations.

9971. But would you go so far, then, as to advocate what is pursued in one or two colleges in America, that is to say, using dogs as patients for amputations and all other surgical operations, and treating them as patients—I mean, keeping them under observation after the operation?—I do not think that there should be any restriction upon the kind of operations that should be performed upon animals, if it is held by surgeons that they could gain skill in operating upon the human subject by that means.

9972. But would you advocate students being allowed to perform them?—If they can learn something from it towards operating upon their patients when they get their qualifications.

9973. For acquiring operative skill?—Yes. I believe, however, that the great gain would be in intestinal surgery more than what you mentioned just now, amputations, and things of that sort.

9974. (*Dr. Gaskell.*) Might I ask one question in connection with something you have just said? Do you distinguish different parts of the eye as to the amount of pain that is likely to be caused; are all parts of the eye equally sensitive to pain?—Oh, no.

9975. The vitreous humour, for example?—That would have none.

9976. The retina?—There is no pain.

9977. The internal chamber of the eye, unless enlarged by pressure?—The internal chamber of the eye is the vitreous humour, and it is not painful. There is no pain in the vitreous humour.

9978. Is not the pain in the eye mainly a question of the cornea and the conjunctiva?—The cornea and the conjunctiva; and the iris is very painful to be touched, and the ciliary body is very painful to be touched.

9979. (*Sir Mackenzie Chalmers.*) There is one question I forgot to ask you; have you any opinion upon this point—whether licences and certificates ought to be granted to physiologists who are not medical men? Have the Royal College any opinion upon that point?—I have not heard it spoken of.

Mr. E. A. Schäfer, LL.D., SC.D., F.R.S., called in; and Examined.

Mr. E. A. Schäfer, LL.D., SC.D., F.R.S.

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9980. (*Chairman.*) You have been appointed by the Royal Society of Edinburgh as their representative?—Yes. I have also been appointed by the University of Edinburgh as their representative. I was not specially summoned to-day on behalf of the University of Edinburgh, but if it is convenient to the Commission, I can give evidence on behalf of the University of Edinburgh as well.

9981. To a certain extent, I suppose it covers the same ground?—Practically it covers the same ground.

9982. You have been kind enough to send us a print of what I take to be the message of the Royal Society of Edinburgh?—Yes.

9983. Could you give us just a short account of that Society. Is it a Society under Royal Charter, like the Royal Society of England?—I presume so. I do not know much about the history of the Society, but it is organised on exactly the same lines as the Royal Society in London.

9984. That is sufficient. Therefore it devotes itself to science generally, not to any particular science?—Yes, to science generally.

9985. And, amongst others, to physiology and pharmacology?—Amongst others to physiology, pharmacology, and the medical sciences generally.

9986. Has the body which requested you to come and give evidence a council?—Yes.

9987. Can you tell us what that council is; is it elected by the general members?—By the general members annually.

9988. How many are there on the council?—I cannot say positively. I should think about 15 or 20, including vice-presidents and the president.

9989. Are you a member of the council?—I am.

9990. Were you present at the meeting at which these views were drawn up?—I do not remember being present at the meeting.

9990A. Does this statement represent the unanimous view of the council?—I understand that there was no other opinion expressed at the meeting. I believe it represents the unanimous view of the council as the council was then present—of course, I cannot speak for every individual member; and it has been more than once before the council. Although I am not quite sure whether I was present at the first meeting of the council at which the subject was considered, I was a member of a sub-committee which was appointed to draw up this statement. I did not draw it up myself. I was not the convener of the committee, merely a member of it.

9991. As this represents the views of the Royal Society of Edinburgh, perhaps we had better take it as it is printed. Would you kindly read it?—"Statement by the representative appointed by the Royal Society of Edinburgh to appear before the Royal Commission on Vivisection.—The Royal Society of Edinburgh, being deeply interested in the advance of all branches of science, values the opportunity which has been given to it of expressing its views upon the important question which is at present being considered by the Royal Commission on Vivisection. From very early times the experimental method of investigation has been practised in physical and biological science, and it may be regarded as affording the most fruitful means of obtaining accurate information in both of these fields. In the latter department this method necessarily involves experiments on living organisms. The whole fabric of physiological knowledge rests on such experimentation, and those who are conversant with the general trend of physiological research, know that results of the highest importance to the welfare of mankind have thereby ensued. From the time of the discovery of the circulation of the blood to the recent brilliant results which have so greatly enhanced our acquaintance with the functions of the brain and nervous system, the history of this branch of science reveals a long list of remarkable discoveries achieved by this method of inquiry. The knowledge which has been thus acquired has had a material effect in procuring the cure of disease and the relief of suffering, and has thereby been the means of saving and prolonging innumerable lives. In the field of pathology, the advance which has been made by means of the experimental method has been no less striking. Within the last thirty years the methods of the physician and surgeon in combating disease have been completely revolutionised. It is now possible to attenuate and perhaps destroy the virulence of many of the deadly complaints which afflict mankind. A new science—bacteriology—has sprung into existence, largely through experimental effort, and a new light has been shed upon many departments of pathology which were formerly obscure. Tubercle, diphtheria, yellow fever, tetanus, hydrophobia, anthrax, malaria, sleeping sickness, typhoid fever, and other diseases," among which I would include Malta fever, Nagana, and plague, "are now better understood, and by aid of the information gained by experiments on animals, most of them can be successfully grappled with." "The relation of man to his surroundings has thus been modified, and he now enjoys a degree of immunity from many of the dangers by which he is surrounded. In the sciences of pharmacology and therapeutics no advance in accurate knowledge is possible without experiments on animals. The action of drugs must be first tested in them, and the physician is guided by the information which is thus obtained. The Society, while fully admitting that every care should be taken that in the prosecution of experiments on animals no unnecessary pain is inflicted, is, at the same time, of opinion that the imposition of any further restrictions would impede the progress of physiological and pathological research, and would be attended by serious results to the progress of medical science and to the well-being of the community at large. Further, the Society would strongly deprecate any attempt to draw a distinction between experiments which are designed for utilitarian purposes and those which appear to have a purely scientific object in view. The history of science shows that it is not possible to draw such a distinction, seeing that so many scientific discoveries, which at the time they were made appeared to possess no utilitarian value, have in the course of time proved to be but the beginning of lines of investigation which have led to the acquirement of knowledge of incalculable benefit to humanity."

9992. Those are the views which have been submitted to the Council and approved by them, I understand?—Yes.

9993. The Council consisting, I suppose, of gentlemen who may be said to be representative of a considerable number of sciences?—Of all departments of science.

9994. I see at the end they advert to the desirability in their opinion of allowing experiments for the purpose of what we call pure research as opposed to research in order to discover a remedy for a particular disease; but I should like to ask you a little further: have your Society expressed the opinion as to whether or not experiments on animals should be used for purposes of demonstration in lectures to students?—No, that is one of the points to which I referred when I drew your attention to the fact that I was here to represent both the University of Edinburgh and the Royal Society. I consider that evidence on this point would be better tendered on behalf of the University of Edinburgh.

9995. Then I will not ask you anything further about the Royal Society, because I understand that these are their views expressed, and as they are not specially a medical body the importance of their view, I think we should all be of opinion is, that it is the view of scientific men generally on the value of experiments?—Yes.

9996. And specially in this case?—Yes.

9997. Perhaps any other evidence which you have to give we may take as being given on behalf of the University of Edinburgh?—I think you may take it as being given also on behalf of the Council of the Royal Society, the greater part of it at least, because any other evidence I have to give is supplemental to their statement. I have been given more or less a free hand by the Royal Society to give evidence on their behalf.

9998. Are you yourself in practice in Edinburgh?—I do not practise medicine.

9999. As a physiologist?—Yes, I am Professor of Physiology in the University.

10000. And therefore the Royal Society of Edinburgh are perfectly well acquainted with your views?—Quite.

10001. And with the nature of your experiments and generally?—I think so. Most of the work which we do, or a large part of it, is brought before the Royal Society of Edinburgh, and is published in their Proceedings and Transactions. It is partly brought before the Royal Society of London, and published in their Proceedings and Transactions, and partly in other journals.

10002. Having told us that, and that you have a free hand from the University and practically from the Royal Society too, I might take the rest without speaking to you as a representative purely but as giving your own evidence?—Quite so.

10003. With regard to what I was asking you about, demonstration before students, do you yourself demonstrate before students?—Yes.

10004. Has it been your practice?—That has been always my practice.

10005. Has it been the practice in Edinburgh generally for the Professor of Physiology?—It has been the practice in Edinburgh ever since physiology was taught in Edinburgh, so far as I can ascertain. It was the practice of my predecessor, Professor Rutherford; it was my practice at University College, London, when I was Professor there, and it has been the practice at Edinburgh since I have been there.

10006. And I take it from that that you approve of the practice?—I not only approve of the practice, but I do not think it is possible to teach physiology properly without showing a certain number of experiments to students. I do not consider that students can properly appreciate the importance of certain branches of the subject unless they actually see experiments performed upon animals.

10007. Could you at all classify what those subjects are, whether there are any special kind of subjects that require it more than others?—Yes. One subject is the action of the heart, the effect of the action of the heart in maintaining the pressure of the blood and the effect of certain nerves in influencing the heart and in influencing the blood vessels. The mere reading or hearing about these effects is an absolutely different thing from seeing them. I can myself very well remember the influence that was produced upon my mind when I first had shown to me by an experiment the effect of stimulating the vagus nerve upon the heart.

10008. Was that in London University?—That was in University College, London, when I was an advanced student.

10009. Who was the Professor then?—The Professor was Professor Burdon Sanderson, who was then Professor there of Practical Physiology. I have noticed myself the great interest, the keen interest, which intelligent students take in these experiments; they are vividly impressed by them in a manner in which they could not be by any narration of the results. For example, while observing the heart in an animal, the animal being, of course, in a condition of complete anæsthesia and absolutely insensible to pain, if one stimulates the vagus nerve and sees the heart immediately stop, not in contraction but in absence of contraction, in inhibition, produces a profound impression upon the mind regarding the influence which nerves are capable of exerting upon the organs of the body. And so with regard to secretion if one has placed a cannula in the duct of a salivary gland and on stimulating a certain nerve observes the secretion pouring from the gland, the impressions which are caused and the interest which is aroused, are altogether out of proportion to the effect which is produced when a mere narration of the fact is made by a lecturer. Such facts are so strongly impressed upon the mind by observation that they are never forgotten; they remain as definite accumulations of knowledge in the mind of the student. There are a certain number of fundamental experiments which I hold that physiologists ought not to be deprived of, and which should always be exhibited to students. One of these I have just mentioned, the action of the vagus upon the heart. I would add the action of the sympathetic nerve upon the heart and blood vessels, and the action of the depressor nerve upon the central part of the nervous system which presides over the blood vessels. There are also, besides the experiments upon secretion, which I have already mentioned, which is due to Claude Bernard the remarkable experiments of Bayliss and Starling upon pancreatic secretion. These gentlemen have shown that the pancreas is capable of being stimulated to secretion by the injection into the vascular system of an extract from the mucous membrane of the intestine which contains the principle to which they have given the name of secretin. This principle, when it is injected into the blood vessels provokes an extraordinary flow of pancreatic juice, and they have shown without doubt that this is the natural mechanism for secretion of pancreatic juice in the body. The material is poured into the blood from the mucous membrane of the intestine, passes to the pancreas, and causes secretion of the pancreatic juice. Before their experiments we practically knew nothing of such chemical stimulation of glands, and they have opened up a whole vista of enquiry, and enabled us to take a much wider view of the subject of secretion than it was possible to take before. Then there is the subject of internal secretion. It is only within comparatively recent years that we have understood that certain organs in the body, the functions of which were previously unknown, have the property of passing into the blood substances which circulate in the blood to the different organs of the body, and which affect the nutrition of the body generally, or secreting glands, or the vascular system, or the nervous system. This subject of internal secretion is one of the very greatest importance. I know of no more striking experiment for demonstration than the very simple one of injecting into the blood vessels of an animal, in which the blood pressure is being taken, a small dose of an extract which is obtained from the supra-renal bodies. If a minute dose of a solution containing the active principle of the extract (not more than one part in 100,000, or less) is injected into the blood vessels of the animal it is seen that the blood pressure is sent up to an extraordinary height, the heart is stimulated to greatly increased activity, and a whole series of phenomena are produced which are due to the action of this drug upon the muscular and other systems in the body. Certain other internal secreting glands are also known to produce effects of a similar or slightly different nature. One of these has only quite recently been discovered; the influence which is exerted by extract of the pituitary body. The effect of this when injected into the blood vessels is, like that of the supra-renal, to cause a very marked contraction of the blood vessels generally, while it differs from that of the supra-renal in producing, not an accelerated action of the heart, but a steadying of the heart, a slower action of the heart, the force of which is, however, well maintained. And it has quite recently been found that, in addition to this action, the pituitary body has a specific effect upon the kidneys, so that if the kidneys are not at the moment secreting there may be an immediate and rapid secretion of urine. It is well known to surgeons that as the result either of the anæsthetic which is employed or of the surgical operation itself the kidneys often do for a time cease to secrete. Under these circumstances, if the kidneys in an animal are in this condition, the secretion being in abeyance, a small dose of this extract of the pituitary body produces an immediate and rapid secretion of urine. There can be very little doubt that this discovery will have an important influence in connection with the views which we hold regarding the secretion of urine, and in all probability will have an important influence upon the treatment of cases of suppression of urine. This is an experiment which can be shown with the greatest ease to a class, and upon an entirely anæsthetised animal; and I have myself noticed the impression that it produces in the minds of students who see it, an impression which cannot be produced by any amount of lecturing or display of diagrams. One other point I would specially mention as of importance for exhibition to men who are to be engaged in the practice of medicine and surgery, viz.:—the effect upon the movements of the body of stimulating certain parts of the brain. It is now well known in consequence of the experiments of Ferrier that when certain parts of the brain in monkeys are stimulated certain definite movements of the body are produced, and this discovery has had a far-reaching effect in moulding our views upon the functions of the brain. The original discovery was made by Fritsch and Hitzig in the dog. But it was owing to Ferrier's application of their methods to the monkey that physicians and surgeons have been enabled to apply to man results which have been obtained upon animals, because the conformation of the brain in the monkey, although

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simpler than that of man, is essentially similar, whereas the conformation of the brain in the carnivora (as the dog and cat) is different, and one cannot say that such and such a part of the brain, such and such a convolution, or such and such a fissure, represents exactly another convolution or fissure in man. In the case of the monkey and the ape we can make that assertion with the utmost positiveness, and now that these experiments have been confirmed and largely added to by the experiments of Sherrington and Grünbaum upon the brain of the chimpanzee and ape generally, their importance in relation to the practice of surgery and medicine in man has come out even more decidedly. This is a class of experiment which also makes a vast impression upon the student; he sees that what he has been taught in the lectures is not a myth or a fable; that it is an absolute statement of fact; he sees that the brain can be played upon, as it were, much in the same way as a musical instrument can; that by stimulation of this part a movement of the eyes is produced, by stimulation of that part a movement of the mouth, by stimulation of another part a movement of the finger or thumb, or of the toes. He sees for himself that this is so, and when he afterwards comes across cases in his practice in which there are convulsions due to some irritation of the brain, beginning in this, that, or the other part of the body, he knows what part of the brain is likely to be affected. These experiments—there are not many of them—do not require a large number of animals, because the experiments upon the heart and blood pressure and the effects of extracts and of the excitation of certain nerves can be performed in a single animal. The experiments upon secretion can be performed on one or two animals, the experiments upon the brain can be performed upon a single animal. Although a large number of facts can be shown, a very small sacrifice of animals is required. I may say that I have never found that horror of these vivisectional experiments which has been asserted by many anti-vivisectionists. It is entirely absent.

10010. (*Colonel Lockwood.*) Among the students?—Among the students; it is entirely absent.

10011. (*Chairman.*) We have had some witnesses who have told us of the great horror that is excited, and others have said, at least one or two, that great levity is excited. What have you to say with regard to that latter assertion?—I was not aware that levity was alleged, except in the book the "Shambles of Science," but I did not myself take that seriously.

10012. I do not think any other witness has said so?—Levity would not be permitted, I may say at once, by any class of students.

10013. Have you seen any tendency to that in lectures that you have either given or attended?—Never. The students with whom I now have to deal, and those with whom I had to deal at University College, were for the most part serious students, most anxious to learn. I do not mean to allege that there is not amongst such a class of students a man here and there who is desirous of finding something humorous in anything that a lecturer may say; and there are even some lecturers who even occasionally raise a laugh, but never in connection with vivisectional experiments. Levity might be shown at a lecture, but at the actual experiment and at the results of the experiment levity is impossible. Nobody who knows the class of students who study in Edinburgh could suppose for a moment that this would be tolerated. There is a remarkable public feeling amongst the students in Edinburgh which makes itself felt in the conduct of the classes. If your class sees that you are desirous of teaching them and of showing them experiments which are intended to add to their knowledge and to elucidate the facts which you have been teaching them in your lectures, they are absolutely quiet, and there would be no difficulty whatever, certainly during these experiments, in hearing the proverbial pin drop.

10014. Has that been your experience in London as well as in Edinburgh?—That was always my experience in London, but my experience in London is not of so great importance, because the classes in London were small in number, and a small class can be more easily controlled; but when one is dealing with large classes such as we have in Edinburgh one cannot control each individual student.

10015. I asked the question about London specifically because these accusations of extreme levity were made with reference to London demonstrations?—I should think things must have altered very much since my time if that were possible, because I never found in my long experience at University College the least tendency to levity.

10016. Both as student and lecturer?—Both as student and lecturer. I have now been teaching physiology in one capacity or another for 37 years, and I have a very large experience of classes of physiology and a very large experience of demonstrations and experiments, and I have never in a single instance found that there was the least tendency to levity when an animal was under experimentation.

10017. (*Chairman.*) You had just finished what you were saying about demonstrations, but I wanted to ask you this question. I understand from your evidence on that point that you think demonstration is useful for the purpose of showing to students decided facts, facts which are ascertained?—Yes, fundamental ascertained facts.

10018. And entirely ascertained facts?—Yes, entirely ascertained facts.

10019. Not matters of research?—No.

10020. And that, in looking at it in that light, it is not necessary to repeat the operations on many animals?—I do not understand you, because all experiments that are shown to students are repeated.

10021. But at one lecture you would experiment upon one animal?—Yes.

10022. To show the particular action of a particular drug, we will say?—Yes.

10023. You would not repeat that before the students on a number of animals?—No.

10024. But only at another lecture where you had occasion to show the same demonstration?—Yes.

10025. There have been some suggestions made that a number of animals are used up and thrown aside and others produced. What do you say with regard to that suggestion?—That is contrary to fact. No doubt the number of animals used for demonstration varies in different institutions. I imagine that in some they use more animals than we do in Edinburgh, because it is possible in some places, where there are fewer to see the experiment, to show more things, for there are some things that the student must be very close indeed in order to see; whereas in Edinburgh, where we have a large number of students, we show experiments to an audience of from 80 to 100.

10026. Is demonstration, in your opinion, useful with an audience as large as that?—Yes, in a specially constructed theatre. Our theatre is constructed exactly like an operating theatre. An operating theatre is constructed with a very steep auditorium, where the audience stand and look immediately down upon the operating table.

10027. But how far would the furthest person be?—He would not be much further than the length of this room.

10028. Say 20 feet?—The length of this room is a little more than 20 feet, I think.

10029. Say 30 feet?—I could give you the dimensions of the theatre,* but at any rate it has been arranged like an operating theatre, so that every member in the auditorum should be able to see most things. But in addition to that my practice is to allow the audience to file past. If, for example, the experiment is on the action of the heart or the pressure of the blood, and is going on continuously, they file past afterwards so as to be quite sure that they see the details of the experiment.

10030. But you could not, surely, at a distance of 30 feet, get any very useful idea as to whether certain secretion was being distilled from a certain vessel?—Yes, certainly, you can see the dropping quite easily; and the dropping is further recorded—there are special means of recording the falling of drops. The drop falls upon a lever, which effects electrical contact, and this moves another lever, so that the audience can not only see the dropping fluid, but can see its record on the smoked paper which is being moved along by clockwork. All this can be seen quite well in my demonstration theatre.

10031. Then there is another matter that I want to

* *The distance from the furthest bench to the operating table is 15 feet.* *E. A. Schäfer.*

ask you about. What do you say to the claim that is made by some, that experiments should be permitted for the purpose of gaining skill in operation?—I think that is rather a question for the surgeon than for the physiologist. I am speaking as a teacher of surgeons, but not as teaching them surgery.

10032. But still you are able to form an opinion?—Yes, I have formed a very decided opinion that it is in the highest degree desirable that any new operation should, if possible, be performed upon a suitable animal. For example, I know quite well that the eminence of Sir Victor Horsley as a brain surgeon has directly arisen from the experiments which he performed upon the brains of monkeys at University College many years ago. He would be the first to acknowledge that his reputation as an expert on that branch of the subject has arisen from such experiments. And obviously it would be an improper thing to attempt a new operation upon, say, the human intestine, without having tested the operation upon animals. I cannot conceive of this not being done. It would only mean the sacrifice of patients. I understand that the present law forbids it, and I am surprised that it is so. I had forgotten that that was the law myself, although I have studied the Act; I think that that portion of the law ought to be repealed. I suppose it was not contemplated in those days that such operations as are performed nowadays would be possible.

10033. What sort of operations do you think this sort of practice would be necessary or useful in, because the last witness, whose speciality was eye disease, seemed to think you would gain nothing for the purposes of operation on the eye from studying from the living object?—I am not able to say anything about the eye, because I am not an expert upon the subject, but I should have thought otherwise from general considerations; at any rate, I would rather myself not be operated upon by anybody who had not previously operated upon the eye in either animals or men?—I would not like to be the first person practised upon.

10034. Upon a living animal?—I would rather that the individual had operated upon other men.

10035. Everybody must take their chance of that; but I was speaking about a man beginning to practice operations on mankind without having previously experimented upon animals. The last witness said, I think, that in the matter of the eye, experiment on living animals was not an essential thing?—I really do not know. I suppose he has tried it himself. I suppose you asked him that question.

(*Sir Mackenzie Chalmers.*) He had not done it himself.

10036. (*Chairman.*) No, he said that he had not?—I cannot conceive, if the operation is done in precisely the same way, that an amount of knowledge might not be got from operation on living animals which could otherwise only be acquired by operating on man.

10037. But it would involve an immensity of practice upon animals if it is necessary, because I suppose you would say that a surgeon ought to experiment upon animals to acquire skill for every operation he would be likely to be called upon to perform?—I do not think so at all. I should not think so for amputation, for example, for one moment.

10038. Because amputating a rabbit's leg is not much use for amputating a man's leg?—No.

10039. But I was speaking rather of internal operations?—For such an operation, as in resection of a portion of the intestine, I do not think it is necessary nowadays to operate first upon animals, simply because the methods of operation have been so perfected, that anyone who has assisted in an operation will probably be able to do it perfectly well afterwards himself.

10040. What class of operation are you speaking of particularly?—I am speaking of any new kind of operation. Supposing, for example, no resections of intestines had ever been done before, then it would be absolutely necessary to try the experiment, first of all, upon an animal. It was done, of course, first upon an animal.

10041. But supposing that by vivisection of animals it had been ascertained that a particular operation might safely be performed in a certain way upon the human frame, what would you say then?—I do not think it would be necessary then to make experiments on animals in order to acquire manual skill.

10042. That meets very much what you say?—I do not think that it would be necessary.

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10043. You are speaking about a man who is going to perform for the first time a particular operation—not necessarily limited to the very first time, but practically an entirely new operation upon mankind?—Yes, who has thought out a new method.

10044. Without having tried it upon an animal?—Yes, who has thought out a new method and wishes to apply it to mankind; but he does not know for certain what the result may be.

10045. But that confined it very much to a question of vivisection of animals simply for the purpose of acquiring knowledge?—Yes, it does amount to that.

10046. It is not operations for the purpose of acquiring manual dexterity?—The operations with that object, which I would have suggested myself as useful, would be eye operations; if eye experts think otherwise, I would not put my opinion against theirs.

10047. Then I only want myself to ask you one further question, and that is as regards anæsthetics. You have had, of course, very large experience in experiments upon animals which come under this Act, which requires complete anæsthesia?—Yes.

10048. I should like your opinion as regards both your own experiments and any experiments that you have seen performed by others, as to whether or not complete anæsthesia is always secured?—In all experiments which come under licence, complete anæsthesia is secured during the whole period of the experiment, however many hours it lasts.

10049. It has been suggested that complete anæsthesia may be obtained at first, but that animals are occasionally allowed to get out of that state of anæsthesia, and that if any more is applied it is too late, and after the animals have shown marked signs of pain by struggling, and so forth. What is your observation upon that?—That is not my experience at all. I do not mean to say that an animal will never make a movement when it is under anæsthetics; of course an animal will as a man will. But the object of the experimenter is to keep the animal under anæsthesia; his objects are likely to be entirely frustrated if the animal moves; and although the condition of anæsthesia may be more or less complete the amount of anæsthesia used in physiological laboratories is in almost every case vastly deeper than that which is used in the operating theatre. It does not matter very much to us whether the animal dies of anæsthesia or not; we lose our time only.

10049A. It has to die before it comes to?—Yes, but it does not matter to us very much, and we are much more inclined to push the anæsthetic because it is of the utmost importance that there should be no movements whatever, inasmuch as the least movement of the animal will entirely upset the delicate apparatus that we have to use. And the suggestion that we knowingly, or even unknowingly, allow the animal to come out of the anæsthesia is, on the face of it, an absurd suggestion, because it would absolutely defeat the object of our experiment.

10050. Have you observed in any operations that you have seen performed, either when you were a student or afterwards, any indifference or carelessness on the part of operators as regards anæsthetics?—I have never observed it. And I would not confine that answer to this country. I have witnessed a great many operations, in Germany and other countries, and the statements which fly about regarding the callousness of foreign physiologists to the sufferings of animals are wild, and quite unconfirmed by anything which I have seen. I myself worked for three months in Professor Ludwig's laboratory at Leipsic, where Dr. Gaskell was also working at the same time, and the system of anæsthesia employed was a very delicate one indeed, and had the result of rendering the animal absolutely quiescent during the whole of the experiment.

10051. That is in a country where it was not compulsory by statute?—That is in a country where it was not compulsory, and a country which has been specially vilified with regard to this subject. And I have seen at congresses of physiologists large numbers of operations performed, always under full anæsthesia. So that I think statements of that sort must be received very carefully indeed. There was a time no doubt, before anæsthesia was fully admitted, when surgeons in this country ridiculed the idea of

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anæsthesia and for some time refused to employ it. There may have been a reminiscence of that in early experiments, but that is long before my time. Within the period covered by this Act, I may say that I have never seen any operation performed without anæsthesia. Of course, I have seen operations in which the animals have been allowed to recover, and then the anæsthesia has passed off; but the actual operation I have never seen conducted without an anæsthetic.

10052. Are you speaking of this country?—I am speaking now of this country. I am also speaking of other countries.

10053. But assuming that in this country an anæsthetic has always been applied, the suggestion rather is, that it has not been applied fully and carefully, and so as to ensure complete insensibility to pain?—That suggestion has no grain of foundation.

10054. (*Dr. Wilson.*) May I take it generally that the same kind of experiments are resorted to to demonstrate your lectures as have been described to the Commission by Professor Starling?—I think generally, yes. I do not think I show quite as many as Professor Starling does, for the reason that I have mentioned.

10055. You use, of course, a large number of frogs for demonstrations?—Almost all our frogs are killed before we use them. I do not think that frogs are ever used which are not killed.

10056. The question has been raised, and I wish to put a few questions to you on the subject. You never, of course, submit any frog to an anæsthetic apart from the insensibility which is secured by pithing as a rule?—No, I think not. I have chloroformed them, but as a matter of fact they are killed. I do not think we ever use frogs which are not killed.

10057. Would you kindly describe the kind of pithing to which you resort?—The actual meaning of the word pith is the destruction of the medulla oblongata. I believe that to be the original meaning of the word. A pithed ox, for instance, is an animal in which a stab has been made in the medulla oblongata and the animal drops dead because its respiratory centre is destroyed; but by a pithed frog we generally mean complete destruction of the brain and spinal cord.

10058. Of both the brain and the spinal cord?—Yes. If I were to tell my assistant to pith a frog I should expect him to destroy the brain and spinal cord.

10059. Do you illustrate the experiments relating to circulation on a live frog?—Yes, on a frog with the brain destroyed.

10060. I think you gave evidence before the previous Commission?—I did.

10060A. I have been looking up your evidence, and I think you said that "in experiments upon the heart and circulation, it would not in many instances be allowable to remove the brain, because the removal would cause too great a shock to the animal"?—That is a mistake.

10061. You would like to correct that now?—If it is necessary to correct it, because it is perfectly certain that you can observe all the phenomena of the heart in circulation, even if the brain is removed. Without a qualification it was an incorrect statement.

10062. I made a note that it is in answer to Question 3865?—It depends upon what you mean by the word "brain." I was there meaning the whole of the brain, including the medulla oblongata.

10063. But not the spinal cord?—The spinal cord of course must be left, and the medulla oblongata for some experiments upon the heart and circulation must be left, and this is reckoned a part of the brain by anatomists. In that answer I have not the slightest doubt that I intended to include the medulla oblongata, but in the general popular signification of the term "brain" we mean the cerebral hemispheres. The popular signification of the brain is the cerebrum; and that, of course, can be removed without influencing the circulation, but you cannot remove the medulla oblongata without influencing the circulation. I have not the slightest doubt that in that answer I intended the brain to be understood to include everything within the skull; in the anatomical sense it is right; in the popular sense it is wrong.

10064. Then you contend that mere decerebration without destruction of the medulla oblongata renders any experiment upon a frog painless?—Absolutely.

10065. That is a point which has been raised, and it is disputed. For instance, it is admitted that a frog can jump and swim, and even, it is said, after it has returned to fairly normal conditions, can breed when the season comes round, even if deprived of its brain?—I do not see at all why it should not. That does not mean that it could have any conscious sensation. I do not know of any authority which holds that an animal deprived of the cerebral hemispheres can have any conscious sensation.

10066. It merely leaves the question open?—Yes.

10067. I do not want to press the point, but you yourself honestly believe that if a frog is only decerebrated it is deprived of all feeling?—I do.

10068. Now with regard to experiments on the larger animals. You of course have to use a good many dogs and cats?—Yes, in some experiments.

10069. Have you any doubt or difficulty at all in being able to conscientiously comply with the provisions of the Act, when you are carrying out even the most prolonged and severe operations before your students?—With anæsthetics, do you mean?

10070. Yes?—None whatever.

10071. You can always make sure?—Quite.

10071A. Would you be prepared to certify, on your soul and conscience, as they say in Scotland, that all experiments carried out on dogs, cats, rabbits, and other animals before your class are carried out to the best of your endeavour and belief, painlessly?—Yes, I would, certainly.

10072. It was stated by Professor Starling that in operating upon dogs to demonstrate a lecture, opportunity was occasionally taken to teach students methods of restoring or reviving the animals when the heart ceases to beat on respiration cases?—In consequence of anæsthesia?

10073. Yes?—In consequence of an excess of anæsthetic?

10074. Yes—that opportunity was taken to demonstrate to the students how the animal might be brought to, as it were?—Yes.

10075. In administering chloroform to patients, when an impending danger of that sort is created, is not the anæsthetic stopped at once?—I should hope so.

10076. Regardless of all risk of pain?—As long as there is insensibility there cannot be any recovery of consciousness to pain.

10077. But the anæsthetic is stopped at once?—I presume so.

10078. So that in a contingency of that kind, when you are operating upon an animal, you could not positively say—you could not be certain that sensibility may not have returned, if you are bringing the animal to?—You are assuming that the anæsthetic has been pushed to an extent in which the animal is practically dead, the heart or respiration having stopped, and that we are endeavouring to resuscitate the animal after that?

10079. Yes?—Absolutely the first thing to do is to shut off the anæsthetic. The animal having already absorbed such a large amount of anæsthetic, what you have to do is to get rid of some of it from the system. You then perform artificial respiration until the animal begins to respire of its own accord, and the moment that it does that you stop the artificial respiration, you allow it again to inhale the chloroform and air, and you go on with the operation. The animal has begun to respire, but its consciousness has not returned.

10080. How can you be certain of that?—We test consciousness in animals or in man by touching the cornea; that is one of the most sensitive parts of the body; and if we find that there is no reaction after touching the cornea, no closure of the lid, we know that there is no conscious sensation.

10081. What would be the agent that you would use—adrenalin?—No, artificial respiration.

10082. But for injecting into the veins you have mentioned adrenalin?—Yes, but I do not think we would apply that in this case. I think we would simply trust to artificial respiration.

10083. I think in connection with Dr. Oliver you were the discoverer of adrenalin?—More or less.

10084. But this adrenalin which is derived from the supra-renal capsules has failed, has it not, as a cure

for disease of these capsules—Addison's disease, for instance?—I have no knowledge. Addison's disease is a complicated disease. I do not think it is confined always to the supra-renal capsules; it is often due to phthisis, which affects the supra-renal capsules; and I do not think you would expect under those circumstances the giving of adrenalin to be of any pronounced benefit, but it is not a subject that I am competent to speak about; it is a clinical subject.

10085. Do you often use curare in this class of demonstrations?—Never. I have not used curare at all for years. I have not been engaged in any experiments which require the use of curare.

10086. It is very difficult to obtain it, I believe?—I believe it is difficult.

10086a. Would it not be very difficult to decide as to the completeness of the anæsthesia if curare were used along with chloroform?—Not if you maintained the same dose of chloroform. If you give curare you always give chloroform through an apparatus which delivers the regulation dose—a certain amount of chloroform mixed with a certain amount of air.

10087. By means of a Woulff's bottle?—There are special apparatus constructed for that purpose. I have devised one myself (which I think is better than the Woulff's bottle) which shows on an index the proportion of chloroform and air. What we do is this: we first chloroform the animal in a box or under a bell jar, when it is deeply under chloroform we put a tube into the trachea. We then connect that tube with this apparatus and pump air into the lungs—air saturated with chloroform, and air without chloroform. We observe that that mixture is sufficient to entirely anæsthetise the animal and to prevent any reflex action on touching the cornea. Then we can go on indefinitely with that mixture. I do not use curare. I have for years conducted no experiments which required the use of curare—which required complete paralysis of the voluntary muscles. We go on indefinitely without altering the apparatus. The animal will be kept alive for an hour, two hours or three hours, as the case may be, and will not move, and therefore we know that if we did give curare the animal would be equally anæsthetised.

10088. But have you not found it sometimes rather difficult to use chloroform for dogs with safety?—I made a special series of investigations on the action of chloroform and on the way in which chloroform kills. Those experiments were made with Dr. H. J. Scharlieb, and were published in the Transactions of the Royal Society of Edinburgh a few years ago. We found that we could with perfect security give chloroform in certain proportions, and that we could even increase the proportion to what would otherwise be a dangerous extent, if in the first place we added to the chloroform a small amount of alcohol, and also if prior to the administration of the chloroform we gave a small dose, a very small dose, of atropin under the skin. The way in which chloroform kills, unless it is pushed to an extremely dangerous extent, to which no administrator would ever think of pushing it, is by its action upon the nervous centre for the vagus nerve. It causes a direct inhibition through the vagus of the heart, which inhibition is so powerful that the heart stops, and it is very difficult indeed to get the heart to beat again. That effect of chloroform can be entirely abolished by the prior administration of a small dose of atropin, and we have very strongly expressed the opinion that in every case of the administration of chloroform there should be a prior administration of atropin in this manner. Whether this will become the practice of the medical profession or not I do not know, but it is my invariable practice to administer, especially to dogs, a dose of atropin, so that if they should get an excess of anæsthesia they shall not be liable to be killed by it.

10089. I have here a copy of a report of the Suspended Animation Committee in which I see an experiment described on a mongrel setter?—On what page?

10090. It is page 16: "Tube tied into trachea"?—

10091. In that case was an anæsthetic being administered at all?—"Anæsthetised with chloroform; ¼ grain morphia also given hypodermically."

10092. "Tracheal tube occluded for 4 minutes 20 seconds"?—Yes.

10093. But during that time the anæsthetic could not be being delivered by your apparatus?—A much stronger anæsthetic was being administered to the animal during that time—carbon di-oxide.

10094. You were asphyxiating it?—Obviously. It was absolutely insensible, and under any circumstances it takes several minutes for an animal to recover from chloroform.

10095. Here is another experiment on page 17: "smooth retriever," which was "anæsthetised with chloroform but died whilst being put under"?—Yes.

10096. That was one of the accidents?—Yes.

10097. You do meet with those?—Yes. No atropin was used in these experiments, it would have invalidated the object of the experiments.

10098. Then on page 19, Experiment IV., a "Dalmatian female." "This animal died whilst being put under chloroform." That is stated, is it not?—Yes.

10099. Then at the bottom of the same page—Experiment V.—a fox terrier." "The animal took the anæsthetic very badly"?—Yes.

10100. I am noting these to show that it is very difficult in some cases with dogs to safely chloroform them?—That depends. In these cases it did not matter very much whether the animal got an overdose of chloroform or not, because if it did we would take another animal. Our great object was to have the animal very thoroughly anæsthetised, consequently we took no special precautions to prevent its being killed by the anæsthetic otherwise than observing it carefully and endeavouring not to give a dose which would actually kill it. But the great object was to have it anæsthetised as deeply as possible. And I should explain that the expression "The animal took the anæsthetic very badly" means that the anæsthetic rendered the respiration rapid and shallow, so that there was a fear of the animal dying, not that it was imperfectly anæsthetised; it was rather too much anæsthetised, and went over rather too quickly.

10101. For those experiments, of course, you had a special licence?—No, those were done under the ordinary licence.

10102. Then there is Experiment XXXII., on page 48, "Moderate-sized mongrel terrier, male." "In this case also, as in the last, no anæsthetic was administered"?—Yes.

10103. That was one of your drowning experiments?—Yes.

10104. I suppose that was one of the cases in which you acted under a special licence?—Yes under a special certificate. I should like to explain that. In this series of experiments which were undertaken in order to determine exactly what happens during death by drowning, all except two (and the total number, I think, was 36) were conducted under the influence of complete anæsthesia during the whole time of the experiment; but it was of the highest importance, that one could appraise the value of these experiments, to do a certain number of control experiments in order to observe whether, so far as could be determined, the phenomena would be the same without an anæsthetic as with an anæsthetic. I therefore got permission from the Home Secretary to do ten experiments without anæsthetics, and I did two of them. The results which were obtained with those two showed so conclusively that the anæsthetic did not invalidate the object of the experiment that I left the other eight experiments, and did not perform them at all; and these are the two to which I refer. In these two experiments the animals were simply drowned by being held under water and not allowed to recover at all; and the obvious phenomena, such as the pulse and respiration, were observed and a *post-mortem* examination was made in order to see whether the *post-mortem* conditions were the same when they were drowned without any anæsthetic as with an anæsthetic.

10105. Would you briefly describe your method of restoring, or resuscitation, as it is called?—I will with pleasure. It simply consists in laying the patient upon the floor or ground, kneeling by his side, placing your hands flat on the lower part of the back of the chest, and allowing your weight to fall upon the chest, so that the weight of your body presses the air out of the lungs. Then when you raise yourself the pressure of your body—the weight of your body, is removed, the resilience of the chest wall causes the chest to expand again, and air is drawn into the lungs. By repeating these movements a sufficient number of times per minute, from 12 to 15, you can completely respire a man, so that he will not feel any necessity

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for performing respiration himself. This I have shown by an experiment which I performed upon a gentleman who was working in my laboratory upon snake poison. He was led to believe that if, in cases of snake poisoning, artificial respiration were performed for an hour or two, the poison might have opportunity of eliminating itself, and wished to know whether by this method one could maintain respiration for as long as an hour. He allowed me to perform artificial respiration upon him for a whole hour without himself at any time wishing to respire, or taking any active part whatever in respiration. This method of performing artificial respiration was entirely suggested by the results which I had obtained in resuscitating dogs which had been drowned. I found that with dogs, which also have a resilient bony cage like man, it was possible to perform artificial respiration, simply by squeezing the bony cage, and placing the animal in a prone position, so that all fluid would flow out from the mouth, which was allowed to be dependent. Simply pressing was sufficient to drive out the water and air which was in the lungs, and in the mouth, and then the resilience of the bony cage would cause the air to pass in again.

10106. But could you make fit comparisons between experiments on dogs and experiments on man with Sylvester's method, for example?—I have tried Sylvester's method, of course.

10107. With dogs and man?—Yes; and I found it was impossible by Sylvester's method, either with dogs or man, to maintain respiration. It is quite impossible for you to allow another person to apply Sylvester's method for any considerable time; you must breathe; you do not get enough air into the chest by this method. You cannot do all the complicated motions involved in Sylvester's method in the space of time which is required to effect the requisite exchange of air per minute. You cannot do it efficiently. I have repeatedly tried it upon individuals, and found that it was quite impossible by it to get them in such a condition that they had not a *besoin de respirer*—a desire to breathe—a necessity for breathing. That is not the case with this pressure method, applied in this way. There is, as you doubtless know, another pressure method—Dr. Howard's method. Dr. Howard was an American who applied pressure when the body is on the back; his is also an efficient method; but has certain dangers. One is that in applying the pressure to the ribs you are somewhat liable in old subjects to break the ribs; and, as a matter of fact, I have been told by a gentleman who was present at a demonstration by Dr. Howard upon an elderly man in Dublin, that he did actually break some ribs. Another danger in cases of drowning, which came out very strongly in these experiments on drowning dogs, is that in drowned persons the liver is enormously congested, it is much too full of blood, and extends far below the ribs, and, if you press upon this part of the abdomen and chest, you are liable to rupture the liver; this would be a serious danger in applying Howard's method to cases of drowning. The method which I have explained to you, in which the patient is placed upon his face and pressure is applied to the back, has been applied in three of four cases of drowning with success by my students; to whom we teach it now always as a regular part of the curriculum. Two of these occurred at the seaside; these were cases of apparent drowning during bathing. One case in particular, of which a student told me, was at Shanklin. One of his fellow bathers got into difficulties and sank, and was not pulled out for nearly ten minutes. This student immediately applied the method, and in a quarter of an hour the man was up and walking away. An unfortunate case occurred to my knowledge, in which the method was being applied by one of my students. There were slight signs of recovery—I do not say that the patient would have recovered, because there are sometimes slight signs of recovery which pass off. But a doctor who knew nothing about the improved method came up—he only knew Sylvester's method—he ordered the patient to be turned over, and then set to work with Sylvester's method, when the symptoms of recovery immediately disappeared.

10108. But have you tested Sylvester's method and your own method spiro metrically on man?—Yes.

10109. And your own method shows that there is a greater respiration?—Yes, it shows that there is a much greater respiration; neither Sylvester's method nor the Marshall Hall method gave as much air as we normally take in with ordinary respiration; whereas both the Howard method and my own method gave a sufficient amount. It was tested very carefully spiro metrically.

10110. And do you say that you could not have evolved your own method without experiments on dogs?—As a matter of fact, it was suggested in the course of these experiments. I would not say that I could not have evolved it without those experiments.

10111. Now, I want to ask you a few questions about those brain experiments to which you have referred. You have performed a great many experiments on monkeys, have you not?—Yes.

10112. And Sir Victor Horsley, of course, and Dr. Ferrier, in this country, besides a number of physiologists on the Continent, have performed experiments on brain localisation?—Yes, and there are a good many more in this country than you have mentioned. There are Professor Sherrington's experiments, for example.

10113. And Grünbaum's?—Yes, with Dr. Grünbaum they have been very suggestive; and Dr. Beevor's with Sir Victor Horsley.

10114. Have not many of the results been rather contradictory, and practically of very little value in brain surgery?—I should think not; but you must ask a brain surgeon about that. Sir Victor Horsley will give you the best evidence on that point. They are not contradictory, so far as animals are concerned; I will answer for that.

10115. Experimentation on animals has done nothing for insanity, for example. That is on the increase?—I am afraid it has not.

10116. Or epilepsy?—Probably not. You must apply to a clinicist; I do not know.

10117. Perhaps it is not within your knowledge, but I have a note here of a statement made by Dr. Ferrier, who said at the British Medical Association meeting in Edinburgh as recently as 1898—that was long after his own experiments were carried out?—Yes.

10118. "The treatment of intra-cranial tumour forms rather a melancholy chapter in therapeutics"?—You must ask somebody else about that.

10119. You do not know about it?—I know nothing about it.

10120. I have several other instances of the same kind here of which you personally have no knowledge?—Absolutely none.

10121. But as regards these investigations of yours, is it not of interest to you to see how they are applied?—It is of immense interest.

10122. But you have not really time to read?—Yes, I read; but I am no authority on the subject. I could not say anything either for or against a statement of that kind.

10123. Will you take it from me that at the same meeting Dr. Ferrier further said that only 7 per cent. of cranial tumours are capable of being surgically dealt with?—I will take anything you like from you; but it is no good asking me that sort of question; you must ask a surgeon, or a neurologist.

10124. You see all these experiments are constantly paraded as being of such immense value to humanity and to the profession in medical and surgical practice?—I cannot gauge their value, or their want of value, I am afraid.

10125. Well, with regard to the operations themselves on animals, is not even your own mapping out of brain functions disputed by other physiologists?—No, I think not—only on unimportant details.

10126. I will ask you this question. Is it not logical to infer that as there is such a wide difference in the size, conformation, and arrangement of convolutions and fissures between the brain even of a monkey and that of a man, that useful deductions as to localisation of the functions of the human brain, based on experiments on a monkey, must be liable to a considerable amount of error?—No, I think not; I think the analogy is fairly complete, as has been shown by cases in which the brain has been exposed in man. In such cases it has been repeatedly found that excitation of the same or the corresponding part of the brain in man to that which yields a certain result in a monkey will give the same result. That has been repeatedly shown in man.

10127. In all these experiments on monkeys the operations themselves are no doubt carried out painlessly, but, at the same time, a great deal of pain must be suffered afterwards when the animal is under observation?—There is no sign of it, absolutely none.

10128. But you cannot say that there is none?—I can only say that there is no sign of pain. The animal is in a perfectly normal condition, eats its food, and runs about the cage, except for such paralysis as we may have produced as the result of the operation. If there was any sign of pain we should immediately kill the animal; but we have never had occasion to do this since the introduction of antiseptic methods.

10129. Have not experiments on the brains of animals been carried out ever since the time of John Hunter, during the whole of last century?—And long before that. Experiments performed before the introduction of antiseptics may well have been painful.

10130. (*Sir Mackenzie Chalmers.*) By reason of the after results?—Yes, because inflammation was then possible. But there is no such thing now. There is no sign of inflammation when you experiment on the brain. The conditions are perfectly normal.

10131. (*Dr. Wilson.*) But you cannot say that there is no pain?—You can only say that there is no sign of pain, no sign of *malaise*, no sign of illness whatever. You would be surprised to see how well these animals are, and look.

10132. Do you still believe it is necessary to continue these experiments on animals?—When we come to that question you must understand that there is no finality in knowledge. Our knowledge is an ever-widening circle, and as it widens there are more points to investigate.

10133. But I am referring to useful knowledge applicable to medicine and surgery?—All knowledge is useful; but what shall be applicable to medicine or surgery we cannot at the moment say. Who would have thought that Pasteur's experiments upon fermentation would be applicable to medicine and surgery? Who would have thought that there was useful knowledge in them? And yet that has led to the whole of Listerism.

10134. No?—Yes, directly.

10135. No?—Yes. Pasteur's experiments on fermentation led directly to Listerism.

10136. But by Listerism I understand the use of the spray?—Nothing of the kind. That is a mere detail.

10137. Really. I have seen Lister operate, but he afterwards gave up the spray. That was entirely the method?—That is not Listerism.

10138. We will have that out with some other witness. But was not all aseptic surgery a revolt against Listerism? There was a revolt against Listerism you know?—But aseptic surgery is the most complete carrying out of Listerism.

10139. But Listerism is antiseptic surgery?—No, certainly not. The principle of Listerism is the elimination of micro-organisms; you can eliminate those by antisepsis or by asepsis.

10140. Well, I did go into that discussion at the time, and, as I understand Listerism, it was what was called antiseptic surgery?—You must ask a modern surgeon.

10141. But, you see, you make a strong statement?—Yes, and I am quite prepared to stand by everything that I say. My experience is derived from animals alone, but that experience is very extensive, and I have never seen any sign of pain in an animal which had been anæthetised during the operation, and treated either antiseptically or aseptically afterwards. There are certain parts of an animal that we would not treat antiseptically, because the antiseptic itself might be a source of irritation; but, as a general rule, it is safe to apply a certain amount of antiseptic to the outside of a wound, because it ensures, especially in animals in which one cannot cleanse the skin so thoroughly as we can in man, that there shall be no invasion of germs from the outside. But the principle is the same. The principle is the exclusion of germs. In one case you kill them, and in the other case you prevent their entrance, but the essential principle of Listerism is the exclusion of germs. There is no question about it, I should think.

10142. But did not Lord Lister use the spray to kill the germs?—Yes, because he thought that the germs were more universally present than they are. He thought that they were universally present in the air, and that he must exclude air which had not been treated with antiseptics. But he has himself long since recognised that that is not necessary.

10143. I will not press that question, then, further, as you do not pretend to be a surgeon?—But I do pretend to understand something about that subject, because I deal with it every day.

10144. But you do not seem to have read up the subject very closely. Have you even seen Lord Lister operating under the spray?—He does not operate at all now.

10145. Have you ever seen him operating under the spray?—No, I have never seen him operate at all. I have operated myself under the spray, but I do not operate under the spray now.

10146. Was there not a revolt against Listerism, as it was called?—It was falsely called Listerism, if it was so called.

10147. No, it was called Listerism—the use of the spray, the use of antiseptic dressings and antiseptic ligatures. All that was included under the term Listerism?—Ligatures were not necessarily antiseptic. They were treated by antiseptics, but they were aseptic ligatures. But Listerism is the exclusion of germs from wounds. That is how I should define Listerism. How do you define Listerism? I am not examining you, I admit; but still, as part of the discussion you ought to tell me how you define it.

10148. I define it as I was taught. Killing germs in the air is Listerism.

10149. But was there, then, no revolt against Listerism?—No.

10150. But the revolt against Listerism was carried on by surgeons, who operated on the peritoneum?—I would not call it a revolt against Listerism; I would call it a modification of the method.

10151. I will not press this question further; I will get it out of someone else?—I would certainly rather that you asked a surgeon.

10152. (*Dr. Gaskell.*) Have you found that by giving atropin with chloroform you have always been successful?—Do you mean to say that it is impossible to kill the animal?

10153. I mean have you ever had any deaths?—Not with careful administration. Without atropin, with the most careful attention you may have deaths.

10154. Is that method of giving atropin more or less confined to your laboratories, or is it used elsewhere?—I think it is chiefly confined to our laboratories.

10155. You gave us a number of experiments that you advocated for students. Do I correctly understand that that included all that were advisable, or only samples?—Those are samples. I think other experiments might be shown, those are some fundamental experiments which I jotted down just before I came here.

10156. I noticed that you made no mention of respiration experiments?—One naturally shows respiration experiments along with the others. When you investigate the action of the vagus upon the heart you record its action upon respiration at the same time. I did not mention respiration experiments, I admit.

10157. I meant rather Hering and Breuer's experiment?—I am not in the habit of showing that.

10158. You do not consider it important?—I consider it important, certainly, but as a matter of fact I am not in the habit of showing it.

10159. Do you consider that the demonstrations that you do at Edinburgh are more or are less than what are done in other places?—I think they are rather less, because we have so large a number of students to deal with that as it is we are compelled to divide our classes for demonstrations into four batches, and we are compelled to repeat the same experiment four times.

10160. Have you read "The Shambles of Science"?—Yes.

10161. Have you noticed the nature of the demonstrations given there?—Yes.

10162. In your opinion were those justifiable?—Which are you referring to?

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10163. All those quoted in "The Shambles of Science." Perhaps you do not remember them?—I am afraid not. Do you mean to say exactly as they are described?

10164. No; I mean the kind of experiment, the object of the experiment. There were experiments, of course, for advanced students?—Yes, they were experiments for advanced students. I may say that my experiments are none of them for advanced students. My experiments are experiments to the whole class. All students come to them.

10165. (*Sir Mackenzie Chalmers.*) Of what year?—They come to them in their second year.

10166. (*Dr. Gaskell.*) Do you give another course for advanced students?—We give a course to advanced students, but they do not have demonstrations in that course.

10167. Not special demonstrations?—Not special demonstrations; they come to the others.*

10168. In your time at University College was such a course for advanced students ever given as is described in "The Shambles of Science," or is that an innovation?—I think it is an innovation. There was not such a complete course given for advanced students in my time.

10169. Do you think it is a good innovation?—A very good innovation.

10170. Do you think that more care ought to be taken as to finding out the status of students before they go to those demonstrations?—I do not quite understand what care is taken there. It seems to me, from what I have read and what came out in the case of Bayliss *v.* Coleridge, that reasonable care was taken that the students should be *bonâ-fide* students and should be advanced students. They were recommended by the teachers in their respective schools. The University of London is quite different now from what it was when I was there. They now admit students from all schools to certain demonstration lectures. In my time, only my own students, every one of whom I knew, came to the demonstrations.

10171. I should rather like to know from you whether you would advocate any change in the law as it is at present administered or stated in the Act?—Perhaps I may read a statement with regard to that.

10172. I should like you very much to make a statement, because I hoped that I should have heard one already?—The statement is this. Although the Act has not been without benefit, because it has given to all intelligent laymen a definite assurance that no wanton cruelties (such as were basely insinuated before the system of licensing and inspection was instituted) are possible, and the reports have shown that all experiments are carried out with the least possible amount of pain, and in 99 out of 100 cases are absolutely painless from beginning to end—it must be admitted that it somewhat tends to hamper research. There is often too great a delay in the allowing of certificates, and by the time the inhibition to use them is removed (the normal period of inhibition being one week according to the Act, and the actual period of detention being often very much longer, at the discretion of the Secretary of State), the best opportunity for conducting the experiments may have lapsed. It was quite clearly the intention of the Act that all certificates should be granted by two eminent medical or scientific authorities, and although power was taken to disallow or suspend a certificate which had been granted, nevertheless this was obviously intended only to be used in case of any abuse or infringement of the Act. The Act nowhere gives power to the Secretary of State to impose any conditions on the certificates, but this power has been illegally assumed by successive Secretaries, and certificates are thereby materially modified and their usefulness seriously impaired. I would like to give an instance of that which occurred to myself. I mentioned two experiments which I performed on animals in connection with the subject of drowning. I asked for permission to perform certain experiments without general anæsthesia, because I was anxious to determine whether every condition was exactly the same without anæsthesia as with anæsthesia; accordingly I asked for permission to put a cannula into the artery of a dog's leg with complete local anæsthesia—cocaine, and during the drowning to make an experiment upon the blood pressure in that dog. I obtained a certificate for that purpose, but there was written over that certificate, "The preliminary operation which is suggested under local anæsthesia is hereby disallowed." (or words to that effect.) I consider that that was an illegal action on the part of the Secretary of State, but I had no remedy because the Secretary of State would simply have disallowed the certificate altogether, and I was compelled to do the best I could under the circumstances. This I put forward as an instance of a research, and an important research, being hampered by the authority of the Secretary of State acting, so far as I am able to ascertain, without expert advice, and, indeed, contrary to such advice. To enforce this illegal position the threat of suspension of the certificate is held over the head of the applicant. Further, all applications are referred, first, to the Association for the Advancement of Medicine by Research; and, secondly, to the Inspector under the Act. I do not object to a confidential reference to the Association for the Advancement of Medicine by Research, because that Association is composed of experts on the subject who are very capable of forming a judgment as to the expediency of proposed experiments, and their judgment can only have the effect of strengthening the position of the Secretary of State when he is called upon to decide on any doubtful question; nevertheless, it must be conceded that even this is clearly outside the four corners of the Act. On the other hand, the Inspector has to do with the administration of the Act, and is not necessarily an expert upon the subject of physiology or pathology or pharmacology; he has no special knowledge of what is or is not necessary for the advancement or knowledge in the special branches of these subjects; his duties are defined by the Act, and consist in visiting registered places for securing a compliance with the provisions of the Act. Personally, I have not a word to say against any advice which may have been given by the present or past inspectors in Great Britain; but the abuse of the position of the inspector has come out very glaringly in the evidence of the Inspector for Ireland, whose advice seems to have been acted upon for a time in opposition to that of the granters of certificates. I have studied the Act carefully, and I can find nothing in its provisions which warrants an appeal being made from the experts who are authorised to grant certificates to officials who are not experts in the science and are not conversant with the necessities of the investigation proposed to be employed. That is what I have to say about the present restrictions. I would strongly deprecate, on behalf both of the Royal Society of Edinburgh and of the University of Edinburgh, the imposition of any further restrictions whatever. Any restrictions on scientific work are detrimental to its progress, and further restrictions of experiments on animals might prove disastrous to the progress of physiology and medical science in this country. Since the revival of scientific medicine in the last two centuries we have taken a leading part in contributing to its progress. In no other country in the world is more regard paid to the ethical aspect of the treatment of animals, and the care which has always been exercised by physiologists in this country to prevent pain in the performance of experiments, is a reflection of a general sentiment which animates all class of society. (The callousness of the laity generally with regard to the sufferings which are inflicted upon animals intended for the table, or which are involved in pursuit of pleasure, is due, I believe, in the majority of cases, to sheer ignorance.) Nevertheless, I have not found any disregard of animal suffering to prevail amongst physiologists and medical men of other countries, in spite of the fact that in some countries the people generally are far more callous regarding animals than are we ourselves. It is strange that this should be the only country in the world in which restrictions are placed on the use of animals for the purpose of advancing medical and scientific knowledge, in spite of the fact that there is overwhelming evidence that the actual amount of pain inflicted is infinitesimal compared with the results achieved and compared with that which is daily and hourly inflicted in the pursuit of pleasure or of commerce. Even quite recently a clause was inserted in an Act of Parliament prohibiting stray and unclaimed dogs seized by the police being employed for the advancement of medical knowledge. The silliness of such a regulation is self apparent, for these animals are condemned to die, and are in many cases killed by a process of suffoca-

* *Mr. Schäfer subsequently wrote to say that he is informed by Dr. Sutherland Simpson, the University Lecturer on Experimental Physiology, that this year for the first time he has shown a single experiment on a rabbit, under Certificate C, to the advanced class of Experimental Physiology.*

tion; at the best they are killed by the same anæsthetic as would be employed in the laboratory; and the result is that other dogs have to be killed in the laboratory and the number of animals sacrificed is increased by so many. Who benefits by this? Certainly not the canine race.

The necessity of employing dogs has been emphasised by others who have given evidence, but it cannot be too strongly insisted upon. No other animals are available for experiments in important fields of physiology. Among these I would instance experiments upon the heart and circulation, upon the secreting glands and the digestive system, upon the kidneys and the formation of the constituents of urine, upon lymph formation, upon metabolism, and lastly experiments on the nervous system, in certain of which animals which can be trained must be employed. Dogs are the only animals which are available for many such experiments.

10173. Do not you think it would be a good plan if animals which are kept alive under Certificate B were not necessarily obliged to be kept in the laboratory in which they are operated upon; that they might go to a farm, for instance?—I was not sure that they were necessarily kept in the laboratory.

10174. We have understood so here; that they are obliged to be kept in a licensed place?—Certainly, if it is the case it is unnecessary and might be detrimental. As a matter of fact, I do not think I have understood that. I do not know whether I have infringed the Act myself in that respect.

10175. They have to be kept in a licensed place, I understand?—Supposing that a horse is being used for the preparation of serum?

10176. That does not come under it?—It might be done experimentally. Are you not to put it out to grass? It is nonsense.

10177. (*Sir Mackenzie Chalmers.*) There is a special form of certificate for that; the Board of Agriculture get special certificates.

10178. (*Dr. Gaskell.*) But the ordinary case is that the animals have to be kept in the laboratory, in a town very often, and it has been strongly advised by others that Certificate B should be altered in that respect. I wanted to know your opinion on that point?—There is no reason whatever for it. The new place would be registered.

10179. You naturally agree with that?—Yes.

10180. Would you also agree with this, which has been suggested to us: that in case of inoculations those inoculations should not necessarily be done by the licensee, but that authorised laboratory assistants should be allowed to do them?—I do not know. I think I should like to reserve my opinion on that. I have not thought about it. I would not allow my own laboratory assistants to do any experiment for me or to make any observations.

10181. I am only speaking of mere inoculations?—I would reserve my opinion.

10182. Many laboratory assistants are experienced in that respect?—Yes, of course. What you really mean is that laboratory assistants should be licensed?

10183. No, that was not the suggestion made to us. The suggestion made to us was that the licence to the professor should carry with it the power of the laboratory assistant to do a simple thing like an inoculation.

10184. (*Sir William Church.*) There are pathological researches at the present time going on in which perhaps 100 inoculations take place as quickly as possible?—Yes.

10185. At the present time it is a great infliction that the head, who is perhaps the only man who holds a licence in connection with that research, should have to spend all his time in making these 100 or 200 inoculations—pricks?—Yes, if that is what you mean I should certainly agree that that should be done, because a man who does that is acting simply as a machine.

10186. It has to be done by a man who holds the licence?—It has to be done under his sanction. I did not understand the question.

10187. (*Dr. Gaskell.*) I am sorry. That is all I meant. Would you be inclined to confine the Act to warm-blooded animals—a modification in that direction?—Yes, I should. I think it is absurd to suppose that cold-blooded animals feel in the same way that warm-blooded animals do. I do not see why it should not be so confined.

10188. I believe the Act originally was framed with those words?—Yes, it was so framed. Nobody who has observed fishes, for example, can possibly imagine for one moment that they feel pain in the same sense that warm-blooded animals do.

10189. Then there is one other question that I want just to have your opinion upon. Do you use morphia often in your laboratory?—No, I do not use morphia. I used to use it more often, but I use it less often now, because I use atropin, and they are somewhat antagonistic.

10190. Would you say that morphia only dulls pain and does not remove it?—Certainly not. A sufficient dose of morphia absolutely removes all signs of pain.

10191. (*Sir Mackenzie Chalmers.*) You yourself are a medical man, are you not?—I have a medical qualification. I am on the Medical Register.

10192. What I want to ask you is this: Do you think that licences and certificates under the Act ought to be granted to physiologists who have not any medical qualification?—Certainly. If you are not to grant them to physiologists who have not a medical qualification you would eliminate some of our most eminent physiologists.

10193. The difficulty is if a man has not a medical qualification what guarantee have you of his qualifications?—You have the guarantee of those who sign the licence and certificates, and of the director of the laboratory where he is to work. There is no instance of private work in private laboratories. Some of the very men I have had to do with have had no medical qualification. One of the most eminent workers in my own laboratory at the present moment is not a medical man, he has gone through no medical mill, and has no medical qualification—Dr. F. A. H. Marshall; he is a doctor of science, but he is one of the best experimenters I know. There is further the very eminent instance of the Professor of Physiology of Cambridge, and Dr. W. B. Harvy, also of Cambridge.

10194. None of whom have medical qualifications?—None of whom have any medical qualification at all. Physiology is only accidentally and incidentally a medical science; it is the science of life, and as medicine depends upon life, physiology has come to be included with medicine; but it must be treated as an independent science, and to exclude anyone on the ground that he has no medical qualification is to my mind ridiculous, for if a man has special knowledge of a subject and a special qualification for working at it, what more is required? And when one can adduce instances of men who have advanced the science so materially as those whom I have instanced, the objection is a *reductio ad absurdum*.

10194A. The next point I want to put to you is this. You mentioned certain experiments of your own on dogs, but I did not quite catch the number of dogs that you experimented upon. You performed two experiments without anæsthetics. How many did you perform with anæsthetics?—36 altogether—two without anæsthetics and 34 with anæsthetics. A certain number of those were not actually experimented on because they died whilst being put under the anæsthetic.

10195. And as the results of the experiments on those animals, in your opinion and that of other competent people, you have discovered a more fruitful method of resuscitating the drowned?—Yes.

10196. Has that method been in any way communicated to the Royal Humane Society?—It was only communicated by my having sent a copy of the paper to the secretary—in no other way. It is adopted by individual instructors in various parts of the country. I think it is unfortunate that the Royal Humane Society, the Life-saving Society, and the National Life Boat Institution have none of them taken it up, although it is well known to physiologists, and is becoming widely known to medical men. But societies like those are apt to fossilise; it is very difficult to get anything new instituted in connection with them. They have all their instructors trained in the old methods, they have all their leaflets printed, they do not know that one method is any better than another, and they have no means of judging.

10197. As a matter of fact, your method is easier to teach?—It is very much easier to teach.

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10197A. And much easier to practise?—And much easier to practise. It can be done by one man or one woman, or even a boy or a girl; it requires no physical strength. I believe many lives would be saved if it were generally known, because it can be applied so promptly and easily.

10198. (*Chairman.*) By anybody?—Yes; too much time is lost by the present methods, with all their preliminary proceedings—laying the patient down, turning him over, pulling out the tongue, undoing garments, and so on, when every second is of importance.

10199. (*Sir Mackenzie Chalmers.*) I may take it generally that by the sacrifice of 36 dogs we have a much more prompt means of restoring the drowned, which may in time save thousands of lives?—I believe that is so.

10200. As regards dogs themselves, I believe in Edinburgh superfluous dogs are destroyed by drowning?—They used to be destroyed by drowning; they are now destroyed by suffocation, I believe. I think they are put into a chamber with coal gas.

10201. So that at any rate the two dogs which were not under anæsthesia simply suffered the common death of superfluous dogs?—Yes.

10202. I noticed that in your statement you said that as regards new drugs, drugs must be first tested on animals?—Yes.

10203. We have had evidence to the contrary, and I want you to develop that a little one way or the other. We have been told that you can draw no inference whatever from the operation of a drug on an animal as to its operation on man. What is your observation with regard to that?—If you take a drug that you know nothing about, and administer it to a man, first, there is a certain element of danger, and, secondly, you can only judge of its action upon man by external signs, whereas upon animals you can judge very accurately its action on different organs of the body. You cannot do that on man. Take a drug which is supposed to act on the liver; we know that many drugs are supposed to act on the liver, and are called cholagogues; when tested upon animals, that many are not cholagogues, that they have no action on the liver; and yet for scores of years these drugs may have been administered in diseases of the liver with the idea of stimulating the liver to action, which idea, when subjected to the test of experiment, immediately vanishes.

10204. In the case of an experiment upon an animal, you verify the experiment by a post-mortem examination?—Whenever it is necessary.

10205. When applied to man, you would hardly like to test the experiment in that way?—No.

10206. We were told by a gentleman who represented homeopathy the other day that among homeopathists all experiments were made upon man, and not upon living animals. Do you think there is any danger in that?—I do not think there is any danger in the doses that homeopathists give.

10207. But in gradually increasing doses, I am afraid you would not like to do it on human life?—I think it might be all right, but one can never know with any new drug what dose might be poisonous.

10208. (*Chairman.*) The witness said that in testing a drug they did not use homeopathic doses; they used the drug in its ordinary form, and tried it as high as they could?—I am afraid I am no authority on the subject of homeopathy, but I would put an extreme case: Suppose a new drug having such a powerful action as cobra venom, and suppose you tested it in ever so small a dose upon a man, it would probably kill him.

10209. (*Sir Mackenzie Chalmers.*) We have heard a good deal for and against experiments made before students. In your opinion it is essential, to complete the education of a man who is going to practise as a doctor, that he should see certain experiments on animals?—That is my very strong opinion.

10210. I wanted to ask you this, further. It has been alleged that witnessing experiments on animals has a demoralising influence on students, and tends to make them inhuman. From your 37 years' experience as a teacher, what do you say to that?—I say that it is absolutely without foundation. The men I know best are the men who have been my assistants, who have been engaged as students and as teachers in operating upon animals. I should say that amongst all my friends there are none more humane than these who are constantly engaged in experimenting upon animals. So far as the effect upon students is concerned, it is obvious that what you have mentioned cannot be the case. The great mass of medical men who are turned out nowadays have seen experiments on animals—they certainly have at Edinburgh, for my predecessor showed many more experiments than I do, and hundreds and thousands of students passed through his hands, and I have not heard that the Edinburgh medical graduate is more callous than any other medical man who has not seen experiments on animals.

10211. To Dublin medical men experiments were denied for some years?—Yes, they were not shown in Dublin for many years; I do not say that those gentlemen are not more tender-hearted than others, but I have no evidence of it.

10212. It has been suggested that by seeing these experiments certain individuals, at any rate, get a desire to perform cruel experiments. Have you ever known an instance of that?—Never.

10213. It has further been suggested to us that in these demonstration experiments, as well as others, very light anæsthesia is practised. Is that according to your experience?—That is certainly contrary to the fact. I have very considerable experience of demonstration experiments, and I know as a matter of fact the great danger, in our experience, is the loss of the animal from over-anæsthesia. We are very careful that there shall not be any sign of movements, because that might be misinterpreted by the students. Although we ourselves know that movements do occur in cases of anæsthesia—every surgeon knows that—it is possible that a young student might not understand it. He knows it perfectly well after he has been in the operating theatre.

10214. I do not know if you have read Miss Lind-af-Hageby's evidence?—I read the second part of it while I was waiting upstairs.

10215. She apparently witnessed a good many demonstration experiments, and she told us that continually there was movement in the animals?—I do not know. I think that one experiment that she witnessed was on a choreic dog. There was, of course, movement in that.

10216. Would an anæsthetic stop the chorea?—I do not know; I should think not that form of chorea. Of course, movement invalidates experiment. You cannot work properly with movements; you are liable to have your apparatus injured.

10217. At any rate, in your laboratory during your 37 years' teaching, did the animals make purposive movements and struggle?—Certainly not; never.

10218. You may, I suppose, get movements when you are killing an animal?—If you kill the animal by pithing, you would have movement of muscles, which would not be voluntary movements, but simply due to excitation of the motor nerves that you were cutting through. But the usual plan is to kill the animals with chloroform; in fact, that is the invariable plan with us.

10219. You push it rather further?—Yes, we either do that or inject chloroform into the blood vessels, which kills them immediately.

10220. Miss Hageby spoke to hearing cries from some of the animals seen under demonstrations. Is that possible?—It has never happened when I have been present. I should think it is quite impossible.

10221. In your 37 years it has not happened?—No, never.

10222. Do you agree with the evidence that we have had, that when an anæsthetic is administered sensation disappears before power of movement disappears?—That is quite well known to be the case.

10223. Do you further agree that power of movement returns before sensation returns?—That is also well known.

10224. Is that sensation generally, or is it sensation to certain stimuli?—I should say sensation generally. There are higher and lower forms of sensation. The higher forms of sensation are, of course, the last to return.

10225. (*Dr. Gaskell.*) When you say that you have never heard any cries in your experience, have you

never heard a sound coincident with respiration of a sort of whining character; I mean to say, through the respiration tube, principally through a little blocking in the tube?—Yes, that may occasionally occur if there is mucus in the tube.

10226. (*Sir Mackenzie Chalmers.*) A sort of râle?—Yes. I suppose by this time one hardly considers any member of this Commission as belonging to the laity—they must know by this time much about the subject; but I may be permitted to explain that it is very well known that patients when they are taking chloroform, although they are perfectly without sensation, make all sorts of noises, sing, scream, and make ridiculous utterances without being the least conscious of it, and without having any memory of having done so when they wake up. And in the same way one of the symptoms which is observed when a dog is put into a box or given chloroform is, just before he has become profoundly anæsthetised, a succession of loud noises, not exactly a bark, not exactly a whine, and not exactly a howl, but peculiar noises which might be taken by anybody who was not an expert on the subject to indicate that the animal is under the influence of pain. It is nothing of the sort at all. It belongs to the same class of noise that patients make who sing and cry out when they are going under chloroform, just before they are completely under its influence.

10227. (*Chairman.*) Does that differ with different dogs as it does with different human beings?—Yes, very much—some are perfectly quiet, and others make a great deal of noise. I do not like to hear it myself, and I am afraid that the students who are not always in the room at the time might sometimes think that we are ill-treating the dog, whereas, as a matter of fact, the animal is simply in a box with vapour of chloroform being pumped into it.

10228. (*Sir Mackenzie Chalmers.*) Do you use pure chloroform or A.C.E.?—I do not use A.C.E. I use chloroform with a small proportion of alcohol, one-tenth of alcohol; I find this quite as efficient as A.C.E., and simpler. With A.C.E. you introduce three drugs into the animal. When you use an anæsthetic you must introduce a drug, and it is better to reduce the number.

10229. Have you ever experienced any difficulty in giving an anæsthetic to dogs and keeping the dogs under anæsthesia?—In my experience dogs are more easy to anæsthetise than any other animal. One is much more liable to lose a rabbit than a dog with chloroform, especially if you give the dog a prior dose of atropin. It is very rarely indeed that I have lost a dog after introducing atropin. That would be due to sheer carelessness in the anæsthetic being pushed to an extraordinary extent. We cannot in a laboratory always have the services of a skilled anæsthetist, and this makes a great deal of difference; we are sometimes obliged to ask a person who is not a skilled anæsthetist to give the chloroform. And, as everybody knows, anyone who is not a skilled anæsthetist is much more likely to kill a patient than a skilled anæsthetist, because he will push the anæsthetic too far if he sees the least movement, and thinks the patient is coming out.

10230. In the case of an animal if you push the anæsthetic the worst that can happen is that the animal dies prematurely?—Yes.

10231. So that you can push the anæsthetic better with an animal than with a human being where you wish the man to recover?—That is so. There is the element of anxiety in giving an anæsthetic to a patient. That is not the case with an animal, because it does not matter seriously if it dies; consequently you push it more than you do with a patient.

10232. Then there is one other small point. We had some evidence last time, I think, as to various ways of pithing frogs, and it was suggested to us that the spinal cord might be severed from the brain and purposive movements would still go on; and, although the spinal cord is severed from the brain, the frog would feel pain. What is your opinion as to that?—I think all the evidence that we have is contrary to that. I think the evidence from man, if a man is to be compared with a frog, is conclusive.

10233. When once you have severed the spinal cord from the brain of man?—There is no sensation below the point of severance. The fact that movements are produced is well understood. You might just as well say that the tail of a newt, which continues to writhe and perform spontaneous movements, has all consciousness and volition left in it. No one for a moment would hold such a thing.

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10234. (*Dr. Gaskell.*) That is just what they do say?—I mean no expert authority.

10235. (*Sir Mackenzie Chalmers.*) Or a person who has studied the subject and is capable of forming an intelligent opinion?—No.

10236. As regards the use of atropin, has that been applied as yet to human beings?—It has.

10237. In conjunction with an anæsthetic?—Yes; Dr. Scharlieb, who did the experiment with me, and is an expert anæsthetist in London, tells me that he would use it always, but he cannot always get surgeons to agree that it shall be used.

10238. Do you think, as the result of your experiments on animals, that a possible improvement has been made in administering anæsthetics to human beings which may have the result of saving life?—Yes; but I would not take the credit of this to myself. It was first shown by the experiments of Dr. Embley and Dr. C. J. Martin, of Melbourne, who is now at the Lister Institute, that the fatal effect of chloroform is usually produced on the vagus, and that cutting the vagus (and giving atropin has exactly the same effect as cutting the vagus, because it cuts off the influence of the vagus from the heart) entirely abolishes this inhibitory effect of chloroform. So that although I have carried the giving of atropin into practice and I have recommended that it should be given in all cases of anæsthesia, I do not wish to take credit for the discovery of the fact.

10239. Have you discovered any dangerous results from giving a dose of atropin when given with an anæsthetic?—No, none whatever; the dose required is very small. In a large dog, for example, 1-50th of a grain of atropin is quite enough to keep the animal in a safe condition for three hours. In the case of man, I do not think a larger dose is required, because man is more susceptible to atropin than the dog.

10240. Of course the atropin itself has no effect on the sensation of pain or on the anæsthesia which is produced?—No.

10241. Then there is one other question I want to ask you as regards the Home Office Administration. You agree that under the Act the Home Secretary can suspend or disallow a certificate?—Yes.

10242. Does it not follow from that that he may impose conditions?—I do not know that it does follow from that, but he can impose conditions on the licence.

10243. I am speaking now of a certificate?—There is nothing in the Act to say that he shall alter the certificate. The certificate is granted by two authorities, and there is nothing in the Act to say that he shall alter the certificate.

10244. But an alteration, I suppose, is alternative, and possibly preferable to disallowing it?—When he puts himself in the position of the grantors, I do not think that he is right. He makes himself an expert authority on the subject.

10245. At any rate, he is the person responsible to the public for the operations that go on?—I think under the Act it was intended that the grantors should be responsible to the public. Otherwise I do not see the use of them.

10246. The Secretary of State has to answer in Parliament, which the grantors have not to do?—Yes, of course.

10247. Then as regards the place of the experiment, I did not quite understand your suggestion. You thought it was unnecessary to keep the animals in the place?—I do not think it is of any importance that they should be kept in the same laboratory.

10248. Is it not simply for the purpose of inspection; that if the animals could be moved about the inspector would not be able to inspect them?—I think the inspector would be able to inspect them if they were removed to another registered place.

10249. You would confine your amendment of the law to allowing them to be moved from one registered place to another registered place?—As for example, from a laboratory in town to a farm in the country, which might also be registered for the keeping of animals.

Mr. E. A. Schäfer, LL.D., SC.D., F.R.S.

12 June 1907.

10250. (*Chairman.*) Is there anything else you wish to bring before us?—Something has been said about the importance of clinical observation, and as to the possibility of clinical observation superseding experiments altogether. I should like to illustrate the defects of mere clinical observation unassisted by experimentation on animals by a statement regarding the discovery of the active principle of supra-renal extract. Dr. George Oliver had been making a large number of clinical observations upon the effect of various organ extracts upon the circulation, but had been unable to arrive at any definite conclusions regarding them. Amongst these was extract of supra-renal capsule, extract of thyroid gland, extract of brain, and so on. He consulted me as to what steps might be taken to arrive at a clearer understanding with regard to their action, and I invited him to investigate their physiological action along with me upon animals in the laboratory. This we proceeded to do; and the result of the investigation was that the majority of the extracts from which he supposed that he had obtained definite results in man gave no indications of physiological activity; whereas, on the other hand, the extract of supra-renal capsule gave such manifest indications of activity that it was quite clear a very important principle was contained within this organ. The properties of this principle we then proceeded to work out, and the result of the investigation led to the discovery of adrenalin, which has proved of immense physiological, and will in all probability prove of great therapeutical and clinical importance. I would also point out that two methods must always be employed in physiology in investigating the functions of an organ. The first is the method of stimulation or addition; the second, the method of removal or subtraction. This may be illustrated by experiments upon such organs as the ductless glands. If we again take the case of the supra-renal capsule, we find that the effects of addition are, first, increased contraction of the blood vessels; second, increased action of the heart; third, contraction of the uterus, and, generally, a tonic influence in all parts supplied by the sympathetic nerve. If, however, the addition be excessive and prolonged, certain chronic effects are produced, the most striking of which is an atheromatous condition of the arteries, which may eventually lead to aneurism. The effect of removal, on the other hand, is in most animals death within a few hours with symptoms of extreme vascular and general muscular weakness, these symptoms being similar to those which result from the gradual destruction of the gland by disease. So again with the thyroid body. The effects of addition or increase of its secretion are the production of a condition of nervous excitation, and certain ill-understood effects on nutrition. The effects of its removal vary in different species of animals. In some the result is *nil*, possibly owing to the fact that there may be small accessory glands of the same structure and function left behind in the body. In other cases, on the other hand, there is a general condition of malnutrition of the connective tissue produced, together with depression of the functions of the higher parts of the brain, sometimes combined with an excitable condition of the lower levels of the nervous centres, leading to increased reflex action and even to convulsions. All these effects can be counteracted by thyroid administration. The second method of experimentation necessitates the keeping of the animal after removal of the organ for days, weeks, or months, in order to understand the condition of the experiment; and the animal used must be one the organs of which are generally similar to those of man, if the conclusions which are drawn from the experiments are to be applicable to man. On this account, and for this second method of experimentation in particular, it is in very many instances essential that dogs should be the subject of experimentation. This is especially true for work upon the alimentary and glandular systems, whereas for work upon the central nervous system monkeys as well as dogs require to be used.

TWENTY-THIRD DAY.

Wednesday, 19th June 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

The Hon. STEPHEN COLERIDGE, called in; and Examined.

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10251. (*Chairman.*) You are Secretary of the National Anti-Vivisection Society, I believe?—No; I am Honorary Secretary; Mr. Stewart is the Secretary.

10252. How long have you held that position?—I have been Honorary Secretary for ten years—since 1897.

10253. You have been good enough to send us a short note of the subjects on which you desire to give evidence. There is only one point that I wish to call your attention to—that is, your reference to the ethical question. I only want to say as regards that that we have had a great deal of evidence upon that question, to which we attach due importance, I hope, and that we have become pretty well possessed of the opinions and views which are held upon it. We are anxious to have the views of your Society upon it, too; but I hope that in consideration of the great amount of evidence we have heard, you will put that part of your case as briefly as possible?—I have prepared a very short statement, which I think I might as well read, because it will, at any rate, afford the Commissioners material for cross-examining me upon it.

10254. Let me ask you first about your Society, so as to have it on the notes. When was it founded?—About 1875 or 1876; just before the passing of the Act.

10255. And it is a very large Society?—Yes, by far the largest in the world.

10256. I dare say you can tell us how many members there are?—I have got the figures here. To illustrate its increasing importance, I give the income for each five years since 1887: £8,651 for the first five years; £15,712 for the next; £26,352 for the next; and £36,097* for the last five years, showing a large increase.

10257. Does membership involve a subscription?—Yes. We have a great number of honorary members, whom we have elected, and a very large number of our branches have very large numbers of members each; but some only subscribe a shilling, and such small subscriptions, so that it is impossible for us to ascertain the names of everybody.

10258. You have not a fixed subscription?—Yes, for headquarters—10s.

10259. And I may take it, I suppose, that while the evidence is your evidence, it will represent generally the opinions of your society?—Yes; whenever I wish to dissociate myself in any way and give a personal note to my evidence, I should say that I was not in agreement. But generally, I am in agreement with my society.

10260. I think now you had better give us your statement, and you will allow me, if I think you are getting off that line at all, to interrupt you?—Yes. I assume that it is not exactly like a court of justice. I have put down, so that there should be no question about the claim of my society really to represent the anti-vivisectionists, the force and strength of the other societies, and, without desiring in any way to set up an odious comparison, I think it is proper that I should say that the income of the London Anti-Vivisection Society, which is next in importance to the National, amounts to about a quarter, and that the income of the British Union amounts to about one-eighth of that of my society. There is the Manchester Society for the Protection of Animals from Vivisection, with an office at 9, Albert Square, Manchester, and an annual income from donations and subscriptions of £205 3s. 8d. There is the Parliamentary Association for the Abolition of Vivisection, with an office at Parliament Mansions, Victoria Street, London, and an annual income from donations and subscriptions of £174 9s. 6d. I think you have had four witnesses from that society. There is the Bristol and West of England Anti-Vivisection Society, with an office at Bank Chambers, Clifton, Bristol, with an annual income from donations and subscriptions of £164 10s. 9d. There is the Friends' Anti-Vivisection Association, with no official address, and an annual income of £118 5s. 5d. There is an Electoral Anti-Vivisection League, with an address at 92, Ladbroke Grove, W., and an income of £83 6s. 4d. There is the Society for the Abolition of Vivisection, founded by the late Mr. Jesse, who left it some money on condition that it never associated itself with any other Anti-Vivisection Society. This exclusive association has an income from donations and subscriptions of £60 9s. 6d. a year. It also enjoys the interest from Mr. Jesse's bequest. I have ascertained that there are 79 members of that society. Then there is the Scottish Society for the Suppression of Vivisection. I have asked for their last year's report, but up to date I have not received it. That is another very old Association, but the membership is not nearly so large as is the membership of the Scottish branch of my own society. There is a World's League of Opponents of Vivisection, which addressed its letters from 32, Sackville Street, which, according to the published report, has 63 members, with a total yearly subscriptions and donations of £56 0s. 1d. There is a Church Anti-Vivisection League, with no official address in its yearly report and an income of £54 4s.† There is the Bolton Anti-Vivisection Society, with no official address, and an annual income from donations and

* *Mr. Coleridge subsequently wrote to say that this sum included the "Coleridge Defence Fund."*

† *The Secretary of the Church Anti-Vivisection League subsequently wrote saying that the League had an official address and an income of* £60 15*s.* 2*d.*

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subscriptions of £39 1s. There is a Society for United Prayer for the Prevention of Cruelty to Animals, especially with regard to the practice of vivisection; it appears to have no office; its income is £33 16s. 11d.* There is the Pioneer Anti-Vivisection Society with no official address, and an annual income of £6 17s. 6d. This exhausts the Anti-Vivisection Associations as far as I am aware, and I think leaves no doubt of the justice of the claim of my Society to represent the cause of anti-vivisection in this country. I cannot give you the exact membership of our own Society. The Birmingham Branch is very large, but they subscribe, some of them, only a penny or twopence, or sixpence. The first striking achievement of my Society, with which I had the honour to be associated, was the prevention of a public meeting held at the Mansion House in 1889, the object of which was to start a fund to erect a Pasteur Institute in England. We prevented that public meeting taking place, and the result was that no Pasteur Institute was set up. England to-day is, I believe, one of the few European countries of importance that is spared a Pasteur Institute, and in England there is no rabies. Another remarkable achievement of my Society has been the long agitation against the diversion of Hospital Funds in London to Medical Schools, which are places registered for vivisection under the Act of 1876. We conducted that agitation with such persistence that King Edward's Hospital Fund were at last obliged to appoint a Committee, composed of Sir Edward Fry, the Bishop of Stepney, and Lord Welby, to inquire into the charges we had publicly formulated. That Committee, after hearing evidence from all parties, made a report which confirmed the statements we had published. It was stated that where money was paid over to schools out of hospital funds, the schools remained "debtors to the hospitals in respect of these pecuniary contributions made to them," and recommended that in future the funds of hospitals and schools should be kept entirely distinct.

10261. That is the particular statement that they confirm?—Yes, that is so. In spite of this remarkable report, however, no school has repaid a penny of the money diverted to it from the hospital; and in some cases the deliberate diversions of money subscribed for the sick poor to schools registered for vivisection continues, with the knowledge of and without any effective protest from King Edward's Hospital Fund, which thereby contemptuously disregards the recommendations of the distinguished Committee appointed by itself. I present to this Commission a publication, for which I take the entire responsibility, briefly called my "Guide to the Charitable," which sets out the financial diversions of hospitals to schools up to the present date (that is for purposes of cross-examination). In the last few years we have conducted public controversies in the "Fortnightly," "National," and "Contemporary" Reviews, and many other periodicals; we have held mass meetings in the largest halls in London; we have founded thriving and earnest branches all over the three Kingdoms. We have conducted a Parliamentary campaign in favour of the Bill I have drafted, and have kept the question of vivisection so prominently before the public that it has been found necessary to appoint this Royal Commission to inquire into it. We claim to represent the consensus of opinion of almost all the greatest names that have adorned the history of England in the field of thought. In Cymbeline the Queen suggests to Cornelius, her doctor, a process of investigation similar to the modern inoculator's experiments under Certificate A. He says:—

> "I will try the forces
> Of these thy compounds on such creatures as
> We count not worth the hanging (but none human),
> To try the vigour of them, and apply
> Allayments to their act; and by them gather
> Their several virtues and effects."

Upon which Cornelius, in a spirit far different from the modern vivisector, replies:—

> "Your Highness
> Shall from this practice but make hard your heart:
> Besides, the seeing these effects will be
> Both noisome and infectious."

From which passage we may claim the greatest name in England and the world for our cause. And it may be interesting to the Commissioners to know that my attention was first called to this striking passage by my late friend, Sir Henry Irving, himself an ardent anti-vivisectionist. Andrew Marvel, in a striking passage, says:—

> "Heaven's King
> Keeps register of everything,
> And nothing may we use in vain;
> E'en beasts must be in justice slain."

Dr. Johnson dismissed the vivisector with one crushing sentence:—"What is alleged in defence of these hateful practices everyone knows, but the truth is that by knives, fire, and poisons, knowledge is not always sought, and is very seldom attained. I know not that by living dissections any discovery has been made by which a single malady is more easily cured. And if the knowledge of physiology has been somewhat increased, he surely buys knowledge dear who learns the use of the lacteals at the expense of his own humanity. It is time that a universal resentment should arise against those horrid operations, which tend to harden the heart and make the physician more dreadful than the gout or the stone."

10262. Where is that from?—I can find it you; it is a fine passage I think. And the cause of anti-vivisection counts, and has counted, among its supporters, living and dead, Cardinal Manning, Lord Tennyson, Robert Browning, John Ruskin, Thomas Carlyle, James Anthony Froude Freeman, the historian, the great Lord Shaftesbury, who was the first president of the society, and filled that office till he died, George Meredith, member of the Order of Merit, General Booth, the Lord Chancellor, Mr. John Morley, member of the Order of Merit, Cardinal Gibbons, James Martineau, Spurgeon, Lord Brampton, Wagner, Leslie Stephen, Sir Edwin Arnold, Mark Twain, Tolstoy, Victor Hugo, Dean Stanley, and a host of distinguished men and women in every walk of life whose opinions upon a matter of conduct cannot be disregarded, and Her late Majesty Queen Victoria, who was in favour of the total abolition of vivisection. As the humble spokesman of this constellation of great writers, I feel the extreme inadequacy of my powers fitly to give voice to their solemn condemnation of painful experiments on animals as a practice repugnant to the promptings of humanity and degrading to mankind, but I desire to be permitted to say that, inasmuch as four generations of my family before me have earnestly supported this humane cause, my own convictions have come down to me as a consecrated inheritance. My society desires to see vivisection totally abolished by law, but it will strive for and accept any measures that have for their object the amelioration of the condition of vivisected animals. We are guided in our policy inside Parliament and without by the consideration that we are the faithful representatives of the hapless creatures in the laboratories who cannot speak for themselves. We base our objections to vivisection as sanctioned and protected by the Act of 1876 on the plainest moral ground. We hold and invite the Commissioners to maintain that the right of man to take animal life involves the duty of taking it with as little accompanying pain as possible. We are prepared to show that under the existing law there is no limit to the agony that may legally be inflicted upon animals, and we abominate a statute that authorises gross inhumanity. We say that to keep an animal alive and torture it is a wicked act, that no profession of lofty motives will ever make that wicked act right; if this were not so, it might be right to rob a bank with the professed intention of devoting the stolen money to the services of the poor. The whole question of man's rights over and duties towards the helpless animals given into his dominion by the Ruler of the Universe, is a moral question, and has nothing to do with science. And if all the benefits so boldly claimed as having been conferred upon physiology by this practice were in deed and truth so received, the issue would remain solely a matter of conduct upon which the least scientific of us has as much a right to be heard as the most expert vivisector; and we maintain that if there be any cruelty inseparable from this particular science every thoughtful man who takes a wide view of human well-being must conclude that it is far better for us all to do without physiology than to be without pity. I have carefully read the evidence of Mr. Byrne on behalf of the Home Office, and as it stands it has, in my opinion, justified to the fullest extent the expressed and published conviction of my society, that the whole truth about the administration of the Act of 1876 by the Home Office could never be elucidated without cross-examination by counsel properly instructed.

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* *The Secretary of the Society for United Prayer for the Prevention of Cruelty to Animals subsequently wrote to say that he Society had an official address and an income of* £89 0*s.* 8½*d.*

We should have preferred to substantiate the charges which it is our duty to make against those officials out of the mouth of Mr. Byrne, their representative; but as that is now apparently impossible, I shall endeavour, on behalf of my society, to prove the indictment we have drawn by unimpeachable evidence. (1) I am here to charge the Home Office officials with repudiating the most important duty deputed to them by Parliament, videlicet, that of protecting animals from unjustifiable suffering; (2) I am here to charge the Home Office officials with having constituted themselves the injudicial defenders of the vivisectors from criticism by my society in the past, and in their evidence tendered before this Commission; (3) I am here to charge the Home Office officials with having appointed inspectors who have displayed such bias that they have thought it their duty not to make detective efforts to protect animals from illegal treatment; (4) I am here to charge the Home Office officials with having made entirely disingenuous statements in their official utterances, and with having constituted themselves the mere spokesmen of the vivisectors; (5) I am here to charge the Home Office officials with accepting the suggestion made to them by some nameless adviser that to starve animals for days is not cruel when they must know perfectly well that if any one of them starved their own horses (if they have any), the nearest policeman could successfully prosecute them for cruelty to animals under Martin's Act; (6) I am here to charge the Home Office officials with suppressing in the Annual Parliamentary Return the names of those who take upon themselves the very grave responsibility of signing the certificates exempting licensees wholly or in part from the obligation to employ anæsthesia in their vivisections, although in the Report of the former Royal Commission we find this clear direction: "We recommend that his (the Home Secretary's) advisers be from time to time elected and nominated by himself. Their names should be made known to the profession and the public." (7) I am here to charge the Home Office officials with shielding the names of such licensees as they know to have broken the law, although the former Report contains these words:—"Abuse of the power conferred by the licence must, of course, render the holder liable to its withdrawal, but this will involve great disgrace," a phrase that clearly indicates that the framers of that Report contemplated the publication of offenders' names, for no great disgrace can appertain to a secret remonstrance of a secret withdrawal of a licence. (8.) I am here to charge the Home Office officials with preparing for the Home Secretary evasive and insufficient replies in the House of Commons to plain questions on the administration of the Act, and with making evasive and insufficient replies themselves in official correspondence with my Society, and with leaving perfectly proper questions unanswered altogether. (9.) I am here to charge the Home Office officials with putting forward annually a Parliamentary Return in which it is asserted, on the official authority of the Government Department, that not a single experiment in thousands inflicted upon animals in Great Britain with its permission can be specified as entailing any pain at all, when all the while this plausible assertion is based upon no better evidence than the bare assertion of the vivisectors themselves, who are not to be expected to report themselves cruel men. (10.) I am here to charge the Home Office officials with placing a certain vivisector year after year beyond the reach of the safeguards erected by the Act to protect animals from illegal treatment by licensees, by giving him permission to vivisect in private places, thereby placing him beyond the possibility of legal inspection; because by Clause 10 of the Act of 1876 the inspector has no right of entry into, or inspection of, any unregistered place.

10263. What is the name of that particular person?—Victor Horsley. (11.) I am here to charge the inspectors certainly, and the Home Office officials apparently, with having made no inquiries, public or private, into the vital question of the character for humanity of the licensees to whom they have delivered over the animals to be vivisected. Although those officials must be aware that the Report of the Royal Commission in 1876 contained this pregnant warning: "It is not to be doubted that inhumanity may be found in persons of very high position as physiologists," and although the Act of 1876 placed upon the Home Secretary the tremendous responsibility of personally protecting animals from that undoubted inhumanity. (12.) I am here to charge the Home Office officials with having placed themselves in improper private confidential relations with a private society composed of supporters of vivisection entitled to no more consideration than the National Society composed of opponents of vivisection.

10264. (*Sir Mackenzie Chalmers.*) Would you mention that society's name?—It has been called in the House of Commons the Society for the Propagation of Scientific Research; its real title is the Society for the Advancement of Medicine by Research. These manifold charges, which I am prepared fully to substantiate before leaving this Commission, justify, in the opinion of my Society, the imputation, which we do not fear to make, that the Act which was intended by Parliament to constitute the Home Secretary the guardian of the vivisected creatures, has been so administered that this solemn responsibility has not been impartially fulfilled. So safeguarded are the vivisectors from the possibility of anybody but themselves knowing what happens in the laboratories, that the opportunity of raising the impenetrable veil that covers their doings can never occur but through their own initiative. Through the hasty temerity of Mr. Bayliss, who brought an action against me, we did for once ascertain from the evidence of that vivisector and his friends the history of the life and death of one at least of the victims of the laboratory. Their evidence proved that under the present law a deep incision can be made into the body of a live dog, and it can be deprived, by the tying up of a duct in its inside, of the proper use of one of its internal organs; it can then be sewn up again and put in a cage, and left in that cage from December to February to see what the result would be of that operation upon it. In February it can be taken out of the cage and a fresh incision can be made into its body to see whether what has been done to it produces inflamation or not. The wound can then be closed up again with a pair of steel forceps. Then with the steel forceps closing up this wound, the living dog can be handed over to a second vivisector, who proceeds to fasten it down tight on to a board and to open its neck with another deep incision, exposing the gland; he can then fix little pipes on to the end of the arteries, he can put a tube into its windpipe, and attach electrodes to its cut-out nerves. The dog in that condition can be tied down on that board for about an hour, and then handed over by this second vivisector to a third operator, who finally puts an end to the miserable dog's life by plunging a knife into its heart. Now the anæsthetics during these prolonged and fearful mutilations can legally be applied by an automatic pump in another room, connected to the dog under the floor by a tube, and this pump, on which alone the insensibility of the dog depends, can be left in the sole charge of a laboratory boy. The vivisectors say that this automatic pump is sufficient to maintain the unconsciousness of the dog throughout the whole operation; but, inasmuch as the dog cannot tell us whether it is unconscious or not, and as no analogy can be set up with human anæsthesia, because no surgeon in his senses would attempt to anæsthetise a man or a woman with an automatic pump in another room under the management of a laboratory boy. I maintain that we may reasonably refuse to accept the opinion of these vivisectors that such means are efficient to maintain total unconsciousness. Further, we have a perfect right to hold the opinion—and we do hold it—that the sufferings of the dog between these vivisections when it was in that cage were very grievous. To procure this priceless information from the vivisectors themselves in the witness-box cost altogether about £5,000; but in my opinion this revelation of what vivisection really is in this country under the present administration of the law was well worth the expenditure. As to the assertions of the vivisectors who have come before you that they individually have never inflicted pain in their experiments, I am not here to ask you to believe them or to disbelieve them; but I assert that the present law, as administered, cannot possibly protect animals from the extremest torture at the hands of any licensed vivisector who happens not to be humane. The comfortable hypothesis of the Home Office appears to be that a person who applies for a licence and receives certificates is necessarily humane. To that hypothesis we distinctly demur. Neither the dignatories of the physiological circles nor the vivisectors who are banded together in the private society for the advancement of their practices, nor the inspectors appointed by the Home Secretary, testify to the personal character or humanity of the applicant for licences

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and certificates. They aver nothing but the applicant's scientific fitness. Scientific fitness is no guarantee of humanity, nor is the habit of cutting into living animals necessarily the concomitant of a tender heart. Therefore we assert that the present law and the present administration of it do not protect animals from the worst conceivable fate. We represent a growing volume of indignation against such a law and such an administration of it. There have been members of my society so moved by the hideous possibilities of the laboratories as to be hardly restrained from violence. The sufferings of dumb creatures appeal with overwhelming force to an ever-increasing proportion of our countrymen and countrywomen, and, as I believe, to those who are the truest judges of right conduct. I am reluctant to make a personal attack upon any vivisector; I have come here on behalf of my society to denounce a system and a practice rather than to expose individual barbarities. But I may perhaps be permitted to say that the permission recently given to a vivisector to drown, resuscitate, and drown again dogs without any anæsthetics, which have been already alluded to here, have been regarded by my society with particular detestation.

10265. (*Colonel Lockwood.*) That is Schäfer you mean?—Yes, Schäfer?—I know not how many thousand upon thousands of men, women, and children have been saved from drowning the world over by dogs since they became attached to mankind as their faithful friend. Not long ago I read of a dog that had saved ten lives from drowning, and perished in a gallant but unsuccessful attempt to save an eleventh. It seems to many of us an act that is degrading to humanity to reward all this splendid devotion by taking dogs and subjecting them to these horrid experiments, and it seems to me inexplicable that any man could be found to do such dreadful business. The physiologists tell us that they can learn more from the dog than from other animals. To that proposition we oppose no contradiction. From the dog we can learn courage, constancy, loyalty, steadfastness, faith, love; but it is not these qualities that interest the vivisector; it is the weight of its spleen or the pressure of its blood that elicits his curiosity, and he digs into its living body in his horrid quest. I desire to bring before the Commissioners the sixteen experiments performed by Dr. Crile in Sir Victor Horsley's laboratory, in one of which the foot of a dog was deliberately crushed "under incomplete anæsthesia." Apart altogether from the question of pain, this series of experiments, involving the most repulsive operations, in which every conceivable outrage is perpetrated upon bodies of the victims, have filled decent people who have faced their perusal with disgust and horror. I am here to say on behalf of my society that if physiology cannot be pursued without such loathsome practices it is high time it were abandoned. We are confident that we are expressing the feelings and sentiments of the vast majority of Englishmen when we assert that the mere power so to treat living creatures does not involve the justification of such treatment as a matter of conduct. The knowledge of what will happen to a dog when its feet are crushed in pincers and boiling water is poured into its inside seems to me, as a layman, as remote from any practical service to humanity as is the discovery of a variable star or the five hundred and sixty-eighth asteroid. But, looking through a telescope and tearing a living dog to pieces are very different human actions, and this fundamental difference must ever place physiology as now pursued in a degraded position altogether distinct from that of the noble sciences. Physical pain is perhaps not the worst evil that can afflict mankind. To us, "with such large discourse looking before and after," bodily pangs are less insufferable than remorse, ruin, jealousy, or a broken heart. But with animals this is not so; to them physical pain is the worst of all evils. They have no armour of the mind wherewith to summon to their aid an intellectual fortitude. They can look up to no martyr's crown, they cannot bring their miseries to the feet of a pitiful God. Therefore it is that our sympathies are so deeply moved by the spectacle of animal suffering, therefore it is that we say that to pursue knowledge through the agony of animals is an act only possible to a man whose heart is dead. If it could be proved that anybody's life had been prolonged by these practices our answer is that if the vivisectors could make us all live to be a hundred it would be a miserable exchange for an extinction of pity in the human heart. It is comparatively unimportant how long we live; what matters is, how we live. There is one more pathetic concomitant of vivisection which is totally ignored by the physiologists, but which I desire to enforce upon the attention of the Commissioners. Grievous and terrible as are we believe the physical sufferings now permitted by law to be inflicted upon dumb animals, this is not all for which the physiologists and the Act of 1876 are responsible. The knowledge that horrible mutilations may be daily and hourly executed upon the bodies of living creatures with no adequate security, for their insensibility makes very many humane people profoundly miserable, it rises day and night between them and their peace of minds, it haunts their lives waking and asleep, it deprives them of joy in this world which otherwise might be theirs. I am here to ask that this mental anguish, so immense in its aggregation, should not be regarded as of no account by this Commission. In all my reading of the writings of the vivisectors descriptive of the most appalling experiments, I have never met with a single expression of regret that such treatment of helpless creatures was a painful necessity, or a single word of sympathy with the miserable victims in their laboratories, and I am here to tell the Commissioners that the 300 odd vivisectors in the kingdom are responsible for a rising and widespread feeling of bitterness against the whole medical profession, with which in the public eye they are confused. This in the future may be not the least price the physicians of the country will find they have to pay for their supine acquiescence in vivisection as it is now allowed by law. The Commission by recommending Parliament to afford a real protection to vivisected animals have it in their power to check the spread of a deep and abiding enmity to a whole profession, which will be as deplorable as it will be inevitable if the law remains as it is. It will be observed that my Society has been forced to the conclusion that, by placing the interests of the animals in the hands of the Home Secretary, the framers of the Act of 1876, unwittingly, no doubt, made a fatal mistake. The Home Secretary has practically delegated much of his responsibility to the Home Office officials, who have in their turn delegated their responsibility largely to a private association of vivisectors called into existence for the very purpose of representing their own interests as opposed to the interests of the animals as represented by my society. And a state of things has thereby been created and maintained for years which we invite all impartial people to condemn as little short of a public scandal. What might have happened had a Home Secretary come into office who held the opinion that to inflict any amount of suffering was justifiable if it were claimed that science might possibly be advanced thereby, must be as obvious as deplorable, unless the solemn aspiration with which the Royal Commission of 1876 closed their Report is to be regarded as having been entertained in vain. "We trust," they said, "that your Majesty's Government and Parliament of this kingdom will recognise the claim of the lower animals to be treated with humane consideration." Under the present law what has happened in the past and what legally might happen in future fill those I represent with indignation and alarm; and we earnestly invite this Commission to consider with care and to recommend to Parliament the Bill which I have drawn, and which I now present to it. And now before I proceed to advance from these general statements to the production of the evidence for their support, it is my duty on behalf of my society to enter very respectfully my protest against the exclusion of the Press from this Commission, and the exclusion of counsel to represent our society. The ultimate publication of these proceedings will not contain much that the public may rightly claim to know, and of which the presence of the Press would have kept them informed. But my society have deemed it nevertheless to be their duty to direct me to bring their case before the Commission, and, of course, what I myself may say here is not said confidentially, but is intended to be entirely and with as little delay as possible public.

10266. (*Chairman.*) What are the matters of fact that you are proposing to deal with?—I now propose to deal with these charges which I bring against the Home Office seriatim. The first charge that I bring against the Home Office is that of repudiating the most important duty deputed to them by Parliament, viz., that of protecting animals from unjustifiable suffering. The Report of the Royal Commission of 1876 recommended that "Experiments upon living animals, whether for original research or demonstra-

tion, should be placed under the control of the Secretary of State." It further says: "The holders of licences should be bound by conditions, and breach of the conditions should entail liability to forfeiture of the licence." Now this is what I am emphasising: "The object of the conditions should be to ensure that suffering should never be inflicted in any case in which it could be avoided, and should be reduced to a minimum where it could not be altogether avoided." What I have to say about that is this: I maintain that when you are reading the old Act of 1876, it is essential to read it in conjunction with the recommendations made by the Commissioners who reported in 1876, and upon whose Report that Act was passed; and, therefore, I say that the Act of Parliament clearly laid upon the Home Secretary the duty of seeing that suffering should not be inflicted if it could possibly be avoided, and in fact placed the whole question of pain in his hands.

10267. (*Sir Mackenzie Chalmers.*) Would you give us the actual words you rely upon?—Yes—"the object of the conditions should be"——.

10268. No—the words of the Act, please; what are the words of the Act?—I am reading from the Report of 1876.

10269. The Report was afterwards followed by the Act?—That is what I have said. I have said that, in my opinion, and I hope in the opinion of the Commissioners, the Act of Parliament must be read in conjunction with the Report upon which it was founded, and that where distinct recommendations are made in the Report it is the duty of the Home Office to carry out those recommendations within the four corners of the Act.

10270. (*Chairman.*) You could not quite say that; I think no lawyer could say that that was his duty under the Statute. It may be an independent duty?—An independent duty, yes. If you look at the Statute as no doubt you have, the Statute begins by calling itself "An Act to amend the law relating to cruelty to animals," it therefore is a law entirely dealing with the question of pain. The whole Act is to place the regulations protecting animals from unjustifiable suffering in the hands of the Home Secretary and his officials. I mean, of course, that the Home Secretary personally, I presume, delegates it to his officials. Therefore I say that if the Act is read as the result of that Royal Commission, it must be taken that the duty is placed upon the Home Secretary to protect animals from unjustifiable suffering. Now, having as I consider established that, I refer you to Question 129, where Colonel Lockwood asks Mr. Byrne "What expert opinion guides the Secretary of State for the Home Department in deciding as to the painfulness of an operation"; and the answer is "The Secretary of State has not to decide whether an operation is painful or not." Therefore I say that the Home Secretary repudiates, in the words of Mr. Byrne, who, I presume, represents him, the duty which, in my opinion, was placed upon him under the Act.

10271. (*Sir Mackenzie Chalmers.*) Would you read the context there. The question was a question of statistics there?—(*After referring to the evidence.*) It is a perfectly fresh question where Colonel Lockwood begins; it has no relation whatever to the question before it.

(*Chairman.*) It appears to be so.

(*Sir Mackenzie Chalmers.*) But it was on the discussion of the new form of Returns—it arose out of that.

10272. (*Chairman.*) Will you continue, please?—Further down I find, at No. 163, that Sir William Collins asked Mr. Byrne—this is again on the question of pain. "Is that regarded as a painful procedure?" Sir William asked at Question No. 162 in regard to inoculation; and Mr. Byrne replied: "It is not a painful procedure, but it is considered a painful experiment." Then he is asked: "And is so classed?" and his answer is: "There is no classification of experiments in the last Report into painful and not painful; but it is regarded as undoubtedly painful by the Home Office." So that on one page the Home Office repudiate all responsibility about pain, and on the next page they say that they do regard a certain class of experiments as painful. Therefore it is quite clear that they do not altogether speak consistently upon this matter. Then the next question is: "Do I rightly understand that there was formerly a classification into painful and painless?" and the answer is: "Yes; if you will be so good as to look at the end of my memorandum relating to the Annual Return under the Act, you will find that at various stages attempts, mostly unsuccessful, were made to divide the experiments returned into those painful and those painless. That has now been given up, and the last Return does not even profess to do so." So that the Home Office have absolutely abandoned all attempt, so far as I can see, to take upon themselves the duty which I maintain is theirs. Perhaps now, while I am on Certificate A, I might say a word or two about Certificate A. I notice—and this is very interesting, I have made a great study of this—that before 1888, when Mr. Busk was the Inspector, in all the Annual Reports (and I have read them all very carefully through), there is no allusion of any kind to any conditions being attached to Certificate A. Down to 1888 I know Mr. Busk was Inspector, and there was no allusion anywhere in the Reports, as I have said, to any conditions being attached to Certificate A. And therefore it is a fair deduction that down to 1888, under Certificate A, any amount of torture might have been inflicted, because it relieves the operator altogether of the necessity of using anæsthetics in his operations.

10273. I do not quite follow why you say that?—Certificate A represents Sub-proviso 2: "Experiments may be performed without anæsthetics on such certificate being given as in this Act mentioned, that insensibility cannot be produced without necessarily frustrating the object of such experiments." Each year there is a Report and letterpress, and in the letterpress of late years there have been allusions to conditions which have been always attached to Certificate A before it was issued. Before 1888 there is no evidence from the letterpress and Reports, that any such condition was ever attached to it before it was issued; and therefore, I say, that down to 1888 there seems no doubt that under the administration of the Home Office permission was given to inflict any amount of torture upon animals, because experimenters were permitted to perform experiments without anæsthetics simply; all they had to do was to say that insensibility could not be produced without frustrating the object of the experiment; so that, if the experiment was to find out the result of pain, for instance, it obviously would not have been of any use if the animal did not feel the pain. I am only bringing this forward to show that under the present law any amount of torture not only may be, but I am afraid has been inflicted.

10274. (*Sir Mackenzie Chalmers.*) Now, let us go to since 1888?—I am going on to since 1888. In 1888 Mr. Erichsen became Inspector, and in his report for 1888 we find these words: "The 'inoculation condition' introduced last year, by which it was rendered imperative on the part of the experimenter to destroy the animal under anæsthetics, provided pain had been developed as the result of the conditions to which the inoculation gave rise, has been in all cases enforced." I regard that as a very proper and humane proviso that should be added to Certificate A, and it clearly apparently was added to Certificate A in 1887; in the Report of 1888 we find it. In the Report of 1889 we have an announcement to the same effect—namely, that as soon as pain is developed under inoculation the animal is to be destroyed. In 1890 I find a grave alteration was slipped into this Report. In this Report we find the "inoculation condition" is thus defined: "This condition renders it imperative on the operator to destroy the animal under an anæsthetic if severe pain has been induced, so soon as the result of the experiment has been obtained." That is a very different condition, you see. That lets in any amount of pain, provided the experimenter is of opinion that the result of his experiment has not been obtained; whereas, before that, there is nothing about the result of the experiment having been obtained. "If pain ensued, animal destroyed," was a further condition down to 1890. Therefore we see in 1890 this slips in, this permission of pain. In 1891 the words are: "The animal shall be killed if pain result." That appears to be a reversion again to the more humane conditions. And practically no allusion is made to the matter again under Dr. Poore's *régime*.

10275. (*Colonel Lockwood.*) Dr. Poore succeeded to Erichsen?—Dr. Poore succeeded to Erichsen in 1892. The last of Erichsen's Reports says: "The animal shall be killed if pain result." It seems that Erichsen reverted to his more humane condition before he retired, or died—whichever it was, I forget. Then

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comes in Dr. Poore, and Dr. Poore never says a word about it. He maintains the mystery of silence as to the conditions under which Certificate A is issued, and in 1900 he retires. Then in 1901 Mr. Thane takes office; and this is what he says in his first Report: "In the event of pain ensuing, the animal to be killed 'as soon as the main result of the experiment has been attained.'" That obviously is, in my opinion, an inhumane condition. It lets in, so far as I can see, any amount of pain under Certificate A. The vivisector has only to form the opinion that the result of his experiment has not been obtained, and the animal can be subjected to as much pain as he thinks fit to inflict upon it. That is all I have to say on Charge 1. My next charge is that I charge the Home Office officials with having constituted themselves the injudicial defenders of the vivisectors from criticism by my Society in the past, and in their evidence tendered before this Commission. I wish to draw the attention of the Commission to Dr. Crile's book on "Surgical Shock."

10276. (*Sir Mackenzie Chalmers.*) There is one preliminary point I should like to ask you. On what do you base the distinction that you draw between the Home Office and the Home Secretary—between the officials and the Home Secretary?—I draw it on this ground. I suppose you are aware, like everybody else, that the answers prepared for the Home Secretary are prepared in the Office; I have common sense enough to know that, and I presume that the Office is therefore responsible for the answers given by the Home Secretary; that is all. I make no distinction, except that I do not attribute them to the Home Secretary himself.

10277. You do not surely regard the Home Secretary in answering to Parliament as a mere mouthpiece—a sort of telephone from the Office?—It is a matter of degree.

10278. (*Chairman.*) That is what you do regard him as, I understand?—The answers, I have not the slightest doubt, are prepared for him in the Office.

10279. They are prepared and drafted in the Office, no doubt, but the Home Secretary is responsible; and not only is he responsible, but, I think, not infrequently he suggests that they should be different?—It is not for me to draw a distinction; I am not inside the Home Office.

10280. (*Sir Mackenzie Chalmers.*) Constitutionally the Office does not exist; there is only the service of the Home Secretary, but practically, of course, everything has to be prepared for the Home Secretary, and he may or may not, according to his individuality, exercise his right?—That is too technical a point for me; that is for the Home Secretary and his officials to settle between them. I am quite aware that all the charges which I am now bringing have nothing to do with Sir Mackenzie Chalmers, because he was not there; and further, the Home Secretaries are gone too. There stand the permanent officials.

10281. (*Chairman.*) You put the blame on the permanent officials?—Certainly.

10282. (*Dr. Wilson.*) On the Department?—Yes, I regard the Department as my adversaries in this matter.

10283. (*Chairman.*) I think my expression was more accurate, because the Home Secretary is one of the Department; he is the head of it?—Yes.

10284. (*Sir Mackenzie Chalmers.*) I only wanted to know the reason for it?—That is it. I refer now to this book on "Surgical Shock," and somebody had better verify my quotations. Would you like it, my Lord?

(*Chairman.*) As this is relating to the Home Secretary, perhaps Sir Mackenzie had better have it before him, and I will listen.

(*Sir Mackenzie Chalmers.*) I have never seen it. (*The book was handed to Sir Mackenzie Chalmers.*)

(*Witness.*) If you look at Experiment CXXXIII—I do not know the exact page—you will see the description.

10285. Let us see who Mr. Crile is?—He is an American who came over here and had a licence given him from the Home Office to perform certain experiments, and he did them in Sir Victor Horsley's laboratory. The first 16 were done in England, and I will not allude to the others, except that just in passing I must allude to two which were not done in England; and I will tell you why. I want to show that the personal taste of Dr. Crile does not prevent him from torturing animals. In experiment CXXXIII. we find these words (I am just going beyond England for the moment); he says "Experiment lasted two and a-half hours. Flame was applied to the dog's paw, and we are told, in the control experiments, as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame.'"

10286. That was an American experiment?—I think so; it was not done in England, anyhow.

10287. And he put as the anæsthetics chloroform and ether?—I do not care what they were; they were not full.

10288. (*Chairman.*) You only read that experiment to show that Dr. Crile is capable of performing what you consider a cruel operation?—Yes; and there is one other experiment, No. CXXVII., just before that one, "The animal did not take the anæsthetic well, and a part of the experiment was made under incomplete anæsthesia, the anus and rectum of this dog were then 'dilated.'" These quotations I merely read to show that the personal taste of Dr. Crile does not prevent him from inflicting torment, in my opinion. Now I come to what was done in Sir Victor Horsley's laboratory. I will go by steps. On page 17, Dr. Crile states "In the first 16 experiments which were carried out in the University College, London," so and so.

10289. You now, I understand, are going to read descriptions given by Dr. Crile of operations carried out by himself in Sir Victor Horsley's laboratory?—Yes, that is how he describes them himself in his own book. On page 23, Experiment V, performed on the 8th of May, 1895, is thus described:—"Healthy male foxterrier, three years' old. Central pressure in femoral. Preliminary section of the inferior branches of the right stellate ganglion, by dissection between the scapula and the spinal column, resecting two ribs. Under incomplete anæsthesia, crushing of foot caused a very sharp rise, followed by an equally sharp decline of pressure. This was repeated several times." Here we have Dr. Crile's own description of crushing the animal's foot under what he himself calls "incomplete anæsthesia." That was done by Dr. Crile under a simple licence, which provided that the animal must be under complete anæsthesia during the whole experiment, and be killed before it recovers consciousness. I want to point out that in Dr. Poore's Report these 16 experiments were not described as painful, and therefore we must assume that the Home Office regarded them as painless, or chronicled them as painless. Over Dr. Poore's signature are these words:—"In experiments performed under licence alone, the animal suffers no pain, because complete anæsthesia is maintained from before the commencement of the experiment until the animal is killed." Therefore the Home Office Inspector, in his own Report, states for the public delectation that animals when vivisected under licence alone are always under complete anæsthesia.

10290. Are the words "incomplete anæsthesia" in the account which Dr. Crile gives of his own experiment, the account which has been given in by him to the Home Office?—Whether it was sent in to the Home Office or not I do not know.

10291. It makes a difference, when you are bringing a charge against the Home Office?—I laid the book before the Home Office afterwards.

10292. It makes a difference whether Dr. Poore was informed that it was done under "incomplete anæsthesia." We are informed in this book by Dr. Crile that it was so; but I presume that Dr. Poore's Report would be founded upon the Report sent in to him by Dr. Crile?—We have it in evidence—I have read it—I cannot at the moment refer to it, but I am sure I can find it—that it is an instruction to all vivisectors to send to the Home Office any pamphlets or publications detailing and accounting for the experiments which they are licensed to perform; and, therefore, I assume that if Dr. Crile did his duty and fulfilled the conditions upon which his licence was given to him, he must have sent that book to the Home Office.

10293. We do not know that it was printed at the time?—Anyhow, I sent it to the Home Office later on.

10294. I mean, that we do not know whether at the time it was before Dr. Poore?—That is a matter for the Home Office to answer. I do not know; I cannot tell.

10295. (*Sir Mackenzie Chalmers.*) It is an American

book, and curiously enough it does not seem to have any date. It seems to have been an Essay which was awarded the Cartwright prize for 1897?—Yes.

10296. That is two years afterwards?—Yes.

10297. (*Chairman.*) That book itself could not have been submitted to the Home Office at the time. However, when we examine the Home Office Reports, we shall see what there was?—I am comng on to that in a moment. I then drew the attention of the Home Office to these experiments which were done in Sir Victor Horsley's laboratory.

10298. When you say then, will you tell us when that was?—I wrote to the Home Office on the 18th May, 1899.

10299. (*Sir Mackenzie Chalmers.*) That is four years afterwards?—It may have been.

10300. From 1895 to 1899?—Yes, that is right. I do not have these books sent to me, you see. Somebody at last sends me a thing, and I at once act when I see it. Then the Home Office asked me to lend them the book—that happened next; so I lent them the book; and their answer was that on receiving my letter, "The Secretary of State caused investigations to be made into these experiments, and the information he has received from competent persons, who either witnessed most of the experiments, or have read the account of them, is to the effect that in no instance had the animals experimented upon been subject to pain."

10301. (*Chairman.*) You told us that you wrote to the Home Office calling attention to this. Was your letter calling attention to Dr. Crile's book generally, or was it calling attention to the statement that this particular dog was under incomplete anæsthesia?—I drew their attention to the experiments which Dr. Crile had done in Sir Victor Horsley's laboratory, and I drew their attention to their own statement in their corresponding statement for the same year alluding to complete anæsthesia, and pointed out the discrepancy. I say that these investigations which the Home Office made can, so far as I can see, have had no other nature than inquiry from the persons responsible, and, therefore, on their defence. It is quite incredible to suppose that inquiries as to whether a dog was put to torture in a particular laboratory of the persons who are responsible for what goes on in that laboratory, will result in anything but a distinct assurance that no pain was inflicted. Human nature being what it is, you cannot expect anything else. I say that those investigations can have had no other nature than inquiry from the person whose conduct is impugned—whether he did it or not; and I say that to make such an inquiry as that, and then write back to me and give me the result of those inquiries as settling the matter, is, in my opinion, simply defending the vivisectors from criticism, and nothing else. They went on and said: "The use of the term incomplete anæsthesia has probably given rise to the misunderstanding. With regard to this," the Home Secretary says, through the hand of Mr. Cunynghame, "the Secretary of State is advised that there are various degrees of anæsthesia. In 'complete anæsthesia' the narcosis is very profound, and the 'reflex movements,' that is the involuntary movements in response to an external stimulus, are suppressed. One of the last of these reflex movements to disappear is the blinking of the eyelids when the surface of the eyeball is touched, thence called the 'corneal' or 'conjunctival reflex.' But short of this is the condition termed by Dr. Crile 'incomplete anæsthesia,' in which the creature is quite insensible to pain, although the corneal and other reflexes can still be obtained." Now "it is quite usual for even severe operations to be performed on man in this state of 'incomplete anæsthesia.'" That is the statement of the Home Office, presumably advised by—well, I do not know whether it was Dr. Poore, I suppose it was; they do not tell me by whom they were advised, and of course I cannot tell.

10302. On what ground do you assume that it was Dr. Poore?—I would assume that Dr. Poore is their adviser, and in the next charge I shall proceed to show that Dr. Poore was a man of very strong bias, wholly incapable, unless through very serious and strenuous effort, of forming an unprejudiced opinion on such a matter as this.

10303. I am afraid that we find a great many of those people on both sides?—No doubt, my Lord, but not, I should hope, in an official position. I am not in an official position. I admit all sorts of prejudices.

10304. I would not go so far as to say wholly incapable of forming an opinion; you might say considerably prejudiced?—Well, considerably prejudiced—biassed I will say. Having received that letter, of course I had to consider what was my position. Here was the Home Office ready to tell us, on the advice of some nameless adviser who no doubt took the responsibility upon himself, that it was "quite usual for severe operations to be performed on man in this state of incomplete anæsthesia," that is, before the corneal reflexes were abolished. Well, I thought that I would test that, and I took the liberty of writing a letter to a certain number of the most important and eminent surgeons of the day, informing them that I had been told on very high authority that "surgeons now frequently perform severe operations on man under incomplete anæthesia, that is, before the corneal reflexes are abolished." Those are the words of my letter to the surgeons. I put it perfectly fairly, exactly as the Home Office put it to me. The replies were very remarkable.

(*After some deliberation.*)

10305. (*Chairman.*) As some of the Commissioners wish the letters to be read, will you please read them?—In the first place, I ought to read my letter to the surgeons. It was as follows: "Dear Sir,—A statement has recently been made to me in writing by a very respectable authority that surgeons now frequently perform severe operations on man under incomplete anæsthesia, that is, before the corneal reflexes are abolished. I know of no way of answering this statement but by obtaining definite information from persons like yourself, whose large experience renders their authority on such a matter conclusive. I should, therefore, really be very much obliged to you if you would be so good as to enable me clearly to assert on your authority that, provided complete anæsthesia can be given with safety to a patient, and provided the nature of the operation itself does not render it dangerous to the patient's life, complete anæsthesia is always administered in *severe* surgical operations. You may possibly be aware that I am one of those who on humanitarian grounds oppose the vivisection of animals as practised in this country, but I hesitate to believe that patients in hospitals are treated with inhumanity. I hope you will pardon this intrusion upon you, which arises from the prominent position you occupy in your profession.—Believe me to remain very faithfully yours. (Signed) Stephen Coleridge." I will now read some of the answers that I received. I ought to say that I am not going to pick out those which favoured me at all. I will give you the whole. There were only two of an adverse view who associated themselves with the Home Office view, and those two replies were from licensed vivisectors who were unconnected with any hospital. The first is from Dr. Bond, F.R.C.S., senior surgeon and lecturer at the Westminster Hospital. He says: "I think you have been very much misinformed, and it would be very interesting to me to know the very high authority on which your information is based. Hundreds of operations are performed yearly at Westminster Hospital, and never to my knowledge is the operation commenced under incomplete anæsthesia." I informed Dr. Bond afterwards that the information reached me in a letter from the Home Office. Dr. Edmund Owen, F.R.C.S., Member of the Council of the Royal College of Surgeons, senior surgeon at St. Mary's Hospital, and senior surgeon at the Hospital for Children in Great Ormond Street, says: "Certainly, in my experience complete anæsthesia is *always* secured in severe surgical operations."

10306. (*Mr. Tomkinson.*) Are those the whole of the letters?—No.

10307. Did the first letter not refer to the question of the eye?—No; they answered my letter. I defined incomplete anæsthesia in my letter, "that is, before the corneal reflexes are abolished."

10308. There is no allusion to that in the reply?—No, they have taken my definition and used the words "incomplete anæsthesia." Dr. Timothy Holmes, F.R.C.S., Vice-President of the Royal College of Surgeons, and treasurer and consulting surgeon at St. George's Hospital, says: "I differ entirely from your informant who says that it is quite usual for even severe operations to be performed on man in a state of incomplete anæsthesia. . . . . Never did I hear of performing serious operations with incomplete anæsthesia, and if you know anything of operative practice you would know that it would be as embarrassing to

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the operator as painful to the patient. . . . . If you accept this statement as true you are quite at liberty to make use of it to your informant; or if your informant will, in his own name, repeat his statement in any respectable paper, I will in my own name repeat the above." Dr. Jonathan Hutchinson, F.R.C.S., late President of the Royal College of Surgeons, consulting surgeon at the London Hospital, and consulting surgeon at the Royal London Opthalmic Hospital, says: "I have not the least doubt that when complete anæsthesia is considered safe, and on the patient's part desirable, it is always secured." Dr. G. May, consulting surgeon at the Royal Berkshire Hospital, says: "I have been attached to the Royal Berks Hospital about forty-five years, and have never known a case of operation where anæsthesia was not complete. This is done not only to spare pain, but because it is thought to be less dangerous." Dr. T. J. Walker, M.R.C.S., who has been for a very long time senior surgeon at the Peterborough Infirmary, says: "Taking my authority for what it is worth, you may assert on my authority that, provided complete anæsthesia can be given with safety to a patient, and provided the nature of the operation itself does not render it dangerous to the patient's life, complete anæsthesia is always administered in severe surgical operations." And this last one is most interesting. Dr. Thomas Bryant, F.R.C.S., Surgeon Extraordinary to Her Majesty the Queen, consulting surgeon at Guy's Hospital, and late examining surgeon at the University of Cambridge, says: "I have received your circular, and read it with surprise, as you clearly have been hoaxed." I replied to that letter that the august quarter from which the information emanated seemed to me to preclude such an hypothesis. I had two replies from two licensed vivisectors, one from Professor Horsley, to the effect that he was out of the country, and the other from Professor Schäfer, that the Home Secretary's advisers are quite correct, and that incomplete anæsthesia "is consistent with complete unconsciousness." As I say, Sir Victor Horsley did not answer, but Professor Schäfer at that time was not, and, so far as I know, still is not in any way connected with an hospital.

10309. You said that there were two who differed?—I thought that there was. I see one did not answer; it is my mistake.

10310. (*Dr. Gaskell.*) Did you say that Professor Schäfer said that the Home Office statement was correct?—Yes, in reply to that letter, although I gave no indication that it was the Home Office at all; he deduced, no doubt, that it was the Home Office.

10311. You had not told him that it was the Home Office?—No.

10312. (*Chairman.*) Are those the whole of the letters?—Yes, except one which I left out.

10313. I did not mean were they all the letters, but have you read the whole of each letter?—I believe so; but I can get the originals. I am not sure. I did not read one from Dr. Herbert Snow, surgeon at the Cancer Hospital, who says, "You must have been misinformed so far as my knowledge goes upon the point at issue," as he has been a witness here.

10314. (*Colonel Lockwood.*) Those are not *précis* of the letters; they are the entire letters?—So far as I know. I placed all the replies before the Home Secretary in a letter to the Home Office, and I received no further reply from them. I remain of the opinion that those collected opinions of those great men outweigh the anonymous advice received by the Home Office. I will not say that Dr. Poore, but whoever it was, I think he misinformed them; and at any rate, I maintain that with those letters before them the Home Office ought to have recognised that they were of great weight. But they never answered me in any way, and, so far as I know, Dr. Crile was never censured.

10315. (*Sir Mackenzie Chalmers.*) Do you know when Dr. Crile returned to America?—No. I will not be quite certain, but I think that even subsequently to those experiments, and subsequently to the receipt of all those letters from me by the Home Office, Sir Victor Horsley, in whose laboratory the crushing of that dog's foot under incomplete anæsthesia took place, still received permission from the Home Office to vivisect where he liked, and not in any registered place.

10316. (*Chairman.*) Was Dr. Crile at that time on a visit to England?—So far as I know.

10317. He never took up his abode here to practice?—No, he went back to America, and he has issued another book since in America. Now the next point I will take up on this same charge is in relation to a question asked by Mr. Weir of the Home Secretary.

10318. (*Colonel Lockwood.*) You mean the present Member for Ross and Cromartie?—Yes, I think he is. "I beg to ask the Secretary for State for the Home Department if he will state the number of persons in England, Scotland, Ireland and Wales respectively, licensed to practice vivisection, and how many of them hold certificates to dispense with anæsthetics." Sir Matthew White Ridley replied, "The number of persons licensed at the present time in England is 145; in Scotland, 52; in Wales, one. The number holding the certificate dispensing with anæsthetics is, in England, 86; in Scotland, 30; in Wales, none. The only figures in my possession as regards Ireland are those for 1895. In that year the number of licensees was six, of whom one held a certificate dispensing with anæsthetics." Now comes the point; this is a voluntary statement on the top of the answer: "In giving the honourable Member these figures, I may remind him that the certificate in question is never given for operations involving serious pain, but only for such operations as inoculations or hypodermic injections." That answer, I am going to assume, was prepared for Sir Matthew White Ridley in the Home Office, and I say that that answer might be intended for the public to believe, and undoubtedly had the effect, and would have the effect, of persuading the public that in all cases of certificates exempting the licensee from the use of anæsthetics, such certificates are only issued for such operations as inoculations or hypodermic injections, when all the while there is Certificate B being issued; and I say that such an answer as that is not a fair answer. That statement of Sir Matthew White Ridley's, I maintain, is quite unfair. It really only refers to Certificate A, and Certificate A plus E, and omits all reference to experiments under Certificate B. But the question does not omit reference to Certificate B. The question is: "How many of them hold certificates to dispense with anæsthetics." I say Certificate B dispenses with anæsthetics.

10319. (*Sir Mackenzie Chalmers.*) No?—Certainly Certificate B dispenses with anæthetics immediately after the initial operation is over; and under Certificate B the moment the initial operation is over (the opening of the animal and fixing the electrodes to the ends of the nerves and tubes down the veins), there is nothing in the law and nothing in the certificate to prevent the animal remaining disembowelled on the table for hours while observations are made upon its blood pressure and various things, while stimulations of electricity are passing through it. Under the present law there is nothing to prevent it, and I say that that is what horrifies the public; and that is carefully hidden in this answer.

10320. (*Chairman.*) I have not been able to follow you, not having the question and answer before me, but do you say that looking at the question the answer is distinctly a misstatement, or is it something which a person who did not know the Act might misapprehend?—That is it.

10321. That is a very different thing, of course. A person who asks a question in the House of Commons is generally supposed to know with reference to what he is asking it, and he can understand the answer?—Perhaps I have made a mistake. The question, in my opinion, involves Certificate B; the question certainly involves "How many of them hold certificates to dispense with anæsthetics." Certificate B is a certificate dispensing with anæsthetics.

10322. (*Sir William Church.*) No?—I beg your pardon; it is so worked in practice, and it is so chronicled every year in the report under certificates dispensing with anæsthetics.

10323. (*Sir Mackenzie Chalmers.*) The animal is allowed to recover. May I just call your attention to the words of Certificate B; it is on page 7. Have you got a copy?—I have not got the Certificate here at the moment, but I know it by heart.

10324. It authorises a person "To perform on living animals certain experiments described below, such animals being, during the whole of the initial operation of such experiments under the influence of some anæsthetic of sufficient power to prevent their feeling pain"?—Yes, during the whole of the initial operation of such experiment, the experiment consisting of first opening the dog, and then chronicling what may

happen to the blood pressure and other things, during hours, perhaps, of subsequent experiments.

10325. (*Dr. Gaskell.*) No; that is all the initial experiment?—No, I beg your pardon.

10326. (*Chairman.*) I do not think this comes to much, really, as a charge against the Home Office; because it is admitted that the answer to the question is one which, technically to a person who understood it, is a correct answer, but one which might lead a person who was not familiar with the Act to misapprehension. It is really putting too much upon the Home Secretary to suppose that he is to make his answer with reference to a person who does not study the Act?—The question is a very clear one. The question asked by Mr. Weir was how many of these gentlemen hold certificates to dispense with anæsthetics, and the Home Secretary gives only those who have Certificate A. He suppresses apparently those who have Certificate B, because he adds the rider: "In giving the honourable member these figures I may remind him that the certicate in question"—that must mean only Certificate A—"is never given for operations involving serious pain, but only for such operations as inoculations or hypodermic injections." I say that that is a disingenuous answer.

10327. To me, hearing you read it, it seems rather a forced construction to put upon it. I think that the unbiassed person, whom we have not yet been able to find, would say that it was rather a forced construction to put upon it?—The question involves all certificates dispensing with anæsthetics.

10328. (*Sir William Collins.*) Are the words "Certificate A" mentioned in the question?—No; the words are: "Certificates to dispense with anæsthetics." I say that Certificate B dispenses with anæsthetics, and therefore it should have been included in the answer.

19329. Did you say that you were going to refer to some return indicating that Certificate B is classed with those certificates dispensing with anæsthetics?—Certainly. There is in the last Report: "Certificates held by each licensee dispensing with the obligation to kill the animal before recovering from anæsthesia"; that is coupled with Certificate E E, and is called "Permitting experiments on dogs, cats, etc.," "Dispensing with the obligation to kill the animal before recovering from the anæsthesia."

10330. (*Chairman.*) It is not expressed there that Certificate B is to dispense with anæsthesia. It really seems to me a small point, I confess. You must have some more important things to come to?—Very well, my Lord; I pass from that then. I next come to the case of the "Chemist and Druggist," of December the 14th, 1901, in which there is an account of an address delivered by Professor Sherrington to a number of chemists and druggists, when the professor is reported to have called upon Dr. Grünbaum to demonstrate the action of the snake antitoxin upon some rabbits. The Report then goes on: "The first was inoculated with cobra venom, the second with antivenom, and the third with the cobra venom and antivenom. The first died in about 30 minutes, completely paralysed by the venom of the snake. The second, although it had also been subsequently inoculated with cobra venom, lived, and was full of life. The third, which had subsequently been inoculated with the anti-venom, also lived, and exhibited a considerable amount of liveliness."

10331. (*Colonel Lockwood.*) Where was this?—I think in Liverpool.

10332. (*Sir Mackenzie Chalmers.*) What was the date of it?—It was published in the "Chemist and Druggist" of the 14th December, 1901. The points that I raise here on this are these:—First of all, I say that Dr. Grünbaum here broke the law. He broke it first, according to this account, by demonstrating at a lecture without Certificate C; that is the first breach of the law that he made—he had no Certificate C, and he was called upon by Professor Sherrington to demonstrate the action of the snake antitoxin upon some rabbits, and he proceeded, according to the account, to do so. Secondly, he appears to me to have broken the law by performing an experiment merely to demonstrate what was already known with this Certificate A; he has Certificate A, but it was to demonstrate something that was already known.

10333. (*Chairman.*) It would not be against the law on his part if it was certified to be a proper case for experiment, would it?—Yes.

10334. With Certificate A?—Yes.

10335. Even although it was passed by the President of the College of Physicians or Surgeons?—Yes, Certificate A does not permit you to get outside the Act. If you are to demonstrate something that is already known you cannot do it. Section 3, Sub-section (1) of the Act provides that the experiments must be performed "With a view to the advancement by new discovery of physiological knowledge or knowledge which will be useful for saving or prolonging life or alleviating suffering."

10336-7. (*Sir Mackenzie Chalmers.*) Generally?—No, it is a question of commas; there are no commas at all. (*After a pause.*) I say "A new discovery." Then there ought to be a comma: "Of physiological knowledge or knowledge which will be useful to them for saving or prolonging life." It must all be a new discovery.

10338. (*Dr. Gaskell.*) No?—Certainly.

10339. (*Chairman.*) A new discovery of knowledge, do you mean?—Yes, it must either be a new discovery of physiological knowledge, or other kind of knowledge which will be useful to them for saving life, or alleviating suffering. It must be for a new discovery of one of those two forms of knowledge.

10340. Surely the terms are that the experiment must be performed "With a view to the advancement" (leaving out some words for the moment) "of knowledge"?—No; it is the new discovery that is the point of the whole section, surely.

10341. The advancement by a new discovery of physiological knowledge?—But the new discovery is the whole point, I think.

10342. Yes, but the new discovery is "The advancement of physiological knowledge, or of knowledge"?—Yes, I think you must take it so; because if you look over the page there is a certificate exempting the holder of it from this particular.

10343. But what I rather meant was. Supposing that a description of what is proposed to be done is sent in, and a licence is asked for, it would not be necessary in that case, if he was licensed generally; he could do the experiment under the general licence, if he had it under Section 1?—No, not without anæsthetics.

10344. No, with anæsthetics?—There was not any anæsthetic in this case; there is no suggestion of it.

10345. But Sub-section (1) is the sub-section which deals with the performance of an experiment which must be done under anæsthetics?—No.

10346. Then the other Sub-sections create an exception?—No, you begin at Section 3, Sub-section (1); you have not got to the question of anæsthetics at all then. It is the beginning of the Act; it begins at the very beginning; it says that you must not do these things except for the purpose of new discovery, that is by Sub-section (1)—nothing else. You must not go on doing the same thing over and over again. That is what it is for. It has nothing to do with anæsthetics at all. The question of anæsthetics comes over the page.

10347. (*Sir William Collins.*) Is your point that, apart from certificates altogether, this was a case in which either a licence could not have been granted for such a purpose, or if a licence had been granted he was transgressing it?—Yes; if a licence was ever held at the time I am prepared to show that Dr. Grünbaum broke the law in what he did.

10348. (*Chairman.*) Sub-section (4) is material, is it not, "Experiments may be performed not directly to the advancement by new discovery of physiological knowledge, or of knowledge which will be useful for saving or prolonging life or alleviating suffering, but for the purpose of testing a particular former discovery"?—Yes, that is so. He had not got that certificate either.

10349. (*Sir William Collins.*) Is that Certificate D?—Yes; he had not got that either. Therefore he was absolutely outside the law, and had no business to do it. He first broke the law, I say, by demonstrating at a lecture without Certificate C. This was really a lecture where there were a large number of persons present—chemists collected there to hear the lecture, and he was invited by Professor Sherrington to do this particular thing which he had no right whatever to do, and he performed the experiment merely to

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demonstrate what was already known with Certificate A; therefore he violated the section which I have just read, Section 3, Sub-section (1). And, further, I contend that it is highly probable that by performing an experiment before the general public he violated Section 6; because I see no reason why a collection of chemists should be regarded as anything else than the general public. Section 6 says: "Any exhibition to the general public, whether admitted on payment of money or gratuitously, of experiments on living animals calculated to give pain shall be illegal." I do not see why a collection of chemists who go to a place should be regarded as anything else but the public. Therefore I maintain that under several headings Professor Grunbaum was entirely outside the Act of Parliament. I drew the attention of the Home Office to these various performances of Dr. Grunbaum, and they replied that it would have attention—that is the first thing. I drew their attention to it on the 17th December, 1901; I waited till the 20th February, 1902, and had no reply. I asked them then if they would kindly give the matter their attention, and then they wrote back to say, "I am directed by the Secretary of State to inform you that he has caused careful inquiry to be made into the matter. The experiments were performed under a Certificate A, the authorities who granted the certificate being, no doubt, of opinion that such experiments tended to the advancement, by new discovery, of physiological knowledge, or of knowledge which would be useful for saving or prolonging life or alleviating suffering."

10350. (*Colonel Lockwood.*) That is Sub-section 4?—Yes, that is it, Sub-section 4 of the proviso. That is practically indicating that the authorities who granted the certificate granted him Certificate D. But he had not got that.

10351. (*Chairman.*) Would you read the words again. That is the same Sub-section (1), is it not?—No, I am on Sub-section (4), which would take him out of Sub-section (1).

10352. What was it you were reading?—I was reading from the letter of the Home Office to me.

10353. What does the letter of the Home Office say?—The letter of the Home Office says: "The experiments were performed under a Certificate A," not D, "the authorities who granted the certificate being, no doubt, of opinion that such experiments tended 'to the advancement, by new discovery, of physiological knowledge, or of knowledge which would be useful for saving or prolonging life or alleviating suffering.'"

10354. Those are the words of Sub-section (1)?—Sub-section 4.

10355. They are the exact words that I have in my hand of Sub-section (1)?—Your Lordship is quite right—Sub-section (1). "Dr. Grünbaum states that the report of the proceedings is somewhat misleading, in that the experiments were not performed until after Professor Sherrington had finished his address, and that they were not in illustration of a lecture. He has, however, conveyed an assurance to the Secretary of State that such experiments in similar circumstances will not be repeated." In my opinion, and I think in the opinion of any fair person, it is quite clear that the Home Office asked Dr. Grünbaum what his account of this was, and Dr. Grünbaum said that the newspaper was all wrong, that this perfectly impartial person I suppose had called upon Professor Grünbaum to demonstrate the action of snake poison, and he demonstrated it to nobody at all—they had all gone apparently. But it is quite clear that the reporter was there.

10356. (*Dr. Gaskell.*) May I ask, were these experiments done in a physiological laboratory?—No doubt it was in a registered place; I am not saying that it was not. I do not suggest that it was not.

10357. Was Dr. Grünbaum working at snake poison at the time?—I do not know; I suppose he was. That I know nothing about.

10358. After the lecture was over he went on with his experiment, and some of the chemists came and looked on?—You are putting that to me?

10359. That is the suggestion of Dr. Grünbaum, I mean?—No, I do not think that is quite so. "Dr. Grünbaum states that the report of the proceedings is somewhat misleading, in that the experiments were not performed until after Professor Sherrington had finished his address." The "Chemist and Druggist" does not say that. The "Chemist and Druggist" says that when Professor Sherrington had finished his address he called upon Dr. Grünbaum. Dr. Grünbaum states "that they were not in illustration of a lecture."

10360. That is all I meant to convey?—Dr. Grünbaum considered that he was going on with work which he was doing before, and the chemists and druggists saw it.

10361. (*Sir John McFadyean.*) Have we any information as to the precise object of Professor Sherrington's address?—It is headed "A Physiological Experiment in Relation to Pharmacology."

10362. (*Chairman.*) That would not be on snake remedies?—I am not a scientific man, my Lord. Would you like to look at at?

10363. (*Sir John McFadyean.*) Therefore, if Professor Sherrington was lecturing on an entirely different subject, Dr. Grünbaum's demonstration could hardly be part of the lecture. Would you mind letting me look at it?—Certainly. (*Handing a newspaper to Sir John McFadyean.*) You see the newspaper reporter asserts practically that Dr. Grünbaum demonstrated this to the chemists and druggists assembled, and to himself too. He describes what he saw. It took 30 minutes; the wretched animal died in 30 minutes completely paralysed by the venom of the snake. That is his description of what he saw. I maintain that he was one of the public, and Dr. Grünbaum broke Section 6 of the Act.

10364. (*Sir Mackenzie Chalmers.*) Just on that point do you say that if one single member of the public is present at an experiment, that breaks the Act?—Yes, one genuine member of the public.

10365. For instance, if you or I went in?—Certainly I should consider myself one of the public.

10366. If any one of the public went in?—I have been invited to go into laboratories, but I do not propose to assist physiologists to break Section 6.

10367. That is your view of the law?—Certainly. I do not set up to be a very great lawyer, but Section 6 seems to me to be quite clear. If I may go in, I presume that anybody may go in—a cabman off the next rank, or anybody.

10368. (*Chairman.*) You have given us the complaint made, and the answer of the Home Office?—Yes.

10369. And nothing more happened?—No. I think Dr. Grünbaum got his licence again.

10370. (*Sir John McFadyean.*) Professor Sherrington does appear to have referred to snake poison in his lecture?—Dr. Grünbaum still has a licence. That is the complaint I have to make upon that. I will just rehearse it again. I maintain that he broke the law by demonstrating at a lecture without Certificate C, by performing an experiment merely to demonstrate what was already known by Certificate A, and by performing an experiment before the general public in contravention of Section 6. I maintain that those breaches of the law took place, and that the Home Office associated itself with his defence. Now, the next case I want to draw attention to is that of Messrs. Dixon and Brodie, in the "Journal of Physiology," volume xxix., No. 2.

10371. (*Sir Mackenzie Chalmers.*) Will you give us the date?—The "Journal of Physiology" is very badly dated, but I give the page. It is page 144. I think at the very beginning they give you the date; but those volumes are a collection of papers. On page 144 these two vivisectors make the following statement: "In studying these reflexes we have found it of the utmost importance to avoid the use of chloroform or ether as the anæsthetic. The experiments must, therefore, either be performed upon unanæsthetised animals, upon animals anæsthetised with morphia, or upon decerebrate animals. Our experiments were usually conducted under one of the two latter conditions, but in a few instances were repeated upon animals lightly anæsthetised with chloroform." We say that this kind of light anæsthetisation—when we are told that to be of any use, if the animals have not morphia or are not decerebrated, they must be unanæsthetised—awakes in us the profoundest distrust.

10372. Could you let me look at that paper?—Certainly (*handing the volume to Sir Mackenzie Chalmers*).

10373. (*Chairman.*) Was complaint made of it to the

Home Office?—Yes, I made complaint. This was defended by the Home Office, so far as I can make out, on the bare assurance of the authors of the paper; they speak of "The authors of the paper."

10374. (*Sir Mackenzie Chalmers.*) The paper itself does not refer to any specific operation or any specific operation on any specific animal?—I daresay it does not. I am dealing with the particular passage which I have quoted.

10375. (*Chairman.*) On some animals lightly anæsthetised?—"Upon animals lightly anæsthetised with chloroform." It was a long time before I got an answer to that complaint either. I wrote on the 30th of March, 1903, to the Home Office, and I did not get a reply till the 9th of July; they then answered me, and this is what they say. "Adverting to your previous letter, I am to point out that on page 102 of the volume referred to it is explicitly stated that, except in the case of animals killed by pithing, all were anæsthetised. On page 144 it is said that in a few instances the animals were lightly anæsthetised with chloroform. The authors of the paper explain what is denoted by the term 'light anæsthesia,' viz.: that the animals were anæsthetic"—that is the word; "*i.e.*, they could not feel pain, but that the anæsthesia was not carried so far as to abolish all reflexes. The inquiry which has been made confirms that none of the experiments described in this paper were performed on unanæsthetised animals"—I did not say that they were—"Under these circumstances there is no reason for thinking that the provisions of the Act, viz., that the animals must, during the whole of the experiment, be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain, was not complied with." So that the Home Office defends an experiment upon an animal of which the authors say that these particular experiments must be performed either upon unanæsthetised animals, or upon animals anæsthetised with morphia, or upon decerebrate animals, and they allude to experiments not under the last two conditions, the experimenters themselves saying that they must be unanæsthetised to be of value. They cover up the unanæsthetisation, in my opinion, by speaking of them as "lightly anæsthetised with chloroform"; because if they were anæsthetised properly with chloroform, according to their own description, the experiments would be valueless.

10376. We have had doctors giving their account of what they understand by the expressions "lightly anæsthetised" and "completely anæsthetised"; but it is a medical question, is it not? Supposing that the Home Secretary, who is not himself a skilled physiologist, is informed that "lightly anæsthetised" is an expression which is consistent with the animal not feeling pain, would not the Act then have been complied with?—That entirely depends.

10377. I mean, as regards the charge against the Home Office. I am not considering whether Messrs. Dixon and Brodie's experiments were lawful?—The answer that I should make to that question is, that it depends upon where the Home Secretary goes for his information. If he goes to the Association for the Advancement of Medicine by Research, which is composed of vivisectors, I should say that the information is not sufficient.

10378. Then would you go so far as to say that the Home Secretary must go to somebody who holds anti-vivisection opinions?—Certainly not. Let him go to Mr. Sewell—to some person who is always operating on dogs for their benefit; let him go to some well-known thoroughly-skilled dog doctor.

10379. You exclude the whole medical profession from that—the ordinary M.D.'s?—So far as I know the ordinary M.D.'s, they are not vivisectors, therefore their opinion as to what is proper anæsthetising for a dog would not have the same weight with me, and ought not, I presume, to have the same weight with the Home Secretary, as the expression of such a man as Mr. Sewell.

10380. Assuming that the Home Secretary went to a person who was a skilled physiologist, and he gave him that explanation, you say that he ought to appeal to a veterinary surgeon?—I think so. I think he should go to an impartial person, and I do not think, looking at it from the point of view of the Home Office, that a licensed vivisector is an impartial person on this question. And on this occasion you must remember to whom the appeal was made—to the authors of the paper. What answer could the Home Secretary expect to get from the authors of the paper, whatever they said in the paper, except that the animal did not feel anything? He never could have expected the authors of the paper to write back and say "we tortured these animals."

10381. Supposing that the Home Secretary had advisers who advised him that "light anæsthesia" is an expression which may cover, in the minds of medical men, absence from pain—I do not know how it is—but supposing he had been so advised, would you say that he was to blame for saying that he found that the law had not been broken?—My answer would be this: that supposing in the former case he had consulted Sir Victor Horsley and Professor Schäfer, he would have got the answer which I have read, that of course, the experiment was completely painless. But if he had consulted Dr. Boyd, if he had consulted Mr. Jonathan Hutchinson, if he had consulted the great surgeons whom I consulted, he would have got the answer that I did, that such a thing was not anæsthetic, and that the corneal reflexes should be abolished before the operation is begun. My complaint against the Home Office is that they go to the vivisectors themselves for advice as to what is painful, and that they have done so always. I shall come on by and bye to the provisions of my Bill, of course. I now come to another case. In Volume XXVI. of the "Journal of Physiology." On page xxxviii. of the Proceedings of the Physiological Society of March 16th, 1901, you will find that Mr. J. Herbert Parsons, in a report of his experiments on dilatation of the pupil from cerebral stimulation, makes the following statement:—"The following are some of the results obtained: (1) Dilatation of the pupil, like other effects of cortical stimulation, is best obtained with slight anæsthesia."

10382. (*Sir Mackenzie Chalmers.*) Is that a general abstract statement, or is he describing certain specific experiments which he made?—At this distance of time I cannot say; you have it there.

10383. (*Chairman.*) What is your complaint about it; did you complain to the Home Office?—Yes, I complained that they allow this slight anæsthesia, and the Home Secretary referred me to page 368 of that volume, where I should find it stated that "the animals in question were completely anæsthetised and insensible to pain throughout the experiments." That, I presume, is a general statement with which vivisectors very frequently now begin a statement of all their experiments, and then when you come to detail you find these words, "slightly anæsthetised." They cannot both be true. Therefore all that the Home Office did when I pointed out that this man himself said that he did these experiments under slight anæsthesia was to point out that he himself, the same man, says somewhere else that the animals were completely anæsthetised.

10384. The whole question in this case, as in the last, seems to be whether slight anæsthesia means sufficient anæsthesia to prevent pain, or whether it means something less than that?—Yes.

10385. (*Colonel Lockwood.*) This same paper which you quote says, "The animals in question were completely anæsthetised and insensible to pain throughout the whole experiments"?—That is a general statement of the whole lot.

10386. (*Chairman.*) That is the statement, of course, of the physiologists there at work—I know you do not admit the statement—that though light anæsthesia it was complete anæsthesia in the sense of no pain being felt?—The word "ἀναίσθητος" means "without feeling, insensible." You must either be sensible or insensible. I decline to accept "slightly anæsthetised."

10387. We cannot press the derivation so far?—What does anæsthetised mean? It means without feeling. You cannot be slightly without feeling. You either feel pain or you do not.

10388. A drug which will produce anæsthesia means a drug which will produce that state of things, but whether a drug will produce that anæsthesia to a different extent, slight, incomplete, complete, and yet there may in each case be absence of pain on the part of the animal, I am not able to say. The term "light anæsthesia" seems to be used by physiologists, at any rate, as being an expression which does cover absence of pain?—If I may say so, I regard such expressions in

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the mouths of physiologists as expressions for the purpose of putting them inside the Act of Parliament.

10389. The Act of Parliament says nothing about slight anæsthesia at all?—It does not.

10390. It says that the animals must be under anæsthetics?—Yes.

10391. And there must be no pain?—Yes.

10392. The Act is complied with whether you put the word "slight" in or anything else if there is sufficient anæsthetic to prevent pain?—Yes.

10393. It comes back to what I said, whether in the mouth of those who used the term, assuming them to be speaking honestly, which you do not admit, the words "slight anæsthesia" cover absence of pain; they say that it does?—My point is that when they say that it does they say it to cover themselves from the Act of Parliament.

10394. The whole question is whether it is true?—We regard it with profound distrust, and we believe that if a properly authorised inspector were present he would see that the anæsthesia was complete.

10395. It would no doubt facilitate our inquiry if we were to adopt your view that we should believe nothing that is said by physiologists, and that those who speak on the other side are to be believed?—I do not take that line.

10396. That is rather what you suggest?—No, I say that when a person's own conduct with regard to the Act of Parliament is impugned, it does seem to me somewhat inadequate to go straight to him, the person impugned, and to take, without any question, his explanation of what he did.

10397. It was only with reference to an expression that you used just now that I said that. You assumed that it was said by physiologists in order to cover up a breach of the law, or you used some such expression as that?—A man is accused of a breach of this Act of Parliament.

10398. Not until after he has used the words?—After he has used the words he is accused of a breach of the Act of Parliament; he is asked by the Home Office to account for himself. What could he do to put himself inside the Act of Parliament but say that the words that he used, whatever they were, were sufficient for the Act of Parliament. You could get it down to an infinitesimal amount of anæsthesia, and you would find that they would maintain, if they were asked themselves, that it was sufficient.

10399. (*Sir Mackenzie Chalmers.*) These are the gentlemen who themselves saw the experiments, and conducted them; who else could say whether the animal suffered or not?—Quite so. There should be an inspector there. That is my answer.

10400. (*Dr. Gaskell.*) Have you ever seen such an experiment?—No; as I have said already, I do not propose to assist any vivisector to break Section 6 of the Act.

10401. (*Chairman.*) I see in Gould's Medical Dictionary "anæsthesia," the condition of total or partial insensibility, particularly to touch"?—What is the date of that book? It is since the Act, I should think.

10402. It is 1900?—Exactly, that is since the Act?—Who is the author of it?

10403. I suppose Gould's Dictionary of the day would be since 1876?—But the definition of anæsthesia is brought up to date to fit the Act.

10404. You said the word anæsthesia in its medical sense?—No; I said in its English sense.

10405. No, in its Greek sense?—Yes, its Greek sense.

10406. But in its medical sense apparently there are some who contend that it means something less than that?—Yes.

10407. The dictionary is, I see, by Dr. George M. Gould, and it is published in London by H. K. Lewis, 136, Gower Street, 1900.

10408. (*Sir William Collins.*) It includes partial insensibility within the meaning of anæsthesia?—That is very important. Then I may take it that light anæsthesia as taken from that book does mean sensitiveness to pain, if that definition is to be taken.

10409. (*Chairman.*) Not from the book which I quoted from just now. This is of course the same gentleman who wrote the account, who said that at the time of the operation the animals were completely insensible to pain?—Now we are getting off the dictionary on to the views of the person.

10410. Of course I merely quoted the dictionary, but it does not govern it at all; it was not very material, but it was apropos of what you were saying about the true translation of the Greek word.

(*Sir Mackenzie Chalmers.*) May I read the next definition of anæsthetic? It is rather curious: "Anæsthetic, without feeling, insensible to touch or pain; a substance that produces insensibility to touch or to pain; diminished muscular action, and other phenomena. Anæsthetics may be general, local, partial, and complete."

10411. (*Chairman.*) That is not so material. I thought perhaps the gentlemen when they were writing their account had present to their minds the definition?—My contention is very simple. My contention is that slight anæsthesia means a condition not insensible to pain.

10412. And you complain of the Home Office not having treated that as an offence after its being brought to their notice?—I say that throughout the Home Office have constituted themselves the defenders of the vivisectors from criticism, instead of being the impartial judges. I go on to the next charge now. I charge the Home Office officials with having appointed inspectors who have displayed such bias that they have thought it their duty not to make detective efforts to protect animals from illegal treatment. That, I think, is almost admitted, but as I am on it I should like to go on to show the bias of other persons besides Dr. Poore. That charge is based upon an answer of Sir James Russell's to Question 530: "When I took office he" (that is Dr. Poore) "wrote to me to tell me that I was to visit registered places about three times a year, but he expressly said that I was not expected to act as a detective," that is to say he was to have more consideration for the vivisectors than for the animals. That is what it amounts to in my opinion. He was further asked at Question 532: "Have you been instructed to make surprise visits," and his answer was "Never."

10413. Yes; I remember the evidence?—Dr. Busk, who was Inspector from 1876 to 1886, alluded to the agitation conducted by my society as "a senseless and mischievous agitation." That is not a wholly impartial description of us, I should think. Then Dr. Poore resigned, or, anyhow, left the Home Office. He ceased to be Inspector in 1899 some time after the correspondence which I had over Dr. Crile, and he signalised his departure from office by making an oration called the Harveian oration, delivered at the Royal College of Physicians in London by him, and it was reported in the "British Medical Journal" of October the 21st, 1899, which is in the room. In that he says: "So with regard to another burning question there be those who apparently hold the view that a guinea pig is of more value than many babies" (I do not know whether he is alluding to me; I never valued a guinea pig above a baby); "and that operations on the lower animals, no matter how carefully they be done, if done for the purpose of research, diagnosis, or relief, and not with the object of making mutton tender, pigs fat, or horses quiet, are to be denounced as atrocities. With such as these it is useless to argue. But seeing that many honoured members of our profession have themselves been vivisected by the envenomed tongues and sharp pens of a few noisy people, it may be well to point out that no conviction for cruelty or breach of law has ever been obtained." For a very excellent reason, that is that the Home Office themselves, who were responsible for Dr. Poore being Inspector, would have to advise the Home Secretary to give assent in writing to a prosecution. It is not very likely that with Dr. Poore there we should have got assent in writing, "and that hearsay and the misinterpretation of physiological writings is not evidence, at least on this side of the Channel."

10414. Is that so, that the Home Secretary cannot issue a prosecution without the consent in writing of Dr. Poore?—No; I say that the Home Office themselves are responsible for Dr. Poore being Inspector, and he would have to advise the Home Secretary to give assent in writing to a prosecution.

10415. What you said was that it is easily explicable, because the Home Secretary could not issue a prosecution?—I did not mean that. What I mean is that we cannot issue a prosecution without the written

consent of the Home Secretary, who would go for advice to Dr. Poore.

10416. You did put it so that the Home Secretary could not issue it without the consent in writing of Dr. Poore?—I did not mean to say that.

10417. You mean that the Home Secretary must issue a prosecution, and that he would not issue it without consulting Dr. Poore?—We cannot begin a prosecution without the written consent of the Home Secretary, and the Home Secretary would certainly not give us that consent without asking Dr. Poore, and that is the man who says that we value guinea pigs above babies, and so on. He went on to say: "The money collected by these strange apostles of mercy is not spent in the law courts, where initial cross-examination would be possible, but in attempts to institute a sort of medical excommunication, and in efforts to dam the stream of charity by sophistical utterances such as Tennyson pronounced to be ever the blackest of lies. Meanwhile it behoves the medical profession to bear a wary eye lest fanatics should seek to impose immoral tests upon applicants for hospital appointments, tests, be it remembered, which would have excluded Harvey from the service of St. Bartholomew's. No hospital governor who coquets with tests of this kind can possibly command the respect of our college, nor can we admit that such an attitude towards research can do anything but harm to the cause of the suffering poor. In this country we have always wisely recognised the uses of opposition. The stability of our political organisation has been largely due to the fact that the party in power is subjected to the ruthless criticism of the opposition, the toad ugly and venomous, which wears yet a precious jewel in its head.' So it has been in medicine, where scientific advance has been checked not only by the criticism of the learned, but occasionally by the clamour of the prejudiced and ignorant." With regard to that ebullition of Dr. Poore's, which he made immediately after leaving the Home Office as Inspector, the moment he was free of his trammels, it seems to me that a man who made that speech must have exercised the most strenuous efforts to preserve his unbiassed attitude of mind towards anybody who criticised vivisectors. The Royal Commission on Vivisection in 1876, on page 21, said: "The inspectors must be persons of such character and position as to command the confidence of the public no less than that of men of science." I say that Dr. Poore, who made such a speech as that, could not command the respect of the public. I admit that he did not make it until he left the Home Office, but it is obvious that that attitude of mind could not have arisen in a day, and it is probable that he indulged in that sort of expletives when he was inspector. I think, therefore that the Home Office ought not to have retained him, and that he was quite an improper person to deal fairly between the poor animals and the vivisectors.

(*After a short adjournment.*)

10418. (*Chairman.*) Will you proceed with your next statement?—I come now to the extreme difficulty of finding out how these certificates are signed, and how long they are effective. I find from information from the Home Office that so far as I can make out, Dr. Poore himself signed certificates exempting their holders from using anæsthetics, and was supposed to inspect the same persons whose certificates he had signed. I venture to think that if these certificates are to be signed at all, and given to these vivisectors exempting them from the use of anæsthetics, it is highly unfortunate that the person who signs them should be the inspector himself.

10419. (*Sir Mackenzie Chalmers.*) In what capacity did Dr. Poore sign them?—Dr. Poore signed certificates before he was made Inspector, but those very certificates remained in force for some years after he was Inspector, and were effective. I venture to say that it would have been more proper if those gentlemen had applied to somebody else to sign their certificates when Dr. Poore became Inspector.

10420. (*Chairman.*) They have to be signed every year, have they not?—That, my Lord, is a question which I have found it impossible to ascertain. The Home Secretary in his place in Parliament said on July 24th, 1899, in the House of Commons: "All certificates expire on the 31st of December of the year in which they are granted."

10421. The certificate—not the licence?—Yes. I was speaking of certificates. Under that you would imagine, of course, that in the year when Dr. Poore was appointed Inspector, at any rate at the conclusion of that year, any certificate signed by him would have expired, and that therefore in the ensuing years the gentlemen who held his certificates would have had to go elsewhere to get fresh ones, but I found that some years after that, it was the fact that certificates were still being used, and held by persons who still held certificates signed by Dr. Poore. I therefore wrote to the Home Office and asked them about it. I stated that in the Yearly Report for 1899 "Dr. Thomas Stevenson is stated to have received a crtificate signed by 'the Professor of Medical Jurisprudence University College London.' Both the Medical Directory and Whitaker's Almanack for the current year give the name of Dr. G. V. Poore as the holder of this Professorship."

10422. Dr. Poore was appointed Inspector in 1891?—Yes, he had been inspector ever since 1891, and here in 1899 was a certificate still held, signed by a person called the Professor of Medical Jurisprudence University College, London, when both the Medical Directory and Whitaker's Almanack for the current year, 1899, gave the name of Dr. G. V. Poore as the holder of that Professorship. I therefore drew the attention of the Home Office to it, and after some correspondence, I received the following reply: "I am directed by the Secretary of State to point out that his answer"—that is to the question in the House of Commons to which I have referred—"was that the Inspector whilst holding the office of Inspector under the Act, had not signed certificates. The certificate to which you refer was signed by Dr. Poore, before he held the office of Inspector." So that it amounts to this, that although we are told that all certificates expire on the 31st of December in each year in which they are granted, in 1899 there was a certificate signed by Dr. Poore, according to the Home Office, before he was Inspector, which must be before 1891, still in force.

10423. Running for eight years?—Apparently so, though the certificates are supposed to expire on the 31st December in each year.

10424. (*Sir Mackenzie Chalmers.*) Is that all the letter?—I was going on. "I am to explain that it is necessary for Dr. Stevenson to hold this certificate in order that he may carry out his professional and official duties in connection with the investigation of poisoning, and it is not limited in time, but is kept in force from year to year by the annual renewal of Dr. Stevenson's licence." So that apparently, although we are told that these certificates expire every year, we are told on the other hand that they are kept alive every year.

10425. Dr Stevenson is the Home Office analyst in poison cases?—I do not know who he is, I will accept your statement.

10426. (*Colonel Lockwood.*) He is the man who gives evidence on trials?—Very likely. Therefore we are told in the House of Commons by the Home Secretary that all certificates expire on the 31st December of the year in which they are granted, whereas in the letter to me I am told that they are kept alive from year to year.

10427. (*Chairman.*) Not "they"?—Kept in force from year to year.

10428. What I understand the Home Secretary to say is that there is an exception made in that case, and possibly in some others, where a person is holding a particular official position and is only doing official work. That is what I should gather from his answer. I should not gather that there is no rule at all that an ordinary certificate expires on the 31st of December?—You must make what deduction you like. I do not make any deduction except that it is a very confused statement. The two statements, one on the floor of the House of Commons, and the other from the Office at Whitehall seem to be contradictory.

10429. It certainly seems to that extent that it would have been more accurate according to his second answer, if the Home Secretary had said, "With the exception of certain officers all certificates expire on the 31st December."

10430. (*Sir Mackenzie Chalmers.*) You did not get a further question asked in the House to explain it?—I do not know at this moment. I may have or I may not. I will proceed with this letter—there is a little more said: "It is the practice of this Department to make licences annual, terminating on December the 31st in

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each year; and all certificates, whether exhausted or not, cease to operate on that day, because no person can perform a prohibited experiment without a licence. But on the renewal of his licence the holder of a certificate which is not exhausted nor disallowed may proceed with his investigation, as he is not required by the Act to obtain a fresh certificate every year." That, I maintain, is a flat contradiction—"he is not required by the Act to obtain any fresh certificate every year," and in the House of Commons—"All certificates expire on the 31st of December of the year in which they are granted." The two statements are wholly contradictory and irreconcilable. I admit that the fact that Dr. Poore's certificates were kept alive for years after he was Inspector is not exact evidence of his having displayed bias; it is more evidence of the carelessness of the Home Office in not seeing that a man did not hold the two offices and inspect the men who held his own certificates. Before I pass from that I will just rehearse my charge. My charge is that the Home Office have appointed inspectors who have displayed such bias that they have thought it their duty not to make detective efforts to protect animals from illegal treatment.

10431. (*Chairman.*) Then what is your next charge against the Home Office?—My next charge is that I charge the Home Office officials with having made entirely disingenuous statements in their official utterances, and with having constituted themselves the mere spokesmen of the vivisectors. On page 4 of the Parliamentary Report for the year 1904 the Inspector, speaking of one class of experiments says: "All performed under licence alone and under Certificate C, 1,102 in number, are unattended by pain." I think that is a most improper statement for a Parliamentary Paper, which goes broadcast all over the world, and is taken to be an assertion of fact on the authority of a great officer of the State. As he cannot pretend that he or his assistant was present at the vast majority of these experiments, this statement can be of no more weight than the uncorroborated assertions of the vivisectors themselves, upon which alone it must be founded, and I therefore assert that it would be more becoming for the Inspector to say that these 1,102 experiments were stated by those who performed them to be unattended by pain. That is a proper statement for him to make. If he was an unbiassed official, why should he adopt and endorse the statement of the vivisectors that all these experiments were unattended by pain? It is a remarkable commentary on the sweeping declaration of the entire painlessness of experiments done under licence alone, that on page 77 of the very same Report four vivisectors in Ireland appear to have admitted that pain accompanied their experiments so conducted.

10432. (*Sir Mackenzie Chalmers.*) Ireland, of course, is not under the Home Office?—No; but are the animals in Ireland more sensitive to pain, or is the Inspector more truthful? If you will pardon my asking the question, I did not mean to put it in the form of a question. That seems, at any rate, a question that should properly have been put to Mr. Thane when he was here. Further down, on page 4, the Inspector commits himself to the following assertion in respect to experiments done under Certificate B, allowing the animals to survive their operations. I have the quotations here if anyone likes to verify them: "Experiments involving the removal of important organs, and even parts of the brain, are performed without causing pain to the animals." If this refers only to the operative procedures under Certificate B it is disingenuous, because we admit, at any rate, that if the law is followed under the operative procedures the anæsthesia ought to be complete.

10433. (*Chairman.*) Is that the Report for 1904?—It is issued in 1905 for the year 1904. I do not know whether the Inspector expects people who have operations performed on themselves to believe that animals do not suffer as they themselves have suffered after recovering consciousness. I do not know whether that statement is deliberately framed so as to relate in fact only to the period of unconsciousness, while it is intended to convey to the reader the idea that the whole experiment is without pain.

10434. These are all cases of recovery afterwards?—Yes, this is Certificate B.

10435. Would you mind reading the Inspector's statement again?—"Experiments involving the removal of important organs, and even parts of the brain, are performed without causing pain to the animals."

10436. "And"—would you go on?—I have not got it.

10437. (*Sir Mackenzie Chalmers.*) "And after the section of a part of the nervous system these degenerative changes are painless"?—That may be so.

10438. (*Chairman.*) That completes the sentence?—Yes. He does not even commit himself to saying that that is always done. In either case, I characterise this as an *ex parte* defence of vivisection and the vivisectors, and I think it is altogether unbecoming in a Government Inspector. On page 5 the Inspector writes a paragraph on inoculation experiments done under Certificate A, the plain object of which is to suggest to the public that no acute suffering occurs in such experiments. This is what he says: "The substance administered may give rise to poisoning or set up a condition of disease, either of which may lead to a fatal termination. To administer to an animal such a poison as diphtheria toxin, for example, or to induce such a disease as tuberculosis, although it may not be accompanied by acute suffering, is held to be a proceeding 'calculated to give pain,' and therefore experiments of the kind referred to come within the scope of the Act 39 and 40 Vict., c. 77." I call that a disingenuous statement. The Inspector cannot be unaware of the fact that terrible suffering accompanies death by tetanus, induced by inoculation under Certificate A, that very many of the 8,292 inoculations done on behalf of the Cancer Research Fund (I will not use the word Imperial myself in connection with such a thing) entail considerable and prolonged suffering. The animals were given diseases, and they must have entailed considerable and prolonged suffering, and that many of the 29,705 inoculations inflicted lingering and painful disease on the animals used. He must be aware of that. If he really is unaware of these facts, he could have learnt them with a little investigation. If he is aware of them, the paragraph is clearly disingenuous.

10439. Just read the paragraph again—the part that you say is disingenuous?—"The substance administered may give rise to poisoning or set up a condition or disease, either of which may lead to a fatal termination. To adminster to an animal such a poison as diphtheria toxin, for example, or to induce such a disease as tuberculosis, although it may not be accompanied by acute suffering, is held to be a proceeding 'calculated to give pain,' and therefore experiments of the kind referred to come within the scope of the Act 39 and 40 Vict., c. 77." I call that a disingenuous statement; that is covering up from the public eye, in a public Report, the fact that a great deal of very severe suffering is caused under Certificate A.

10440. You mean that it suggests that nothing worse, at any rate, can happen in those cases?—Yes, for example, he carefully takes the diphtheria antitoxin and tuberculosis as two cases which are very likely not accompanied by severe suffering, but he must be perfectly well aware that experiments are conducted in tetanus and other things which do cause very grievous suffering, and he ought to say so. He has admitted it further on in the evidence, but this was written before a Royal Commission was going to sit. In either case, whether he is really unaware of the fact that serious suffering follows Certificate A in many cases, or whether the paragraph is disingenuous if he is aware of it—in either case I ask whether an individual who is either ignorant of even elementary knowledge of the conditions of many diseases, or who writes disingenuously in a Government Report, is a proper person to be Inspector under the Act. On page 6 the Inspector says that he and his colleagues found the vivisectors "desirous of acting in every way in accordance with the letter and spirit of the Act." What in the world did the man expect? He need not assure the public that the vivisectors do not break the law when he makes one of his rare visits to the laboratory. The anterior portions of the Report sufficiently indicated the Inspector's own appreciation of the letter and spirit of the Act. And it would indeed be remarkable if the vivisectors found any fault with it. At the head of Table IV. (I am now coming to what the Department says, not the Inspector), and repeated on several pages, the Department makes the following statement:—"N.B.—In experiments performed under licence alone, or under Certificate C, the animal suffers no pain, because it is kept under the influence of an anæsthetic from before

the beginning of the experiment until it is killed." Now, I say that the Home Office themselves who issued that statement can have no authority for it but the assertion of the vivisectors. They have admitted that their Inspectors are not present at any part of the vast majority of such vivisections, and if the intention of the Act was, and is, that the Home Office officials are to constitute themselves the endorsers of the uncorroborated statements of the vivisectors no Inspectors would be needed or provided by the Act. The Home Office officials themselves simply assume and assert publicly in this statement on no authority but that of the vivisectors themselves that they never infringe the provisions of the Act, and they thereby set themselves in open antagonism to the law, I maintain which, by providing inspection, informs the Home Secretary that he is not to be satisfied by the uncorroborated assertions of the vivisectors. I venture, therefore, to assert that this "N.B." which I have quoted is a biassed *ex parte* assertion entirely unbecoming in a Government Report.

10440A. What is the evidence that you suggest the Home Office ought to get? Of course, they ought to have the account for what it is worth of those who actually perform the experiments?—I say that the proper heading would have been not to assert that when those experiments were performed the animal suffered no pain, but that the vivisectors informed them that in those experiments the animals suffered no pain. Then the public could take that assertion for what it is worth.

10441. Apparently their adviser, Dr. Poore, or whoever it was at that time, advised them that it was so?—Dr. Poore was no more there than you or I.

10442. So far as his evidence goes, he is the Inspector?—Yes, he is the Inspector.

10443. And so far as the value of it goes it was in the same direction apparently?—Whether a man be an Inspector or a policeman, or anything else, he cannot know what happens in places where he is not.

10444. Neither can the Home Secretary?—That is my objection.

10445. He does get the evidence of the people who were on the spot?—He gets the evidence of the vivisectors themselves.

10446. They were the people on the spot?—I should not have the least objection to their putting a heading "N.B.—In experiments performed under licence alone or under Certificate C, if the law is properly observed, the animal suffers no pain," or "The vivisectors inform us that the animals suffered no pain," but they say "The animals suffered no pain." They were not there. They have no right to say that, and that goes to the public as an authoritative statement.

10447. (*Sir Mackenzie Chalmers.*) They see a certain number of experiments, and that is a sample, is it not?—They saw one entire experiment in one year, I think—one physiological experiment throughout.

10448. I think they saw a good many?—I have an answer on that very point in the House of Commons.

10449. I want that point cleared up?—It is a vanishing point; what they see is absolutely a vanishing point. I do not blame them for that; they cannot be there unless the law makes it necessary. I will come back to that. I know I have got it. It is something very small, you may take it from me, and I can prove it. I want to draw attention to Question 206 on page 10 of the Minutes. This case was put to Mr. Byrne:—"To give an example, the other day an experimenter wished to have an experiment without anæsthetics in this case. He wished to feed kittens on cow's milk instead of cat's, and a certificate was granted that the experiment might be performed without anæsthetics," and the answer was "Such an application would be granted by the Home Office." My comment upon that is, is feeding a cat with ordinary cow's milk an experiment calculated to give pain? I regard that simply, if I may say so, as flouting those who hold the views that I do. That question and answer seem to me to be a sort of ebullition of a spirit of raillery. No anti-vivisector that I ever heard of would suggest that giving a kitten cow's milk was an act calculated to cause it pain and the necessity of going to the Home Office to license such a thing. It is an attempt to pour ridicule upon the whole of those who with me wish to stop grievous pain to animals. You do not suppose that I, or any of those whom I represent, would support anything so absurd as that.

10450. On that point the experimenter would have been liable to prosecution if he had not a licence?—Why?

10451. Because it is an experiment, and it is an experiment which may cause disease in the animal?—How could the experiment of giving cow's milk to a kitten cause pain? There is nothing in the Act to prevent your trying which of two dogs will first find a bun under a sofa.

10452. It is a feeding experiment, and the licensee applied to the Home Office for a licence, because the result of changing the food in that way might have been to cause disease, and he wanted to find that out?—I pass from that.

10453. It was considered, and the licence was granted?—I do not deny it.

10454. (*Chairman.*) I do not say that it is likely to do it, and it sounds to me very unnecessary, but supposing that the kitten had died from the change of diet, I suppose a complaint might have been made?—My Lord, those whom I represent are not that sort of people. I protest against that being given as serious evidence—that is all. Then at Question 231 (this is very important, I think) Mr. Byrne was asked, "Has any application to be allowed to prosecute a licensed person ever been made and refused?" "No," says Mr. Byrne, "no application has ever been made and refused. An offer to give the Secretary of State's consent was once made." We all see what that is meant to convey, that we have no case, and cannot go to the Court. My answer is, of course, that we cannot make an application. How am I, or any one of the public, to obtain evidence of breaches of the law in cases which are not public? We can get no evidence of what goes on in laboratories except from the printed and published admissions of those who do the acts, and a man who has done an illegal thing is not likely to publish it abroad. They sometimes do, but if they do it is generally six months afterwards, so that I think this innuendo here, that we have made no complaints because there are no breaches of the law, is a very improper one on the part of the Home Office. I should like them to explain to us how we are to prosecute, however badly the Act is carried out! The next question that I want to refer to is Question 248, "Is there no red tape there?" and the answer is, "No, it is a necessity under the Act; the Secretary of State is bound to act in that way. It is curious that of the 60 contraventions, quite a substantial number were contraventions indicating a keen desire on the part of the operator to provide greater security for the animal not suffering than the Act allows him to take." The suggestion there of Mr. Byrne is that the Act of Parliament forces a sort of cruelty apparently upon the vivisectors; that if they were to follow the Act they would have to be cruel. "If he has a certificate, for instance, authorising him to carry out a certain investigation" (this is the instance he gives, you see) "without anæsthetics, and he, in an individual case, thinking the anæsthetic will do no harm, applies it to save the animal pain, then he has committed a breach of the Act. The Secretary of State cannot help that at present under the existing law." I venture to submit to Sir Mackenzie Chalmers and the Commission that this is not so. He could do the experiment under licence alone, which does not compel him to kill the animal unless it is in pain after the anæsthetic has passed off, or it has been seriously injured. And therefore the whole of that answer, I think, is most unhappy and unfortunate.

10455. (*Sir Mackenzie Chalmers.*) That refers to Certificate A, does it not?—Certificate A permits him to omit anæsthesia; it does not forbid him to operate under his licence alone.

10456. He wants the animal to recover?—So it may under his licence alone.

10457. No?—Yes, pardon me.

10458. Take the case of an inoculation experiment—that is what we are really thinking of?—That is quite right. What Mr. Byrne here says, is that if he has Certificate A he is precluded from using anæsthetics while he injects whatever it is that he injects. I say that is not so; he can inject whatever it is he injects under his licence alone; because it says that the animal must, during the whole of the experiment, be under the influence of some anæsthetic sufficiently powerful to prevent its feeling pain; and then it says that the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted upon the animal, be

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killed before it recovers. But in an inoculation of this kind neither has any serious injury been inflicted upon it nor is pain likely to continue after the effect of the anæsthetic has passed off. Therefore there is nothing whatever in the Act to prevent a man using his licence for an inoculation experiment.

10459. (*Chairman.*) Is not this rather on the question of the amendment of the Act, which I understand you are going to deal with afterwards. This is hardly a charge against the Home Office. There seems to be a point which you do not agree with as to the meaning of the Act?—The point I am taking is that in his evidence, Mr. Byrne would have the public believe that the Act of Parliament forces upon him a necessity for inflicting pain. That is what I object to.

10460. I do not see that?—He says, "It is curious that of the 60 contraventions quite a substantial number were contraventions indicating a keen desire on the part of the operator to provide greater security for the animal not suffering than the Act allows him to take." I say that is not so.

10461. Giving anæsthetics when the Act did not really compel him to do it?—No, giving anæsthetics when the Act says that he must not, is what Mr. Byrne says. I say that the Act does not say that he must not; I say that the Act does not put that compulsion upon him.

10462. I cannot see how that is a charge against the Home Office?——

10463. (*Dr. Gaskell.*) The contravention of the Act was, that they did not take out Certificate B as well; that is what the charge says?—I am speaking of the licence alone.

10464. (*Sir Mackenzie Chalmers.*) But you could not use the licence alone for inoculating an animal for the purpose of producing disease. If you aim at producing a disease, say cancer, under that you require Certificate A; the licence would not do it?—Why not?

10465. Because you are going to produce a serious lesion in the animal by producing cancer?—Not if the original operation is painless, a mere prick of a needle.

10466. You require Certificate A for that?—If you do not administer anæsthetics; but you may administer anæsthetics, surely.

10467. But you are bound to kill the animal before it recovers?—No.

10468. (*Chairman.*) Is not this a thing to be discussed, if discussed at all, on the question of amendment of the Act. If the Act is not clear about it you think it should be made clear. I do not see how you can found a charge against the Home Office for taking a particular view of the Act which, after all, is not an inhuman view?—My objection to it is that, as it seemed to me, Mr. Byrne's evidence was rather conceived in a spirit of raillery against us.

10469. No, I remember his giving evidence upon that point?—Then I pass from that. Now in Hansard of the 30th May, 1902, there is an important statement by Mr. Ritchie—it must have been part of a speech: "With regard to vivisection, time did not admit of fully discussing the subject. As to the argument that animals suffered intense pain after recovering from the anæsthetic, he could assure the House that that was very easily overstated, because life was destroyed before return of consciousness in all cases, except those where special certificates authorising the keeping alive of the animal were granted. It might become necessary to appoint additional inspectors, but it would be absolutely impossible to ensure that an inspector should be present at all operations, for that would require an army of inspectors. No certificate was given except under the greatest precautions, and even where such certificates were granted, the animals had to be killed immediately the object of the experiment was attained." I wish the Commissioners to fully appreciate those words: "Immediately the object of the experiment was attained." "Before the certificate was issued the Home Secretary had to be convinced that every precaution would be taken that the operation would be conducted by a properly-trained person for an adequate scientific object. All possible means were taken to avoid causing animals unnecessary pain, the inspectors were most humane men, and so, generally, were the operators. He did not know that there had been any convictions under the Act." I must take those words: "And so generally were the operators," to be an admission by the Home Secretary that not in all cases were the operators humane men.

10470. I should think that was rather a forced construction of a Minister speaking in the House. I dare say he put in the word "generally" thinking, "I am not going to commit myself to the statement that there is no such thing as a hard-hearted operator"?—I just draw the attention of the Commission to that, that is all. I pass from that. And as regards the words "The animals had to be killed immediately the object of the experiment were attained," I have had considerable correspondence with the Home Office about the proceedings of Professor Starling with the brown dog, over which I had to pay damages in the Law Courts. I may point out that whatever Professor Starling did to the dog was not within the purview of the Law Courts on that occasion. The action was brought by Mr. Bayliss solely, and everything that happened before the dog reached Mr. Bayliss's hands was outside the purview of that action. But I have to point out (and I have pointed it out to the Home Office) that I have asked the Home Office repeatedly to tell me under what licence and certificate the first two operations by Professor Starling upon the brown dog were done. I have been wholly unable to get an answer from the Home Office to that very plain, simple question. I wish the Commission fully to understand the point. My point is this. Professor Starling, if he had Certificate B, performed two operations upon that dog, one in December, a long interval of time between, and then another operation in February, to ascertain whether inflammation had been set up in the intestinal, or, at all events, the internal parts of that dog, by the first operation. If he did that second operation under his Certificate B, it was his duty, according to the Home Secretary's own words and according to the Act of Parliament, to kill that animal "immediately the object of the experiment was attained." When he had opened that dog a second time, he had by that opening fulfilled the object of his experiment, which was to ascertain, in his own words in the witness-box, whether the condition of inflammation had been set up by the previous operation. Instead of immediately killing that dog, he handed it over to Mr. Bayliss, and left the building himself, and Mr. Bayliss performed a further major operation upon that dog, which took an hour. My contention is (and I have placed it before the Home Office with all the seriousness that I can) that in that case it was Professor Starling's duty to kill that dog the moment he had performed his second operation upon it, and that this handing of the dog round from one vivisector to another is utterly illegal and ought certainly to have been stopped by the Home Office if they knew of it. If they did not know of it, the evidence in the action brought against me apprised them of it. From the moment that that evidence was before the public, they knew that Professor Starling instead of killing that dog, handed it over to Mr. Bayliss. My contention is that there never was a case more clear of a breach of the Act. And if they say that the second operation performed by Professor Starling was not done under Certificate B, but done under his licence alone, then it was also his duty immediately to kill it on recovery from the anæsthesia. He had no business to leave it to anybody else. So that whatever view they have of it, Professor Starling must in my opinion, have broken the law.

10471. You have read Professor Starling's account of it, have you not?—I heard his account given in his sworn evidence in the witness-box.

10472. And also his account before us?—Yes, I believe he was examined.

10473. (*Sir Mackenzie Chalmers.*) Do you accept his account?—I do not know at the moment that I remember it. I am going much more upon the evidence given in the Law Courts and the subsequent action of the Home Office. The Home Office, at any rate in the long interval between that action against me and this Commission, did not know what Professor Starling was going to say to the Commission, and they did know what he said in the Law Courts, and upon what he said in the Law Courts they ought to have acted, in my opinion.

10474. (*Chairman.*) I am not sure whether I remember rightly myself—I have not referred to the evidence, and it was some time ago—but was it not a fact that the dog was under anæsthesia at the time that it was handed over to Dr. Bayliss?—That was a matter of dispute. There was sworn evidence on both sides.

But I am not dealing with the question of anæsthesia; I am dealing with the question of Professor Starling's duty to destroy that dog.

10475. I am not myself arguing the question whether it was a breach of the law or not; I am only trying to get at what the explanation was; I am not saying what the worth of his explanation was, but I understood his explanation to be that the animal was under anæsthetics; and instead of that dog being killed under anæsthetics, and a new dog being obtained and put under anæsthetics and operated upon by Mr. Bayliss, he allowed this dog to be operated upon. It had to be destroyed, and he thought that as it had to be destroyed without coming out of the anæsthesia, it was saving, as it were, another dog for Dr. Bayliss to make his experiment on that same dog. Whether that was a breach of the Act I am not saying—I dare say it was, but that was the explanation that he gave.

10476. (*Colonel Lockwood.*) Did not Mr. Akers Douglas give me the same answer about that very question in the House of Commons?—Yes, I think he did. My point is that the animal under Certificate B had a right to be killed the moment Professor Starling had performed that second operation.

10477. (*Chairman.*) I quite see the point?—Professor Starling handed it over to somebody else; he did not even stay to see whether the animal remained under anæsthesia. It is his duty to see; it is not his business to hand it over to anybody else. To pass dogs round from hand to hand and nobody is responsible: to wash his hands of it and give it to somebody else is not, in my opinion, the way in which this Act should be carried out. I also wish to point out that if it had not been for this action the veil would not have been lifted in this case; we should not have known that the animal was handed from one vivisector to another, and passed round with wounds clamped up in its side with iron pincers. And this may happen constantly. The inspectors have told me that they knew nothing about the case until the action was brought. I have been told by the Home Office that they knew nothing about it. Now I come to the latter part of this charge, which is, that the Home Office have constituted themselves the mere spokesmen of the vivisectors. The position of mere spokesmen of the vivisectors has never been so openly exposed as in their own Reports. In the Report issued for 1879 when Mr. Busk was the Inspector, this is what he said: "The number of experiments performed, and according to the returns I have received from each licensee, was about 481." First of all he says "about," which is a very ingenuous remark to make; he does not know the number of experiments, and even the "about" is simply based upon the returns which he has received from each licensee, so that one licensee may have performed a thousand experiments, and there is nothing to prevent his reporting it as one, two, five or six. It is a very remarkable thing that year after year a large number of licensees take the trouble to take out licences and certificates, and, according to their own account, never use them. I do not assert anything, but it is a matter of comment. If a man takes out a licence to drive a cab, you generally assume that he drives a cab; and if a man takes out a licence or certificate to enable him to vivisect animals without anæsthetics, it is somewhat strange if he does not use them. And he takes considerable trouble to get them, if they are to be renewed every year, according to one statement of the Home Secretary. In the Report for 1880 I find these words—still Mr. Busk: "In none of the other experiments under these certificates, as I am assured by the experimenters, was any appreciable suffering inflicted." That is the statement put forward, and that is a perfectly right and proper statement to put forward. He says: "As I am informed by the experimenters." If the Home Office always confined themselves to saying in their Reports that they were only assured by the experimenters that this and that did not happen it would be more fair to the public. But still, as I have said, they remain simply here the spokesmen of the vivisectors—that is all. In 1883 he says: "I have been assured by all the experimenters, that in this class of cases no appreciable pain was manifested." There again he is nothing but the spokesman of the experimenters. Then in 1883, further on, he says: "In none of these experiments, as I am informed by the experimenters, was any appreciable pain inflicted." In 1884: "No appreciable suffering would be caused if the provisions of the Act were faithfully carried out, which I have every reason to believe was done." Well, down to the word "carried out" that is a very proper statement. "Which I have every reason to believe was done," is a statement that we will take for what it is worth. He was not there, and I do not see how he can say. He may have every reason to believe. I suppose that is a harmless statement; it does not carry much weight. This is the next Report, still 1884: "From the returns I have received from the various operators, it would appear that no pain was inflicted in these cases except in about 14 or 15 instances, in which disease was produced, but which was very trifling." There again he is merely their conduit pipe. Then the Report for 1885: "No appreciable suffering would be caused if the provisions of the Act are faithfully carried out, as there is not the least reason to doubt they were."

10478. You are not objecting to any of these statements that you have read?—I am only saying that throughout their Reports, and on all occasions, the Home Office constitute themselves the mere spokesmen of the vivisectors. They incorporate in their Reports just exactly what the vivisectors tell them. I say, and say it frankly, that in these Reports they say the vivisectors tell us they do not inflict any pain.

10479. That is rather a different thing from being mere spokesmen?—It comes to that. They do not criticise.

10480. When we speak of a man being the mere spokesman of somebody else, we mean that he is put forward to present that person's views in his interest. This is merely saying "I have merely to report information I have received, and this is the information I have received"?—I do not mean to convey any more than that, except that in their Reports, owing to their being wholly unable under the present Act to be present at vivisections that take place, they constitute themselves merely the spokesmen.

10481. We quite understand now what you mean by the word "spokesman." But we had better come to something about which you do complain of the Home Office?—I do not want to carry it any further than that. Now, in the "British Medical Journal" of the 18th June, 1898, Dr. Cecil Shaw, at page 1582, says: "My own work has consisted of two proceedings—first, the pathological examination of eyes removed on account of injury; and, secondly, an experimental research to determine what changes, if any, follow prolonged irritation of one eye, both with and without traumatism." He subsequently continues the narrative as follows: "In the experimental research my aim was to discover"—I am emphasising the "my," because he subsequently said that he never did any of these things himself; he says, "My own work," and he goes on subsequently "My aim was to discover whether prolonged irritation, external or internal, of a rabbit's eye would produce any visible organic changes in the ciliary region, either in the irritated eye or in the other. In the first experiment the eye was cocainized, and a wound was made in the ciliary region, without antiseptic precautions, and the cornea irritated with powdered jequirity seeds for varying periods up to six months. The jequirity causes purulent conjunctivis." (I do not understand all these words) "and abundant cocci were present in the pus, but though in some cases the ciliary region was again wounded while the eye was bathed in pus, no iritis was caused. In one case there was an extensive ulcer of the cornea, which ultimately healed without perforating. The non-irritated eyes were examined at intervals with the ophthalmoscope, but no departure from the normal appearance of the disc was seen in any case. Internal irritation was then tried, small shot being inserted into the eye, again without antiseptic precautions. In fact, soiled instruments were purposely used, to reproduce as far as possible the kind of wound which is most often found to cause sympathetic ophthalmia. When the incision was made in the sclerotic a few millimetres above the corneal margin, a little vitreous escaped on the shot being put in, but the wound healed rapidly, and in a few days no sign of it, or of external irritation, could be seen. When the wound was made in the ciliary region, more external irritation was seen. In one case the wound opened after several days, the shot was expelled, and all signs of irritation disappeared. In another, the shot was similarly expelled, but after some weeks, and irritation is still present at the end of five months, as shown by injection in the neighbourhood of the wound,

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but no signs of iritis are to be seen, either in the traumatised eye or in the other. This rabbit I have kept alive to observe how long the irritation lasts, and whether it will yet lead to any changes in the iris or ciliary body. After periods varying from three to four months, the rabbits were killed."

10482. Where was that performed?—In Dublin, I think.

10483. (*Dr. Wilson.*) In Belfast, I think.

10484. (*Chairman.*) In the United Kingdom, at any rate?—Yes.

10485. (*Sir Mackenzie Chalmers.*) That had nothing to do with the Home Office?—I have not said so yet.

10486. You were dealing, I thought, with your charges against the Home Office?—I identify ———

10487. The Irish Office with the Home Office?—Oh, yes.

10488. Not for all purposes, I hope?—I look at this from the point of view of the animals, not of the officials.

10489. I understand?—On inquiry I found that the alleged author of these experiments had never held a licence under the Act, much less the certificate which would have been necessary for such experiments as those above described. It looked, therefore, very much as if there had been a breach of the law. Special inquiries were made on the subject, and the society's solicitor went to Belfast to continue those inquiries on the spot. It was found impossible, however, to fix the exact date of the experiments, which was necessary as a preliminary to bringing the matter before any court. That shows the difficulty we have about bringing law actions. The inquiries were, nevertheless, persisted in for some time, but unfortunately the proceedings of vivisectors under the present law are carried on in such secrecy that it was found impossible to obtain the necessary evidence from any independent source. As a last but not very hopeful resource it was determined to lay the facts before the Irish Government. The correspondence which followed shows what happened.

10490. (*Chairman.*) When was this?—This was in 1898. I got a letter from J. B. Dougherty, dated from the Chief Secretary's Office, Dublin, and he encloses the correspondence, and here is the correspondence. It is a letter from Dr. Shaw to his very good friend Dr. Lorrain Smith, who had a licence, and a letter from Dr. Lorrain Smith to his very good friend Dr. Shaw, who had not. The letter from Dr. Lorrain Smith, who had a licence to his friend Dr. Shaw who had not, is as follows; both of them are dated the same day; they must have met in the street and exchanged them, I daresay: "Sir,—On receiving your note of yesterday's date, I referred the matter to Dr. Shaw, and I beg to enclose his reply. I regret that his paper is so worded as to be possibly misleading in regard to the vivisection experiments on inflammation of the eye which I allowed him to use. These experiments are performed by me" (this is the man with the licence) "in investigating the process of inflammation, and for them I hold the necessary certificate. I may further state that I reported the experiments in question to the Chief Secretary at the end of 1897 amongst other similar experiments on the process of inflammation. The experiments in which I as director of the laboratory aided Dr. Shaw were not on living animals." Now Dr. Shaw writes to his friend Dr. Lorrain Smith: "Dear Dr. Lorrain Smith,—I regret that any ambiguity in expression in my paper on sympathetic inflammation in the eye should have given rise to a misunderstanding. Your aid and advice were given in the whole course of the research, which included many bacteriological and other experiments, besides those involving vivisection. These latter, very few in number, were, I understand, part of the series of your own experiments on inflammation, of which you allowed me to make use. I had no responsibility beyond taking notes, and undertaking subsequent histological examination of the tissues.—Very sincerely yours, Cecil E. Shaw." So that Mr. Cecil Shaw, who in the papers said "my own work consisted of this, that, and the other," and "in the experimental research my aim was to discover"; and "this rabbit I have kept alive to observe," and so forth, when asked by the authority to explain how he did this says: "Oh, it was not I; it was my friend Mr. Lorrain Smith. I am sorry if there is any ambiguity of expression in my papers." I can see no ambiguity of expression. It is, "this rabbit I have kept alive," not "my friend Mr. Lorrain Smith."

10491. Is it now your suggestion that those two gentlemen were saying what is not true, and that Dr. Cecil Shaw did these experiments?—That is my suggestion. He said so himself in the paper which he published, and I prefer his statement written by himself before he was hauled over the coals to his refusal to say so afterwards.

10492. (*Mr. Ram.*) By the statement he made himself, do you mean what you have just read to us?—Yes, in his paper. "My own work consisted of two proceedings," and "in the experimental research my aim was to discover."

10493. I did not know whether there was anything else. I was not sure whether you alluded to that or any other paper?—No, I have nothing else to go upon but that paper.

10494. (*Chairman.*) Do you really think that that ambiguity —— ?—I see none.

10495. Well, what appears to you to be an incorrect statement but to other persons apparently an ambiguity?—When you say other persons, the person who called it ambiguous is Mr. Cecil Shaw himself.

10496. When I heard the explanation it is quite capable, to my mind, of being an ambiguous expression. If there had not been any other evidence I should have thought he was speaking of his own work. I should not have known that there was another operator; but I can quite understand if there was another operator how Dr. Shaw should have written that; and that being so, it seemed to me that we require rather more evidence, if we are asked to say that these two gentlemen have concocted a lie, than a mere suggestion that those words are a distinct and express admission by Dr. Cecil Shaw that it was his hand that operated, and not Dr. Lorrain Smith's?—Very well, then, I will supply you with that extra evidence that you want. New light was thrown upon what I consider the somewhat discreditable proceedings in the "British Medical Journal" of 22nd July last, on page 220, of which you will read in the report of the Scientific Grants Committee for the past year the following: "Shaw, C.E., M.A., M.D., 14, College Square, East, Belfast.—For a research on sympathetic ophthalmia, necessitating the purchase of monkeys and dogs and apparatus, £12." I call that corroborative evidence that he, Dr. Cecil Shaw, is the person who did those experiments, because he was given money to do them, and to purchase monkeys, dogs, and apparatus for the purpose.

10497. I understand your point about it?—And, further down—I have not quite finished yet—the editor of the "British Medical Journal" himself does not attempt to conceal the truth as I hold it about Dr. Cecil Shaw, because he says on page 224: "Dr. Cecil Shaw has, owing to circumstances, had to abandon for the present the research for which he received the grant." Putting all that evidence together, I maintain that that is sufficient evidence for reasonable people that Dr. Cecil Shaw did those experiments himself, and that when the Home Office, represented by the Irish Office over there ——

10498. (*Sir Mackenzie Chalmers.*) Oh, no, no, the Irish Office does not represent the Home Office?—Whatever it is—whatever is the Home Office over there in Ireland——

10499. (*Chairman.*) Supposing that Dr. Cecil Shaw had undertaken a research of this kind, and thought it was one that would yield useful results, supposing he had not got a licence, and got his friend, who had a licence and a laboratory, to do the manual work that was necessary, would not everything then be consistent? Would not this passage about the grant and everything else all be consistent?—No, I venture to think not. I think if a man gets a grant to do a thing himself——

10500. It does not say himself; it says his research. It does not say that his hand is to be the hand to operate upon those rabbits?—But he says himself, "My own work."

10501. We have gone through all that, and I have said what the effect is upon my mind?—I must beg leave to say that the effect on my mind is very different—that is all.

10502. So I understand.

10503. (*Mr. Ram.*) Your charge here is the illegality of any operation of this sort being performed by this particular man?—My charge is a little more than that. My charge is that when a person performs what is, in my opinion, a totally illegal act, and a knowlege of

that fact is brought by me to the officials who are his superior officers, instead of listening to me and saying to this man: "You have no business to do this," they always adopt his explanation, and throw it back at me, and say that is the explanation.

10504. (*Chairman.*) What they did was they accepted the statement of Dr. Cecil Shaw and Dr. Lorrain Smith?—Yes, at once. They had not at that moment, of course, before them the statement about the grant to Dr. Shaw himself for doing the very thing, because I had not got it to lay before them at that moment; otherwise it possibly might have led to a little difference. The answer of the Home Office was this: "With reference to previous correspondence relative to alleged violations of the Act 39 and 40 Victoria, chapter 77, by Dr. Cecil Shaw, of Belfast, I am directed by the Chief Secretary to transmit to you the accompanying copies of letters from Dr. Lorrain Smith and Dr. Shaw" (those are the two I have read). "I am to add that, having regard to the contents of these letters the Irish Government cannot carry the matter further." My complaint is that they immediately accept what I call the absurd explanation of these two vivisectors.

10505. (*Sir John McFadyean.*) Might we ask what it is suggested that the authority ought to have done under the circumstances?—They ought to have prosecuted Dr. Shaw for performing a very cruel experiment upon these rabbits without any licence at all.

10506. (*Chairman.*) The specific evidence against him would have been that statement, "My own work"?—They could get evidence that I, as an outsider, could not. They could have got the laboratory attendants and everybody in connection with that laboratory, and got to the bottom of it; instead of which they accept immediately, without any further talk, these two, as I call them, ridiculous letters of these two gentlemen.

10507. (*Dr. Gaskell.*) Is not the case of Ferrier and Yeo a very parallel case?—Yes.

10508. Was not that brought by you to the Law Courts?—It was before my time. I think it was brought by the Royal Society for the Prevention of Cruelty to Animals. I can find out all about it for you.

10509. Did not the Law Courts say that the evidence was distinctly that although Ferrier used the word "I," and spoke of them as his experiments, and got the money for the very purpose, the experiment was really done by Yeo?—I dare say.

10510. It was a parallel case?—I do not know.

10511. (*Mr. Ram.*) Was that before a jury?—I think it was before the magistrate; I do not think there was a jury upon it at all.

10512. (*Colonel Lockwood.*) Is that the end of your charges against the Home Office?—Oh, dear me, no.

10513. (*Sir Mackenzie Chalmers.*) We have been divagating to the Irish Office, and now we are coming back to the Home Office?—I did not really recognise much distinction. The general effect, I only wanted to say, of Dr. Cecil Shaw's case, in my mind, and I hope in the mind of yourself and others, is that—I do not say the Home Office in this case—the Irish Office did on that occasion certainly constitute themselves practically the representatives of the vivisectors—the spokesmen of the vivisectors.

10514. (*Chairman.*) Then what is your next charge?—I am here to charge the Home Office officials with having accepted the suggestion made to them by some nameless adviser, that to starve animals for days is not cruel, when they must perfectly well know that if any one of them starved their own horses (if they have any) the nearest policeman could successfully prosecute them for cruelty to animals under Martin's Act. That is the next charge I make. It is based upon this evidence. The "Journal of Physiology," volume xix., page 193, contains an account of great cruelty inflicted on animals by Dr. Noel Paton. On page 193 he tells us he starved a kitten for 56½ hours; on page 194 he tells us that he starved two pigeons for 96 hours; on page 199 we read: "Experiment 38. Three young rabbits of a brood, procured on June 7th, and starved to June 9th." I asked the Home Office whether the certificate under which these cruelties were perpetrated was Certificate A, and I asked what were the names of the professors who signed that certificate; and I further asked whether, in the opinion of the Home Secretary, starving animals for days together is correctly described as a "trivial operation," because Certificate A, we have been told, is only issued for trivial operations. The answer was that they had communicated with Dr. Noel Paton, and that they had ascertained from that gentleman that the experiments were not performed under any certificate specified in the Act.

10515. Did Dr. Noel Paton say what the object of the experiment was?—I do not know what his object was. I take no more interest in the object than the poor kittens did.

10516. (*Sir Mackenzie Chalmers.*) You got the information from the "Journal of Physiology"?—Yes.

10517. Did not it state any object?—I dare say it did. I take no interest in the object.

10518. (*Colonel Lockwood.*) "On the Relationship of the Liver to Fat," by Noel Paton?—Yes; I am not a doctor; I take no interest in the relation of the liver to fat; I do take an interest in pain inflicted on animals. It is perfectly indifferent to me what his object was.

10519. (*Chairman.*) You are aware that we are also discussing the question of breaches of the Act?—Yes, but the Act does not say what the intention was.

10520. But it says "With a view"?—Yes, that I do not know.

10521. (*Mr. Ram.*) You said that you asked the Home Office three questions. What was the answer to the first?—The answer was that the gentleman said that the experiments were not performed under any certificate.

10522. Then the second one?—The second one falls with it; it was to ask who the people were who signed the certificate. That falls with it. The third was whether starving animals was considered a trivial operation.

10523. Was there any answer to that?—The Home Office Secretary does not commit himself to that; he would not answer it. I will give you the whole letter: "Sir,—With reference to your letter of the 9th ultimo, I am directed by the Secretary of State to acquaint you that he has made inquiry as to the experiments to which you refer as having been performed by Dr. Noel Paton, an account of which is contained in the 'Journal of Physiology' of 1895-6. He has ascertained from that gentleman that they were not performed under any certificate specified in the Act 39 and 40 Vict., cap. 77." That is the whole letter. They will not tell me whether they consider it a painful operation or not.

10524. (*Sir Mackenzie Chalmers.*) If the experiments were cruel they would come under the general law of cruelty to animals?—No, not if they are experiments.

10525. Why not? Surely if a man has no licence it comes under the general law of cruelty to animals?—Yes, of course, if he is not licensed.

10526. Here he had no licence, apparently?—I did not say that; I said he had no certificate. I think he had a licence. I did not make that point. I assumed, of course, that he was a licensee, and I feel sure that he was. If he was a licensee and was doing painful experiments, I say he comes under the Act. I think he is a licensee. If he is not, I should have been sure to ask the Home Secretary how it was that he did these experiments without a licence.

10527. (*Mr. Ram.*) Unless he had a licence there was no good in going to the Home Secretary?—Exactly; I feel sure that he had. I feel sure that I should have asked that.

10528. (*Sir Mackenzie Chalmers.*) Dr. Gaskell thinks he had a licence?—I deduce it from my letter. The point is, here is a licensee who does a peculiarly, in my opinion, painful experiment. If he does it under his licence alone, he is bound to see that the animal during the whole of the experiment is under the influence of an anæsthetic sufficiently powerful to prevent its feeling pain. You cannot keep an animal for 56 hours, I presume, under chloroform?

10529. If he does it outside his licence altogether he comes under the general law of cruelty to animals, surely?—I hardly think so.

10530. He did not purport to do it under his licence at all?—Until he informs you that he considers the experiment outside his licence the natural assumption is that it is inside his licence. It is an experiment

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that comes under the Act, and the Act deals with experiments on animals. He does not make the defence that he is doing anything else; it is not like a carter driving a horse with collar sores; it is an experiment. If this does not come under the Act, nothing does. I next asked the Home Office the following questions: "(1) Were the kittens that he starved for 56½ hours, the two pigeons that he starved for 96 hours, and the three young rabbits starved from the 7th to the 9th of June kept under the influence of anæsthesia during those hours and days mentioned respectively, and if so, what was the anæsthetic employed? (2) If these animals were not so kept under the influence of anæsthesia, and, consequently, if the law was broken, was a prosecution instituted by the Home Secretary against Dr. Paton; and if not, what course, if any, was adopted by the Home Secretary to punish Dr. Paton for breaking the law, and to secure his future obedience to it? (3) Whether Dr. Paton holds a licence and certificates at the present time, and if so, who signed his certificates and his recommendation for a licence?" I did ask the question, you see, in that letter. (*After referring.*) Yes, here he is; he is a licensee, and he had a licence at that time. The answer was: "Sir,—With reference to your further letter of the 5th inst., regarding certain experiments described in Volume XIX. of the "Journal of Physiology," I am directed by the Secretary of State to say that he is informed that Dr. Noel Paton, after careful consideration, did not deem these experiments to be within the scope of the Act 39 and 40 Vict., c. 77. The Secretary of State is clearly of opinion that experiments involving starvation to an extent calculated to cause pain would be within the Act; but the inquiries he has made point to the conclusion that the extent and degree of pain which animals suffer from deprivation of food is a matter of considerable doubt. In any case, Sir Matthew Ridley does not propose to institute proceedings in respect of experiments which were conducted so long ago as 1895-6 and not brought to his notice at the time. Dr. Paton holds under the Act a licence and certificates, particulars of which will be found in the Annual Return of Experiments upon Living Animals for the year 1899." My reply was: "Sir,—With reference to your letter of the 30th of July, 1900, in which you inform me that in Dr. Noel Paton's opinion his experiments were not within the scope of the Act 38 and40 Vict., c. 77, what I desire respectfully to inquire is, not what Dr. Noel Paton's opinions may be of his own experiments, but whether, in the opinion of the Home Secretary, the starving of animals, described by Dr. Paton, were experiments upon living animals within the meaning of the Act." I cannot get an answer.

(*Mr. Ram.*) I thought he had answered that.

10531. (*Colonel Lockwood.*) He evaded it rather?—Yes, and he goes on evading it: "Sir,—With reference to your letter of the 27th ultimo, inquiring whether the Secretary of State considers the starving of animals described by Dr. Paton were 'experiments' within the meaning of the Act, 39 and 40 Vict., cap. 77, I am directed by Sir Matthew Ridley to inform you that, for the reasons given in the letter from this Department of the 30th ult., he decided not to cause proceedings to be taken against Dr. Paton. He therefore deems it unnecessary to express an opinion in the case of Dr. Paton whether or no his contention was correct, and whether or no what he did was an experiment upon living animals, such as to come within the provisions of the Vivisection Act."

10532. (*Mr. Ram.*) Would you read the first letter of the Secretary of State again in answer to your first letter on the subject?—"Sir,—With reference to your letter of the 9th ultimo, I am directed by the Secretary of State to acquaint you that he has made inquiry as to the experiments to which you refer as having been performed by Dr. Noel Paton, an account of which is contained in the 'Journal of Physiology' of 1895-6. He has ascertained from that gentleman that they were not performed under any certificate specified in the Act."

10533. Yes?—That is all, except "Your obedient servant, Henry Cunynghame."

10534. I thought there was some sentence that you read before that the Home Secretary was of opinion that these things did come within the Act?—No, that is the only letter. He rehearses what the other man said. Perhaps you are thinking of this: "I am directed by the Secretary of State to say that he is informed that Dr. Noel Paton, after careful consideration, did not deem these experiments to be within the scope of the Act." "The Secretary of State is clearly of opinion that experiments involving starvation to an extent calculated to cause pain would be within the Act."

10535. There is a distinct answer. I thought he had given one?—Yes, but he did not tell me whether these were such experiments.

10536. That is a direct answer to the question you asked?—No; I wanted to know what these experiments were. I do not want a general statement that there may be starvation, no doubt.

10537. Surely the general contains the particular?—No; he says: "Starvation to an extent calculated to cause pain would come under the Act." That is obvious. I did not want him to tell me that. I wanted to know whether he considered that Dr. Noel Paton's experiments were experiments painful under the Act; and he would not tell me.

10538. He could not tell you that unless he knew what the feelings of the animals were?—I think if you starved a horse for 96 hours he would soon know. I think under the ordinary law there are a very large number of convictions every year through the Royal Society for the Prevention of Cruelty to Animals for the very offence of starving animals. I also want to point out this—there is another point which arises out of this: "In any case Sir Matthew Ridley does not propose to institute proceedings in respect of experiments which were conducted so long ago as 1895-6, and not brought to his notice at the time." The "Journal of Physiology" is a publication that certainly must be within the purview of the Inspector of the Home Office. It is the journal of the vivisectors; and to say that a paper in it was not brought to the Home Secretary's notice at the time, one of two things must have happened; either Dr. Noel Paton did not fulfil the instructions that we are told are given to all vivisectors under the Act, to forward to the Home Office papers that they publish describing any experiments that they perform——.

10539. (*Sir John McFadyean.*) That they perform under the Act. His contention was that it was not under the Act?—But I think you will find that these experiments of Dr. Noel Paton are in a volume that must have gone to the Home Office—the "Journal of Physiology."

10540. (*Chairman.*) But if it was in 1895 how could the Home Secretary prosecute. It would be too late to prosecute summarily five years afterwards?—I am going to say that the "Journal of Physiology" ought to have reached him as soon as it was published.

10541. But taking his letter in 1900, he was quite right, was he not, in saying that it was too late to prosecute?—Very likely. What I am upon is this: It is almost the invariable rule that vivisectors, so far as I can see, never publish an account of their doings until a good six months after those doings have been completed, so as to escape any possible prosecution under the Act. The Act prevents any prosecution six months after the event, and we can only find out what happens in laboratories at secondhand, either from the publications of the vivisectionists, or from the Annual Report of the Home Office, which itself is generally only published three or four weeks before the expiration of the six months.

10542. I only asked the question, because from the Home Secretary's letter there might appear to be a discretion with him as to whether he should prosecute; but in fact the period for prosecution was up?—That is not the point. The point is that Dr. Noel Paton's assurance that he did it outside the Act is immediately accepted by the Home Office. It seems to me that a perfectly impartial person at the Home Office would have said, "These are painful experiments, they are done by a man outside the Act; he has done them outside the Act, and we will withdraw his licence from him or inflict some kind of censure upon him." Now I turn to Question 89 in the evidence of Mr. Byrne. I just want to show that this ought to have got to their notice—I think it ought, at any rate: "Now as to the records and reports of experiments which under statute a licensee is obliged to make, have you any observations to make as to the way in which that is complied with? Is that enforced by the Home Office?" and his answer is: "Decidedly it is enforced by the Home Office." Then the next question is: "Do you find that such records and reports are sent in regularly by the

licensees?" and then it goes on. I take all this evidence down to Question 94 practically to amount to an obligation placed upon vivisectors to send in any publications they make of their experiments. I think Dr. Noel Paton ought to have sent it in.

10543. (*Sir Mackenzie Chalmers.*) There is no obligation to send it in within a very short time. They ought to send it in soon?—I do not know what your regulations are, of course. You see, every day's delay puts a man further and further out of the possibility of prosecution under the Summary Jurisdiction Act.

10544. (*Mr. Ram.*) You are going to deal later on with the alteration of the present law which your Society purposes?—Yes.

10545. It may come up then, perhaps?—Yes.

10546. (*Chairman.*) Have you arrived now at any convenient stopping point in your evidence?—I will just finish this one charge, if I may?

10547. If you please?—Here is the answer to Question 95, which I think is important: "It is partly that and it is partly from the fact that if the supervision by the Home Office of an investigation going on in the country is to be anything like complete it will involve the examination of all the published investigations—investigations as to which anything is published," and so forth. That, I think, shows that this thing ought to have been sent in. And here, again, from Question 97 onwards: "The papers sent to the Home Office are usually not mere descriptions of experiments, but relations by a doctor of the steps of an important investigation which he is carrying out, or the progress he has hitherto made," and so forth.

10548. Does not this relate rather to the question of amending the Act, as Mr. Ram says, rather than any charge?—Perhaps. The effect of the evidence from Question 95 down to 102 comes to this. I think the evidence amounts to an admission on the part of Mr. Byrne that the Home Office have no means whatever of finding out if and what suffering is inflicted upon animals. That is what it comes to. The Home Office cannot judge. Look at Question No. 101: "Why not? (A.) Because the report (which I call a record and report) sent to the Home Office is a mere list of the experiments which he has performed under various headings" (that is the thing he makes his report upon). "It is in no way a description of the experiments." So that by the official things sent in to them they cannot judge what suffering is involved at all. And, broadly, my contention there is that the Act is so administered as to put the considerations of science above those of humanity; whereas the intention of the Act was to put humanity first.

10549. Does that conclude your specific charges against the Home Office?—No, that has got down as far as Charge 5, and there are seven more.

10550. Might I suggest that you might look over those seven and see whether you cannot shorten them, because there are some of these five that I think were matters too small to have much effect upon one's mind, and if you look through your seven other charges you will probably find that you can shorten it?—Very well; I do not want to be too long.

TWENTY-FOURTH DAY

Wednesday, 26th June 1907.

PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

The Hon. STEPHEN COLERIDGE recalled; and further Examined.

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10551. (*Chairman.*) You are going to continue your charges against the Home Office?—Yes, my Lord. I have taken the opportunity, in deference to your Lordship's request, and I have endeavoured to shorten the material which I have to present to you for the remaining charges as much as I can. I have maintained the general order, but I have reduced it a good deal, so that it will not take very long. Just before I begin on them I should like to allude to one or two things which occurred on the last occasion. Your Lordship asked me first of all where the quotation from Dr. Johnson came from. I have looked it up, and I find it comes from the "Idler," No. 17, and there is another piece of the sentence before it which I should like to read; I had forgotten it, but I think it is very much *ad rem*. He says: "The *Idlers* that sport only with inanimate nature may claim some indulgence; if they are useless, they are still innocent; but there are others, whom I know not how to mention without more emotion than my love of quiet willingly admits. Among the inferior professors of medical knowledge is a race of wretches whose lives are only varied by varieties of cruelty; whose favourite amusement is to nail dogs to tables and open them alive; to try how long life may be continued in various degrees of mutilation, or with the excision or laceration of the vital parts; to examine whether burning irons are felt more acutely by the bone or tendon; and whether the more lasting agonies are produced by poison forced into the mouth, or injected into the veins. It is not without reluctance that I offend the sensibility of the tender mind with images like these. If such cruelties were not practised it were to be desired that they should not be conceived; but, since they are published every day with ostentation, let me be allowed once to mention them, since I mention them with abhorrence. *Mead* has invidiously remarked of *Woodward* that he gathered shells and stones, and would pass for a philosopher. With pretensions much less reasonable the anatomical novice tears out the living bowels of an animal and styles himself physician, prepares himself by familiar cruelty for that profession which he is to exercise upon the tender and the helpless, upon feeble bodies and broken minds, and by which he has opportunities to extend his arts of torture, and continue those experiments upon infancy and age, which he has hitherto tried upon cats and dogs." Then it goes on with what I read before.

10552. That leaves no doubt about Dr. Johnson's views in his day?—Yes, I thought you would like it. He is a man of very high character. Of course, that has all to do with vivisection without anæsthetics. I wish to say now that I have brought here the letters from the eminent surgeons whom I quoted, and I find on looking at them that I quoted in full all the short ones, and that I did not quote altogether in full the longer ones; but I have them all here, and I will read them to the Commission if they like to hear them all.

10553. It seems a pity to read them all over again; if you will read the omitted passages that will be enough?—I will read those of which I only published part. What I was reading from, I may say, was printed matter. I was reading from a reprint of my letter to the Home Office, which I found only embodied part of them. I did not know whether it did or not until I got them.

10554. I observe it was indicated in the print that there were parts left out?—Yes; I gave the shorthand writer what I had been reading from, and he put in the dots, no doubt.

10555. Perhaps you had better leave it if you have not got it handy. You will be asked some questions about it afterwards in cross-examination?—Very well. Then there was a mention of the Ferrier-Yeo case, and its similarity to the Cecil Shaw case, which was mentioned by Dr. Gaskell. I have looked it up, and I find that the similarity is very slight. I accepted Dr. Gaskell's suggestion that it was a parallel case; I said "yes," but I find I was wrong. I have taken the opportunity, since the last meeting, to refresh my memory by reading a report of the trial of Professor Ferrier—it is reported in the "British Medical Journal" of 19th November, 1881, pages 836 to 842, which I have here if anybody likes to look at it. I find that the two cases rest on by no means the same footing.

10556. I do not think this is really material. It was an incidental observation really. Whether it is parallel or not is not very material?—Only the innuendo conveyed is of importance to me. I do not mean to use the word innuendo in any offensive way, but the suggestion conveyed was that inasmuch as the Ferrier-Yeo case was a hopeless failure, and ended in the acquittal of the defendants, therefore it was a parallel case, and therefore if an action had been brought against Mr. Shaw a similar result would have ensued—that was the suggestion made to me.

10557. It does not seem to me to be material in this case. Whether the Home Office ought to have prosecuted in Dr. Shaw's case or not must be really independent of what happened a good many years before in the case against another surgeon even if it were a

parallel case. One does not mean when saying that a case is parallel to say that it is a *facsimile*. The Home Office must form their own judgment on the Shaw case, and they do not defend themselves on the ground that it was a decision in another case which hindered them from taking action?—I do not know what they did. If Dr. Gaskell will say that that was not his suggestion——

10558. I do not think it was meant in that way in the least?—It is very short; I only just want to point out the difference between the two cases. I wish to correct my own statement. I accepted Dr. Gaskell's suggestion to me that it was a parallel case. I find that it was not. It was before my time.

10559. It was only a suggestion; it was not an assertion that he had examined the case and found it to be parallel.

(*Dr. Gaskell.*) Not in the least.

(*Witness.*) I like to be quite accurate. If I accept a suggestion and find I am wrong I like to correct it. The Ferrier-Yeo case was a private prosecution brought by my Society, but before my time, and showed very clearly the difficulties under which a prosecution by a private society labours. My answer to Sir John McFadyean was that the Government officials should have prosecuted. The other difference is, that in the case of Professor Ferrier the charge rested entirely upon a newspaper report, a portion of which was dictated from memory by Dr. Roy, who subsequently stated in the witness box that the report was inaccurate. Professor Shaw, on the other hand, was the author of the paper, in which he described his own work: "My own work has consisted of," he said. "In the experimental research my aim was to discover. . . This rabbit I have kept alive. . . ," and he never suggested that the "British Medical Journal" had inaccurately printed his own paper. Therefore on those grounds I say the two cases were quite dissimilar. There is another point that I could not find at the moment, viz., the question and answer in the House on the 26th June, 1903, which deals with the number of experiments actually witnessed by the Inspectors during the whole year. I have got it now, and should just like to read it:—"Dr. Shipman (Northampton): To ask the Secretary of State for the Home Department how many experiments on animals under licence alone, and under Certificate B respectively, have been witnessed by an Inspector at any time during the experiment, and during the whole duration of the experiment."

10560. (*Sir Mackenzie Chalmers.*) What year was that?—The 26th June, 1903. The answer, by Mr. Secretary Akers Douglas, was: "During 1902 eleven experiments under licence alone were witnessed by the Inspector, and one under Certificate B. In the case of one (under licence) the whole experiment was witnessed; and with regard to the others the Inspector usually remained a considerable time, and on some occasions made more than one visit during the progress of the experiment. In the case of the one under Certificate B the Inspector also visited the animal next day. Inasmuch as the Inspector habitually visits without notice he cannot be sure of arriving at the beginning of an experiment so as to be able to see the whole of it, nor even of finding any experiment in progress. It may be added that the Inspector at his visits always sees any animals on which experiments may have been made, and carefully notes their condition." I want to say as regards this Certificate B, as I am on it, that down from November, 1892, to October, 1899, the words of the certificate as issued by the Home Office were "such animals being during the whole of the experiments under the influence of some anæsthetic of sufficient power to prevent their feeling pain." It is "during the whole of the experiment" down to 1899. In October, 1899, the words were changed to these: "Such animals being, during the whole of the initial operation of such experiments, under the influence of some anæsthetic of sufficient power to prevent their feeling pain." I became aware of this very vital change in the wording of the certificate, which seemed to me, and still seems, to admit of a great deal more pain being inflicted upon animals than the certificate in its former form; and I, therefore, wrote and asked the Home Secretary this question: "Whether conditions are invariably added to this certificate" (that is, Certificate B) "enforcing the employment of anæsthetics ensuring insensibility to pain from the commencement of the first operation until the conclusion of all manipulations of the animal, and until the last wound is closed aseptically?" That was my question. The object of that question is obvious. There are experiments, as I presume you are all aware, in which the animal is opened and various operations of an initial kind are done to it, after which it may be kept for hours in a condition of disembowelment during the period when observation of its condition takes place. What I wanted to know was whether during that period of observation the animal was lying disembowelled without any anæsthetic, under this certificate; therefore I asked that question, because after all the manipulation of the animal, if it was kept under anæsthetics during all that period of observation and until the last wound was closed again aseptically, it seemed to me that the certificate did preclude a great deal of torture which it otherwise would not. The answer is most illuminating. The answer was "Whenever the experiment would involve a series of cutting operations (which is very infrequent), conditions are added to the licence enforcing the employment of anæsthetics ensuring insensibility to pain during all operative procedures." That is the answer; and that answer to that plain question leaves me under the conviction, which I am sure the Commissioners ought to share, that that certificate admits of a very large amount of extreme pain and awful suffering, because under that certificate the animal may be kept disembowelled alive without anæsthetics as long as actual cutting operations are not being performed upon it.

10561. (*Chairman.*) Do you propose to show us any case in which that has happened?—My lord, I am dealing with the law.

10562. I know?—I am not on laboratories.

10563. You are a little too combative, I think?—I do not wish to be so.

10564. I merely asked the question in order to ascertain exactly how you apply that proposition, because you spoke of keeping the animal under anæsthetics first, while it continued lying on the table with its bowels open, and then in another place you added the words "until the wound has been aseptically closed," which is a long time afterwards, and possibly includes a time in which the suffering would be comparatively small. Those are two very different propositions. I wanted to know whether you were going to illustrate it by any particular case, because I quite see the force of it if it is not too widely applied?—I have not got the volume here now I am afraid; I thought I brought it.

10565. I daresay you will come to some other point when you can give an illustration?—Then I pass away from that for the moment. I wish to say a word now on the feeding of kittens with cow's milk, which was brought up on the last occasion. I daresay you will remember that my objection to that question was that in my opinion it was conceived in a spirit of raillery against those whom I represent. I perfectly unexpectedly found myself face to face with the suggestion here that the object of that experiment was to create disease in the animal by giving it diseased milk. I am right, am I not?

10566. (*Sir Mackenzie Chalmers.*) No. The point is that the man who wanted to perform the experiment wrote up to know whether it was necessary to take out a licence or not, because the object of his experiment was to try the effect of substituting the milk of another animal for mother's milk. He did not know what the effect would be, whether the mere substitution of different milk for the mother's milk would produce disease or not?—Whether weaning a kitten would do so! All kittens are weaned, surely, from their mother's and given cow's milk.

10567. I think they are generally suckled by their mother?—But there comes a time when they are weaned, like children are given the bottle.

10568. This was at an early stage to see what the effect of substituting other milk for cat's milk would be after the very earliest stage, and he was advised, at any rate, that it might produce deleterious effects on the kitten, and it would be safer for him to have a licence, just the same as with those fish feeding experiments. As you know, a licence is required in cases where fish are kept in a pool where there is effluent sewage, where if the water becomes contaminated the fish get ill and die. A licence has been granted for that, as you know.

10569. (*Chairman.*) I did not consider that the question was asked in a spirit of raillery. The case had

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really come before the Home Office, on which they had to advise?—My point only is that as no suggestion of disease was made on both the former occasions when this cat was alluded to, I naturally was unaware that that was the object of his experiment. If the object of the experiment very likely included suffering to the kittens, it was, of course, a very proper case for having a certificate, and there is nothing more to be said about it.

10570. (*Sir Mackenzie Chalmers.*) There is just, perhaps, one point that I might clear up here. The witnesses for the anti-vivisection societies have taken up two lines. One line is this: that they object to experiments causing pain. Another line taken up has been that we have no right to exploit an animal, whether it causes pain or not, for the purpose of advancing human knowledge. Both those lines have been taken up before us?—I see. Of course, I cannot be responsible for other anti-vivisection societies.

10571. And this point, of course, was in my mind when I asked the question—that a great many experiments licensed by the Home Office, which appear as experiments, are absolutely painless in their results?—Then I pass from that. There was a question asked me by Sir William Collins: "Are the words of Certificate A mentioned in the question," referring to a question asked in the House of Commons—that is, whether Certificate B was mentioned as among the certificates dispensing with anæsthetics. I said "Yes, they were." I could not find it at the moment. I have got it now. Here it is: "Table 1. List of licensees who performed experiments under their licence upon animals. Experiments on cats and dogs without anæsthetics, E always coupled with A or B." I say that that is a general statement by the Home Office that Certificate B is for an experiment without anæsthetics.

10572. (*Sir William Collins.*) That is rather an answer to question 10328. Is it your suggestion that cases under Certificate B were classed among those dispensing with anæsthetics?—Yes, I have found it. They were classed under Experiments dispensing with anæsthetics up to 1897, at any rate. I have it here in the Table for 1897.

10573. (*Chairman.*) You have gone through the first five charges, and you said that there were seven, which I suggested might possibly be shortened?—Yes. I go on with my Charge VI. In this charge I charge the Home Office officials with having suppressed in the annual Parliamentary Return the names of those who take upon themselves the very grave responsibility of signing the certificates exempting licensees wholly or in part from the obligation to employ anæsthesia in their vivisections, although in the report of the former Royal Commission we find this clear direction: "We recommend that his (the Home Secretary's) advisers should be from time to time elected and nominated by himself; their names should be made known to the profession and the public." Now, my Lord, the Act of 1876 having provided that licences and certificates exempting licensees from the use of anæsthetics must be signed by persons holding certain official positions, if the Parliamentary Reports were drawn up in an open, straightforward manner they should give the names of these persons, so that anyone could ascertain who was responsible for exempting each individual vivisector from the obligation to use anæsthetics in his experiments, but it is quite impossible to discover from the report who assumes this very grave responsibility.

10574. What are the precise words of the Section as to what is to be reported?—Section 11: "Application for a licence under this Act."

10575. That names the persons who are entitled to give a certificate?—Yes.

10576. But is there anything about the reporting in the Statute; or is that only in the Order?—No, it is all "may," I think, all the way through; it is not "must."

10577. (*Sir Mackenzie Chalmers.*) "An application for a licence and certificate under this Act must be signed by one or more of the following persons"—that is to say, the President of the Royal College of Surgeons, and others?—Yes, but what his Lordship asked me was whether it must be put in the report.

10578. (*Chairman.*) You are complaining that in the report they do not report those facts?—Yes, that they do not give us the names of the persons who signed the certificates.

10579. Is that based upon what they ought to do for the convenience of the public, or upon what they ought to do if they are to obey the words of the Order?—It is what they ought to do if, as I say, there was no desire on the part of the Home Office to be partial or to screen the vivisectors from criticism.

10580. You do not put it upon any express words, but you put it upon the general duty which arises from the Order?—I also all through my evidence wish to rely very largely upon the recommendations made by the last Royal Commission, which sat before the passing of the present Act, and I say that that Report clearly contemplated the publication of the names of those who take upon themselves this grave responsibility. I pointed out this strange concealment of their names to several of those who may be assumed to be the persons concerned, and I asked them whether it was their desire that their individual names should thus be hidden under the cloak of an official title. Some of them, I think, have been manly enough to tell me that so far as they are concerned they have no objection whatever to the publication of their names; and others have told me that they consider it an insult to suggest that they wish to hide their names—which may or may not indicate that they would not object to their names being published. But whether any of them object or not, I am here to protest against the suppression of their names by the Home Office officials in a public document purporting to inform the public who sign these dreadful certificates. By the system of concealing their names under the description of their official positions, the Home Office officials themselves seem unable to know who the individuals really are who sign these certificates. In the Parliamentary report, dated the 8th June, 1899, I find Professor Schäfer's certificate (I take Professor Schäfer because he has been here), is stated to have been signed by the "Professor of Physiology, University College, London." That is the cloak under which the name is concealed. According to the Medical Directory for 1899, in that same year, the Professor of Physiology of University College was Professor Schäfer himself. The Home Secretary, therefore, was asked, by my request, in Parliament, "Whether the Home Office authorises or allows certificates to be signed by the operator himself on his own behalf?" and the reply was that such a thing "Would be directly contrary to the proviso in Section 11 of the Act." That answer is in Hansard of 24th June, 1899; I have it here if anybody wants to see it. I therefore wrote both to the Home Office and to Professor Schäfer himself to elucidate the truth, inquiring who the person was who is described in the Parliamentary report as "the Professor of Physiology, University College, London." The Home Office said (I have got the letter here), that this signatory was Sir John Burdon-Sanderson, who had resigned the Professorship of Physiology at University College, London, many years before. Professor Schäfer said—this is what he said to me—"I have not signed any certificates on my own behalf. The certificates I hold are signed by Lord Lister and Professor Halliburton only."

10581. (*Sir Mackenzie Chalmers.*) Is it licence or certificate?—"I have not signed any certificates on my own behalf" (he says. "The certificates I hold are signed by Lord Lister and Professor Halliburton only."

10582. Professor Burdon Sanderson, I imagine, signed the licence?—I do not know, I am talking about certificates now.

10583. (*Chairman.*) Was the answer of the Home Office an answer about certificates or a licence. You said that the Home Office referred to Sir John Burdon-Sanderson?—"Whether the Home Office authorises or allows certificates to be signed by the operator himself on his own behalf"—that was the question. I framed the question, so I know what it was.

10584. And the answer was what?—The answer was that it would be directly contrary to the Act if he did it. I then proceed, as I say, to ask Professor Schäfer himself, and he said that he did not do it. He said, "The certificates I hold are signed by Lord Lister and Professor Halliburton only," whereas the Home Office said it was signed by Sir John Burdon Sanderson.

10585. That is what I wanted to know. What does the Home Office say was signed by Sir John Burdon

Sanderson. What is the word they use in their letter?—We will get the letter.

10586. (*Sir Mackenzie Chalmers.*) May I read the letter, which your assistant has been kind enough to hand to me? "With reference to your communication of the 15th ultimo, enclosing a copy of Mr. Coleridge's letter of the 14th December last (which the Secretary of State regrets has not received a reply), I am instructed to say that in no case is an applicant for a licence under the Act 39 and 40 Vict., cap. 77, allowed to sign his own certificates, and Professor Schäfer has not done so. His licence was originally given on the recommendation of Dr. Burdon-Sanderson, by whom the certificate was signed, and who was at the time Professor of Physiology at University College, London, which post Professor Schäfer now holds. I am to add that the laboratories of the Mason University College, Birmingham, are not registered for the performance of experiments under the Act." That is neither here nor there?—I say that there is no inaccuracy in my statement which I make here. The Home Office said that Sir John Burdon-Sanderson signed the certificate.

10587. (*Chairman.*) Signed the licence?—Signed the licence and the certificate. I am not on the licence.

10588. I quite understand that; that is the reason I asked the question?——

10589. (*Dr. Gaskell.*) But not that certificate?—Yes.

10590. He signed the certificate at the time he signed the licence—that is years before?—But we have been already told that certificates expire every 12 months.

10591. (*Sir Mackenzie Chalmers.*) Perhaps it would be convenient once for all to state the practice. The practice is this: The licence has to be renewed every year, as you know. At the end of the year it comes up for consideration. It is originally signed by the people mentioned in the Act?—Is that so? They only recommend for a licence.

10592. Yes, they recommend for a licence. It does not go back to them. Every year the licence has to be renewed before the 28th of February. If the licence is not renewed the certificates fall with it. If the licence is renewed the certificates are also considered, and if they are not exhausted, that is to say, if the number of animals and experiments authorised by the certificate have not been exhausted, then it comes up for consideration whether that certificate should or should not be continued; it does not go back to the original people who gave the certificate, but comes up before the Home Office inspector at the Home Office for continuation. A licence, of course, may be renewed and the certificates not continued, or fresh certificates may be obtained?——

10593. The certificate necesarily falls if the licence falls; but they may or may not be continued if they are not exhausted?—But all that does not explain how it comes about that Professor Schäfer thinks the certificate was signed by one person, and the Home Office thinks that it was signed by another.

10594. (*Chairman.*) It is confusing, I agree, as appears from the different letters; but what I gather is that Sir John Burdon-Sanderson at the beginning recommended the licence and also recommended the certificate?—Signed the certificate.

10595. And also signed the certificate?—Yes.

10596. And that being started is referred to as being his signing the certificate in that letter?—Yes.

10597. But that subsequently the persons who are responsible for Professor Schafer's certificate at the time in question are Lord Lister and Professor Halliburton. That is what I should gather from it.

10598. (*Dr. Gaskell.*) Might I just ask what the date was when Sir John Burdon Sanderson signed that licence and certificate?—I have not got it. You must ask the Home Office. That is my point, that nobody can tell.

10599. All I meant was that if Sir John Burdon-Sanderson signed a certificate for a certain experiment it is very unlikely indeed that Professor Schäfer could be doing that same experiment many years after. He would probably have to get a new certificate?—The point, if I may say so, that I am making is my desire, and the public desire, the humane public, to know who signs certificates authorising vivisectors to do without anæsthetics, and when we come down to any particular case we find it almost impossible to find who that person is who takes that grave responsibility, and the confusion is accentuated by the Home Office saying that it is one person and the vivisector himself saying that it is another. That is my point. In this case there is no mistake about it. The Home Office said it was one person who vouched for Professor Schäfer's certificate in a particular year, and Professor Schäfer says it was two other gentlemen. This, therefore, I say, is a most informing example of the results arising from the use made by the Home Office officials in the Parliamentary Return of titular professorships as cloaks to hide the identity of the vivisectors who sign each other's certificates. Professor Schäfer's certificate is stated in the Report of 1899 to have been signed by "the Professor of Physiology, University College, London." The Medical Directory for that year told us that this person is Professor Schäfer himself. The Home Office assert that the person is Sir John Burdon-Sanderson, and Professor Schäfer says the person is not himself but Professor Halliburton. The task of reconciling these statements is quite beyond me. I say that the impossibility of tracing the identity of the persons responsible for signing certificates has been rendered absolute by the statement of the Home Secretary in the House, that all certificates expire on the 31st December. I understand now that they do not. My position about that is very clear. I say that here are these persons who sign these certificates. Many of them have told me themselves that they have no objection to their names being published. I have protested again and again to the Home Office against their names being concealed in this manner, and I protest again here against their names being thus concealed. If they have nothing to be ashamed of why are not their names published? It is a very grave responsibility that they take upon themselves in giving their brother vivisectors leave to vivisect without anæsthetics.

10600. (*Chairman.*) You attack these gentlemen very strongly?—I am not attacking these gentlemen; I am attacking the Home Office.

10601. You said, why do they wish their names to be kept out just now?—I do not say that they do. I did not mean that. I say, Why are their names kept out? Some, I say, have been manly enough to say that they do not wish to have their names kept back.

10602. I thought what you read certainly was a charge that these gentlemen themselves were extremely anxious to cloak up?—No, on the contrary, I have read what I wrote.

10603. I mean what you read just now?—I give in detail what they said to me in answer to my letter. Some said that they had no objection; others said it would be an insult to suggest that they wish to hide their names. Nevertheless, their names are hidden for them by the Home Office, and against that I protest. I pass on to the next charge. I am here to charge the Home Office officials with shielding the names of such licensees as they know to have broken the law, although the former report contains these words:—"Abuse of the power conferred by the licence must, of course, render the holder liable to its withdrawal, but this will involve great disgrace"—a phrase, in my opinion, which clearly indicates that the framers of that report contemplated the publication of offenders names, for no great disgrace can possibly appertain to a secret remonstrance or a secret withdrawal of a licence. Under Section 11 of the Act, 39 and 40 Vict. c. 77, a person who performs experiments calculated to give pain without a licence in contravention of the Act is "guilty of an offence against this Act," and shall be liable to a penalty not exceeding £50 for a first offence, and for a second to a penalty not exceeding £100, or to imprisonment not exceeding three months. Anybody who commits an offence against any other Act of Parliament which creates a criminal offence, and who confesses himself guilty of the offence, would in the ordinary course be brought before the Courts and subjected to sentence. The Home Office officials, however, have never instituted proceedings against any vivisector for committing admitted offences against the Act, not even in a case described by themselves as a "case of deliberate violation of the Act after warning."

10604. (*Colonel Lockwood.*) What was that case?—That you will find at Question 38. I do not know to this day what the case was; the case has never been revealed to us, not even before the Commissioners. Mr. Byrne said: "This power has been exercised on four occasions," and at the end of that answer you will see "He had been previously warned as to the necessity

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for certificates B and EE in such cases. It was considered by the Secretary of State to be a case of deliberate violation of the Act after warning, and his licence was revoked." That was all it amounted to.

10605. (*Sir Mackenzie Chalmers.*) Would you just give the facts first. A licensee who did not hold certificates B and E E performed the operation of gastric fistula on a cat under anæsthetics, the animal being allowed to recover. That was the case?—Yes, a breach of the Act—a clear breach of the law.

10606. And his licence was revoked?—Yes, his licence was revoked. But that fact—the fact of his licence being revoked, and the licensee's name were never mentioned even in the annual report. And, further, instead of prosecuting and punishing him, he was quickly reinstated as before, this very man, though we still do not know his name.

10607. (*Chairman.*) And you say that his name ought to have been mentioned in the report?—Certainly.

10608. And he ought to have been prosecuted?—Certainly.

10609. (*Colonel Lockwood.*) There is a footnote, you see, to that evidence?—He was not only not prosecuted, he was reinstated; after just a decent interval of a month or two he was put back where he was, as if nothing had happened. "Mr. Byrne subsequently wrote that a few months later the gentleman in question was allowed a fresh licence and Certificate A for inoculations, after special inquiry, on his representation that the revocation of this licence had stopped a very valuable investigation." We are not told what the "very valuable investigation" was. To this day the public cannot discover the name of that offender. Proceeding a little further, I tried to see if I could get it from the Home Office by writing to them. In the House of Commons, I may say—this is in Hansard of the 26th June, 1903, page 629—the Secretary of State said: "Wherever the conditions of the licence were violated the severest notice was taken of it, and, if necessary, the licence was revoked." I wrote and asked "what were the instances of violation of the conditions of the licence to which you alluded in your speech above quoted, of which the severest notice was taken?" I wanted to know who they were. The reply was: "The Secretary of State does not think that any useful purpose would be served by the publication of the names of these experimenters." I just wrote that letter to test it, to see whether they would continue to shelter the names of the peccant vivisectors, and they did.

10610. (*Sir Mackenzie Chalmers.*) There is one preliminary point that I should like to clear up. If you write to the Home Office it is only a matter of courtesy that they answer you?—Yes. I do not complain of any lack of courtesy on any occasion from the Home Office.

10611. The only place in which the Home Secretary is answerable is in Parliament?—Yes.

10612. As a matter of courtesy, they answer your letters, but if the Home Office undertook to give explanations to every one of the 35,000,000 people in England who write to them about any of their business it would be a little more than they could do?—But without undue egotism I may say that I do not regard myself as writing to the Home Office on any of these occasions in my own individual capacity. I have never written to them in any other capacity than as honorary secretary of a great and important society. I quite admit that in my own individual capacity I would have no claim.

10613. I do not know of any difference between a great and important society and any single individual who asks a question?—I only say for myself that I should not write except as the representative of what I consider more important persons than myself.

10614. (*Chairman.*) Does that conclude that charge?—Yes. I come now to my eighth charge. I am here to charge the Home Office officials with preparing for the Home Secretary evasive and insufficient replies in the House of Commons to plain questions on the administration of the Act, and with making evasive and insufficient replies themselves in official correspondence with my Society, and with leaving perfectly proper questions unanswered altogether. I accept Sir Mackenzie Chalmers' dictum that they are not obliged to answer any question at all, of course; but at the same time I think that on a question of the administration of the Act they would naturally answer reasonable questions, and I think they generally do.

10615. (*Sir Mackenzie Chalmers.*) You can take it for granted that they will always make inquiry. They would not necessarily tell you the result of that inquiry?—That is rather my complaint. Mr. Paulton, on the 24th July, 1899, asked "Whether Dr. Poore, while holding the office of Inspector, had signed certificates in his capacity of Professor of Medical Jurisprudence, University College, London"; the answer of Sir Matthew White-Ridley was "The answer is in the negative." I am assuming, of course, that these answers are prepared by the permanent officials. Colonel Lockwood asked: "Does Professor Poore sign the certificates?" to which Sir Matthew White-Ridley replied: "I am informed that it is not the case that he does so." Then Mr. Paulton asked: "Then how is it that the statement appears in the Report of the Inspector? Will the right honourable gentleman further inquire as to its accuracy?" and Sir Matthew White-Ridley replied: "Yes. I was only aware five minutes ago that the Report seemed to indicate this, and, of course, I have not had time to inquire." Now, the certificates signed by Dr. Poore were still in force while he was Inspector, and the official who prepared Sir Matthew White-Ridley's answer must have known it; they must have known that these certificates signed by Dr. Poore were in force at the time the question was asked.

10616. (*Chairman.*) Would you read the question again?—The question is "Whether Dr. Poore, while holding the office of Inspector, had signed certificates in his capacity of Professor of Medical Jurisprudence, University College, London"; and the answer is in the negative. I quite agree that it is perfectly accurate, but I venture to say that it is extremely evasive. The Home Office officials knew perfectly well that these certificates had been signed by Dr. Poore before he was Inspector, but in my opinion it was a strange thing that they should not have added to the answer "Yes, they were signed by Dr. Poore, but not while he held the office of Inspector."

10617. (*Sir John McFadyean.*) Might I ask why that question was not put in a form which would have made evasion impossible? Why was not the question, whether the certificate which was signed by Dr. Poore before he took office was still in force?—Because we did not know it. According to the documents before us, it appeared that he had signed certificates while he was Inspector, because we had at that time the statement of the Home Office that certificates expired on the 31st December. Until this explanation of Sir Mackenzie we had always believed that that was so. Therefore it seemed impossible that Dr. Poore had not signed them while he was Inspector, because there was his name on them, and apparently he was the person. It transpired that he had signed them many years before, and instead of expiring on the 31st December they have been kept alive nominally, Sir Mackenzie Chalmers now tells us.

10618. (*Chairman.*) You say that the replies of the Home Office are evasive. What had the Home Office to conceal in that case? What were they evading? It seems to have been perfectly regular?—No, they did not tell the public the truth.

10619. Evasive, you say, and, apparently, from the way you put it, wilfully evasive answers?—Yes, wilfully evasive.

10620. Therefore they were going to evade something?—Yes.

10621. What was it they were trying to evade, because if they had said all that you say they should have said, nothing could have been said against them?—Yes, I think the public would have been very much surprised and indignant at discovering that a vivisector held a certificate exempting him from the use of anæsthetics, signed by the very man who was to inspect him to see what was done.

10622. No, signed by a man before he was Inspector at all?—But still responsible. The certificate was alive.

10623. The certificate being there, it was alive by virtue of the Act of Parliament?—Yes, quite so, and in my opinion a very improper thing.

10624. You may amend the Act, but I cannot see what the Home Office have got to evade?—The Home Office have themselves said that the person who is Inspector ought not, under the Act, to sign certificates.

10625. And Dr. Poore did not do that?—But the certificate was kept in force after he was Inspector.

10626. That is a totally different thing?—No, I do not see it. I think he was a most improper signatory.

10627. When you say that you do not see that it is a different thing you startle me?—I do not. If it is kept alive every year, it is the same thing as his signing it every year again. That is my opinion, and I think it ought to be the opinion of everybody.

10628. (*Sir John McFadyean.*) The Government have not admitted your view of the question, have they?—What view?

10629. The view you have just been putting forward, that it is improper to allow a certificate to continue because it was signed by the Inspector before he took office?—The Government have said that it is not proper for a man to sign his own certificate.

10630. That is not the question. They have not admitted your view?—Oh, dear, no.

10631. Therefore it hardly seems fair to charge them with evasion; because the charge of evasion seems to imply that your view was necessarily the right one?—I say my view is the view that the general public would take; I am quite convinced of that. That is my charge, and I am quite convinced that the general public would take the view that it is very improper for the Inspector to sign the certificates of the very vivisectors he had to inspect. I am quite convinced of that, and I should have thought the Commission would have been so, too.

10632. (*Chairman.*) Your conviction does not necessarily govern ours?—No, my Lord. I invite you to share it; I can do no more.

10633. (*Sir Mackenzie Chalmers.*) Mr. Coleridge's point, as I understand it, is this: that when a gentleman who has signed certificates or signed recommendations for a licence becomes an Inspector, all licences and certificates signed by him ought, *ipso facto*, to expire?—You have incorporated the licences into it. I have not said anything about the licence. I am entirely upon the question of certificates exempting men from using anæsthetics. With that alteration I accept that statement as my view.

10634. (*Sir William Collins.*) Is your point that Dr. Poore, as a matter of fact, was inspecting work that was being done as the result of certificates given under his own hand?—That is my point, and I think that is very improper. Then, this is not a very important matter, but I should just like to allude to it. I called the attention of the Home Office to Clause 9 of the Act, which provides the machinery whereby results may be collected and subsequently incorporated in the yearly report; and I said that my committee hoped "that the public may, in the next report, be told what results in the way of benefit to mankind and the healing art have been achieved by all the torture that is continually being inflicted upon animals in the ever increasing laboratories of this country?" I asked that very plain question. We are always being told that such wonderful results are being achieved by vivisection, and I asked the Home Office whether, under Clause 9, they would not let the public into the secret and let us have some of the results published in the report.

10635. (*Chairman.*) When was that?—That I wrote on the 8th of December, 1898. The answer was that it would have attention, but nothing further ever happened; the results have never been published in any report. It is not a very important matter, but in passing I should just like to point it out. Now, on the 15th July, 1904, in Hansard I find Dr. Shipman asked the Secretary of State for the Home Department "whether he can state in how many cases the licensed vivisector himself has been the only authority present at the operation whose duty it is to see that anæsthetics of sufficient power to prevent the animal feeling pain are administered?" The answer by Mr. Secretary Akers Douglas was: "I have no information enabling me to answer this question." Surely the Home Office know at how many experiments the Inspector had been present, and consequently, at how many the vivisector himself was obviously, the only authority alluded to in this question; and therefore I think that that is beyond all question an inadequate answer. On the 22nd July, 1904, Dr. Shipman again asked the Secretary of State for the Home Department "How many dogs were vivisected in University College during the year 1903"—a very plain question. The answer is: "As I informed the honourable Member on the 4th of May last, I have no material from which I can answer this question." No material! Therefore if he does not know how many dogs are vivisected in a particular laboratory, he must know nothing at all about what goes on there. The first thing he must know, if he knows anything about it, is the number of dogs vivisected there. It is no doubt a perfectly true reply, but it seems to me that he was in a position to get, and ought to have got the information before replying. He could have found out by inquiring. I presume they keep records there—they ought to do so—of how many dogs they use.

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10636. (*Sir Mackenzie Chalmers.*) There would be no return under the licence, would there? As long as the animal is put under anæsthesia and killed before it recovers from the anæsthesia, it would come in under the licence. There is nothing in the licence to limit the number of animals that can be experimented on in that way?—That I do not know. You issue the licences. I have no idea what powers are given to the vivisectors.

10637. That is the Act itself?—I do not know.

10638. (*Mr. Tomkinson.*) Did that question apply to the number of dogs vivisected during that year?—During 1903, the year before. I asked for the year before. Now, on the 15th of July, 1904, Dr. Shipman asked the Secretary of State for the Home Department "How many experiments under the Act 39 and 40 Vict., cap. 77 have been performed at each place on the register during 1903, and whether he can ascertain how many separate experiments were performed on the same animal during the same period." Here, again, the answer is that there is no material upon which he can give any information. Of course, if he had answered this question he would have known about the brown dog having several wounds in it; but he did not. We know that he did not know anything about that brown dog. I maintain that he ought to have known. I think the Home Office ought to know what happens to every dog that is vivisected. If they have not the machinery at their own command to know, they ought long ago to have asked Parliament to give them that machinery in my opinion, if they care at all to protect the animals from unfair treatment. Then I asked this—and I must say I do think this is a case of evasion: On the 6th October, 1904, I wrote, "I desire very respectfully to inquire whether you yourself, or whether your predecessors in the office of Secretary of State have required from the licensees under the Act any guarantees of their humanity, and if so, what is and has been the nature of these guarantees?" The reply was: "I am directed by the Secretary of State to say that he has no doubt that you are acquainted with the duties imposed upon him" (that is, the Home Secretary) "by the Vivisection Act, and that he has every reason to believe that these duties are duly and faithfully performed by the Home Office." I think that letter is interesting, because it answers a great deal of what Sir Mackenzie Chalmers said about the devolution, and what I wish to say about the devolution of responsibility. Here, over the signature of Henry Cunynghame, we have a statement of the duties that are imposed upon the Home Secretary by the Vivisection Act, and that they are duly and faithfully performed, not by him, but by the Home Office. So that there in that letter the Home Office duly takes upon itself the responsibility for fulfilling the duties put upon the Home Secretary by this Act.

10639. (*Sir Mackenzie Chalmers.*) Does Mr. Coleridge expect the Home Secretary to perform the functions of Inspector?—Of course not. That is what I say. That is my contention. That the Home Office officials are the real persons against whom I bring my indictment. I call that a clear evasion. Because, of course, I know the duties imposed upon him, and I know perfectly well that the Act does not impose the duty in its provisions, but the whole tenour of the Act does, in my opinion. I then wrote: "A large discretion was left by that Act in the hands of the Home Secretary personally, a discretion which I take leave to believe was intended by Parliament so to be exercised as to provide guarantees of the personal humanity of each

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individual licensee. The question that I, therefore, desire most respectfully to put to you is: do you require of each vivisector a guarantee of his personal humanity before granting him a licence or passing his certificates? I put this question" (I say) "as representing a very large number of serious persons to whom your anwer is of grave importance in view of future legislation in the matter." I was then drafting my Bill. Again I received a second evasion. "I have laid before the Secretary of State your further letter of the 27th ultimo, and he directed me to inform you in reply . . . . that if any instance of inhumanity on the part of a licensee is brought to his notice, he will be prepared to act promptly in the matter." That is the answer which I say is an evasion. I replied: "I have received the letter of the 12th of November, written by your direction, informing me that if any instance of inhumanity on the part of a licensee is brought to your notice you will act promptly in the matter. In view, however, of the fact that you have yourself stated that during 1902 only one experiment upon a living animal out of a total of 14,906 was witnessed throughout by either of your Inspectors, the contingency of which you now speak may be regarded as remote. But I beg leave to point out that I did not ask whether you would act promptly in that remote contingency, and I fear I have failed to make my inquiry intelligible. I will, therefore, very respectfully repeat it as plainly as I can." I then repeat the question: "Do you require of each person licensed under the Act 39 and 40 Vict. c. 77 any guarantee of his personal humanity before granting him a licence or passing his certificates?"—a very plain question. And at last the Home Secretary, or rather the Home Office, takes refuge in silence. "I am directed by the Secretary of State to say that he has nothing to add to his previous letter." They will not answer the question—a masterly silence. I suggest that if he did require the guarantees of vivisectors of their humanity he would have said so, and certainly would not have evaded the question and refused to answer it. I therefore must come to the conclusion that he does not require such guarantees; but though he does not require them he does not like to admit it in a letter that may be published. Therefore I say that in my opinion the Home Office evince more solicitude for the vivisectors and for the Home Office than for the animals under their charge. I will not go into this story of the brown dog and Certificate B, except to say (I do not want to go into it all again) that in my opinion as the two operations performed by Dr. Starling upon that brown dog were admittedly done under Certificate B, and as the words of the certificate tell him that he is to destroy the dog immediately the object of the experiment has been obtained—and it was obtained by the second operation—I think he did an illegal act in handing it over to Mr. Bayliss. It was his business to see it killed, and in my opinion the Act gives the animal a vested right to be destroyed painlessly as soon as the object has been attained of the experiment under Certificate B.

10640. (*Chairman.*) Does that conclude that charge?—Yes. I pass to Charge IX., which is a very simple one; it is self-evident from the yearly Returns. I am here to charge the Home Office officials with putting forward annually a Parliamentary Return in which it is asserted, on the official authority of the Government Department, that not a single experiment in thousands inflicted on animals in Great Britain with its permission can be specified as entailing any pain at all, when all the while this plausible assertion is based upon no better evidence than the bare assertion of the vivisectors themselves, who are not expected to report themselves as cruel men.

10641. (*Sir Mackenzie Chalmers.*) The form of the Report has been altered since then?—Yes, down to a certain date the form of the return was "Return showing number of experiments performed on living animals during the year 1903 under licences granted under the Act 39 and 40 Vict. cap. 77, distinguishing painless from painful experiments." If this paper is supposed to distinguish between painful and painless experiments and you can find inside it no single experiment specified as painful, that is a statement to the public that there are none painful.

10642. When was that form abandoned?—Quite recently, I think.

10643. (*Mr. Tomkinson.*) That is an annual return?—Yes.

10644. It has not to be moved for?—I think somebody asks for it. Colonel Lockwood would know that.

10645. (*Colonel Lockwood.*) I think it is laid before the House in the ordinary course; nobody asks for it.

(*Chairman.*) It is laid on the table by the Department?

(*Sir Mackenzie Chalmers.*) Yes.

(*Witness.*) There generally is a question asked, because I suppose somebody wants to know.

(*Colonel Lockwood.*) Yes, somebody is keen about it.

(*Sir Mackenzie Chalmers.*) It is only a question about the date.

(*Witness.*) Charge X. is the next one. I charge the Home Office officials with placing a certain vivisector year after year beyond the reach of the safeguard erected by the Act to protect animals from illegal treatment by licensees, by giving him permission to vivisect in private places, thereby placing him beyond the possibility of legal inspection, because Clause 10 of the Act of 1876 gives the Inspector no right of entry into or inspection of any unregistered place. I am alluding here to Sir Victor Horsley, and there is no denying it. Mr. Byrne, in answer to Question No. 73, says "There were a few cases in which, on account of scientific eminence, or the high position of certain licensees, they were allowed to perform their experiments in unregistered places." High position! High position the former Royal Commission informed us was no guarantee of humanity. They said that persons in very high positions it could not be denied had shown gross inhumanity. Having discovered myself that Sir Victor Horsley could perform experiments where he liked, I wrote to the Home Secretary on the 25th of July, 1905, and I said: "On page 27 there is a note" (I was then alluding to the yearly report) "which runs thus: 'Sir Victor Horsley could also perform experiments at such places as might be necessary for the purpose of his experiments.' You (the Home Secretary) have, therefore, authorised this vivisector to perform his experiments anywhere he likes, and have thereby placed him beyond the possibility of legal inspection, for in Clause 10 of the Act the Inspectors' right to inspect at all is limited to registered places. May I ask whether you have not in this case deprived the animals vivisected by Sir V. Horsley of every safeguard provided by the Act, and whether by so doing you have not shown yourself unmindful of the sacred interests of those pitiful creatures committed to your charge by the law, who are the living victims of experiments? Whatever be your answer to these questions, I take leave to enter my solemn protest, in the name of many thousands of thoughtful and humane people, against this safeguarding of a vivisector from all inspection. The law constitutes you the protector of vivisected animals, and you have made yourself the guardian of the vivisector!" That protest I sent to the Home Office on the 24th of July, 1905, and no notice whatever was taken of it.

10646. (*Chairman.*) You sent it to the Home Secretary?—I sent it straight to the Home Secretary.

10647. That was Mr. Akers Douglas?—Yes, I think it was some new Home Secretary I addressed, because I hoped to get more from the new Home Secretary than the last. I expect it was soon after a reappointment. In spite of that protest of mine, which is very plain and clear, Sir Victor Horsley's permission to vivisect where he likes remained.

10648. Was it a specific permission given to him in writing, or was it a licence or certificate of any kind. How was it done?—I do not know. That is a question Sir Mackenzie must answer. It is simply a note at the foot of the return. He has had it for years I believe. I had not noticed it.

10649. I understand that it was probably endorsed on his licence?—I really do not know.

10650. (*Sir Mackenzie Chalmers.*) You do not know as a fact whether he made use of it or not. He asked for it for a special purpose?—I do not know. I am not attacking Sir Victor Horsley. I am attacking the Home Office for giving him permission. Whether he uses it or not is a matter of indifference to me on this charge. I am charging the Home Office with permitting this gentleman to vivisect outside the Act and beyond the reach of inspection.

10651. (*Sir John MacFadyean.*) Is it your charge that under no circumstances should it be permitted to

be done?—Certainly. No vivisector should be placed beyond the reach of inspection, because the whole Act of 1876 definitely in its provisions implies that every vivisector is to be inspected, otherwise it would not have provided inspectors. And if vivisectors are to be inspected, why is one to be given *carte blanche*.

10652. But might the vivisector not signify his entire readiness to be inspected in an unregistered place? Would that make any difference to your position?—It certainly would. You clearly understand that I am not making any attack upon Sir Victor Horsley.

10653. (*Chairman.*) Is there any special power in the Act given to the Home Office to permit this?—Yes, no doubt it is legal; it is all "may" in the Act. I do not think there is anything compulsory.

10654. For cases done on farms, for instance?—Yes, I am not suggesting that it is outside the Act at all. But what is illuminating is, that though this leave was given to Sir Victor Horsley for many years, and was maintained for many years, and was maintained in spite of my protest in July, 1905, I find that it was withdrawn on the 13th of February, 1906, just a month before the announcement of the Royal Commission in the newspapers, just at the time when we may assume that the Home Office were perfectly well aware that there was going to be a Royal Commission; and it is a very remarkable thing that contemporaneously with the knowledge in the Home Office that there would be a Royal Commission, this leave was withdrawn from Sir Victor Horsley to vivisect where he liked and beyond the reach of inspection.

10655. (*Sir John McFadyean.*) But have they withdrawn all similar leave to other vivisectors?—I should think probably not. There are some, I think, who have to do with inspecting grouse on a moor. I should have no objection to that.

10656. It was a question of the motive which inspired the Government—your reason for attributing this motive to the Government. If they had been in such a fright, would they not have withdrawn the whole of these permissions?—No, I should think not. If it be so—you are putting it into my mouth (I do not know whether they have or have not withdrawn them all)—if they have not withdrawn the others and have withdrawn the one to Sir Victor Horsley, the assumption is that the one to Sir Victor Horsley is less defensible than the others, in the public eye. That is the conclusion I should come to. That finishes charge X. Now I have dealt with Charge XI. under another heading, but I wish to enforce this charge: that although the officials of the Home Office must be aware that the Report of the Royal Commission in 1876 contained this pregnant warning on page xvii.: "It is not to be doubted that inhumanity may be found in persons of very high position as physiologists." They apparently have made no inquiries, public or private, into the vital question of the character for humanity of the licensees to whom they have delivered over the animals to be vivisected, and that although the Act of 1876 placed upon the Home Secretary the tremendous responsibility of personally protecting animals from inhumanity. I have already cited this correspondence as showing the evasive replies given by the Home Office to proper questions. I now cite it to show that we must conclude that as a fact no guarantees of humanity are required by the Home Office. I think it is quite clear that that is so, and in addition to the Home Office correspondence, which ends with a frank refusal to continue the correspondence without giving me that information, we have the following question put to Mr. Thane at Question No. 400: "Do you inquire at all into his"—that is, the experimenter's—"reputation for humanity or the reverse or anything of that kind?" To which he replied: "No, I do not inquire about humanity." On the point of the question of the humanity of vivisectors, I wish to draw the attention of the Commission to the evidence given before the former Royal Commission by Dr. Klein.

10657. (*Chairman.*) Have you finished your charges against the Home Office?—No, it is part of this charge—I cover it under this charge. There is one more charge after this, and only one more. It begins at Question 3538: "(Q.) What is your own practice with regard to the use of anæsthetics in experiments that are otherwise painful? (A.) Except for teaching purposes, for demonstration, I never use anæsthetics where it is not necessary for convenience. If I demonstrate, I use anæsthetics. If I do experiments for my inquiries in pathological research, except for convenience sake, as, for instance, on dogs and cats, I do not use them. On frogs and the lower animals I never use them. (Q.) When you say that you only use them for convenience sake, do you mean that you have no regard at all to the sufferings of the animals? (A.) No regard at all. (Q.) You are prepared to establish that as a principle which you approve? (A.) I think that with regard to an experimenter, a man who conducts special research, and performs an experiment, he has no time, so to speak, for thinking what will the animal feel or suffer. His only purpose is to perform the experiment, to learn as much from it as possible, and to do it as quickly as possible. (Q.) Then for your own purposes you disregard entirely the question of the suffering of the animal in performing a painful experiment? (A.) I do. (Q.) Why do you regard it then when it is for a demonstration? (A.) Because I know that there is a great deal of feeling against it in this country, and when it is not necessary, one should not perhaps act against the opinion or the belief of certain individuals of the auditorium. One must take regard of the feelings and opinions of those people before whom one does the experiment. (Q.) Then am I wrong in attributing to you that you separate yourself entirely from the feeling which you observe to prevail in this country in regard to humanity to animals? (A.) I separate myself as an investigator from myself as a teacher. (Q.) But in regard to your proceedings as an investigator, you are prepared to acknowledge that you hold as entirely indifferent the sufferings of the animal which is subjected to your investigation? (A.) Yes." Now, further down, at Question 3641, you find this: "(Q.) When you take hold of an animal for this purpose, what is done with it; do you bind it up while you are making this injection? (A.) If it is a large vigorous animal, as a dog, we do bind it and fasten it. A cat we generally must chloroform. (Q.) Why do you not chloroform a dog? (A.) We chloroform a cat because we are afraid of being scratched. (Q.) Why not a dog? (A.) If it is a small dog there is no fear of being bitten by the dog." That is the evidence which was laid before the former Royal Commission by Dr. Klein. It was laid before the Commission on a certain Thursday, the 28th of October, 1875. On October the 30th of the same year, three days later, Professor Huxley, who was a member of that Commission, wrote to the great Darwin, and this is what he wrote ——

10658. What is this on? Are we to have the Commission of 1876 brought up?—I am going to bring it right up to date. Dr. Klein has a licence at this moment.

10659. That may be; but we are on the question whether the law requires amending?—This is a charge against the Home Office of continuing a licence to a man whose inhumanity is patent.

10660. Then let us deal with that?—I want to show what Professor Huxley thought of this man.

10661. What Professor Huxley thought of Dr. Klein cannot matter?—But he was on the Commission.

10662. That may be. It is thirty years ago, and the law has been entirely altered since. The law now prevents these things. By all means state that Dr. Klein has a certificate still, and that you think that is an improper thing, as I understand; but we really cannot go through the whole of this?—It is not very long; it is a very short letter from Professor Huxley.

10663. The evidence runs very long. I think at any rate there is more recent matter and more material matter which takes up so much time that I think we can dispense with the history of what happened before this Act, which is, of course, the Act we are discussing?—My Lord, I maintain very respectfully that it is very much *ad rem* what a Commissioner thought of Dr. Klein at the last Commission, in consideration of the fact that Dr. Klein has for years had a licence and certificate from the Home Office, and has one now, on the point that they are perfectly regardless of the humanity of the vivisectors, and take no interest in it.

10664. You have read Dr. Klein's own statement. I object to time being taken up by having cited to us what this, that, or the other person, whether a member of the Commission or not, thought of what Dr. Klein said. We have Dr. Klein's evidence, and the only point that is material so far as I understand, is that you complain of Dr. Klein having a certificate?—Well, my Lord, I think this has been already dealt with before this Commission.

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10665. Yes, and that is another reason why we should not go into it; we have had this read to us before?—It is a very short letter, my Lord.

10666. (*Mr. Tomkinson.*) I suppose that Dr. Klein has not a certificate to experiment in his own laboratory, like Sir Victor Horsley?—No, I believe his certificate is for the Brown Institution. I think there has been a point made before this Commission that Dr. Klein altered his evidence.

10667. (*Chairman.*) That may be?—Then I think it is material, my Lord.

10668. But the alteration of the evidence was before the other Commission?—Yes, but you have had it dealt with here.

10669. You have cited what he said about his practice. If you wish to qualify that in any way or to increase the force of it by a statement which he himself made subsequently, by all means do it?—You see, my Lord, the point has been made here that Dr. Klein altered his evidence, and it is on that ground that I wish to lay this letter before the Commission, as showing what Professor Huxley, one of the Commissioners at the time, thought of that evidence.

10670. But that Commission is not this Commission, and there is no end to that sort of evidence of what other people thought about Dr. Klein. We have Dr. Klein's statement, which you say stamps him as being an inhuman vivisector, and you say that he ought not to have had a licence given him?—I say that he ought not to have one now.

10671. But when it comes to citing other people's opinions to prove that Dr. Klein was an inhuman person, there is no end to it?—Well, my Lord, I submit that I am entitled to read this letter. I request to be allowed to read it. If I am refused I pass on.

10672. I think we ought not to have it. It is admitting a great deal of matter which is not relevant?—Then I pass from it.

10673. You say that Dr. Klein has had a certificate which he ought not to have had?—He has it now so far as I know. He had it in the last yearly report, and he has had it ever since 1884, I think. Then I wish to show, if I may, by citing a short passage out of an article by C. S. Myers, who is a man of great distinction at Gonville and Caius College, Cambridge, a first class man in both parts of the Natural Science Tripos, and the author of various articles in scientific journals, that the opinion of Dr. Klein, in which he said that he had no regard for the feelings of animals, did not expire in the year 1876, but is held by persons of distinction to this day. I do not know whether I may read it for that purpose. It is an article from a man of great scientific eminence, who writes quite recently to say that he endorses Professor Klein's opinion.

10674. I cannot see how that is relevant?—Very well. This is only to bring it up to date, so that it shall not be said that it is thirty years old. It is an article up to date endorsing those views that the sufferings of animals are of no account.

10675. There are a number of people over Europe, as we know, who hold those views?—He is an Englishman and a Professor at Cambridge at the present day. I come to my last charge. I am here to charge the Home Office officials with having placed themselves in improper private confidential relations with a private society composed of supporters of vivisection entitled to no more consideration than the National Society composed of opponents of vivisection. Mr. Byrne was asked at Question 27: "The Association for the Advancement of Medicine by Research is a body consisting of a great number of eminent surgeons, I suppose?" and he replies, "Yes, I believe it includes, nominally, at any rate, a large number of gentlemen of great distinction in this matter of investigation"—I suppose by investigation he means vivisection—"but no doubt the Commission will ascertain its precise composition from the body itself." Then we have had the assurance on several occasions, and we had it from Mr. Akers Douglas in 1903 that: "No licence or certificate was granted except on the recommendation of an eminent medical man, such as the President of the College of Physicians or the President of the College of Surgeons, or without reference to the Society for the Propagation of Scientific Research"; that is what he called it, it is a better name for it, almost, I think, than its real one. Then we have Mr. Byrne, who tells us that it is a purely voluntary organisation. He is asked at Question No. 171: "It has no statutory recognition? (A.) That is so. (Q.) Can you tell us the name of its president or officers? (A.) Not at the present moment. (Q.) In every case, does the Home Office receive advice in regard to applicants for licences and certificates from that Association? (A.) Yes." So that it is a voluntary organisation of the promoters of vivisection, and it is consulted by the Home Office on the competence of vivisectors; and I do not know why, as we are also a voluntary organisation, we should not be consulted as to their humanity. But we are not. Desiring to find out, when I saw this evidence, what this Association really was, I could not find its address anywhere. I wrote in the air, and posted my letter "To the Secretary of the Association for the Advancement of Medicine by Research, London." The Post Office always find anybody, and they found him, I suppose. The letter I wrote was as follows: "Dear Sir,—In view of the enquiry now being made by the Royal Commission, I shall be much obliged if you will let me have a list of the members of your Association and the last yearly report, if you have one."

10676. (*Colonel Lockwood.*) When did you write that?—On the 24th of December, 1906, as soon as I saw this evidence. This evidence was given in October, and in December I wrote and asked for their last yearly report. I got a reply from the Association for the Advancement of Medicine by Research from 135, Harley Street, W., on the 29th of December, 1906, and it is in this form: "In reply to the Hon. Stephen Coleridge's letter of December 24th, the honorary secretary of the Association for the Advancement of Medicine by Research writes to say that he is unable to comply with the request contained in the letter." You see this gentleman did not even sign his name. I could not even tell who he was, but I looked up 135, Harley Street in the Directory, and it was the house, I found, of Mr. Charles Edward Beevor, who, I presume, therefore—I deduced—is the honorary secretary of the Association for the Advancement of Medicine by Research. But he gave me no information—he would not give me the yearly report or tell me who anybody was.

10677. (*Chairman.*) He is a doctor, I suppose?—I do not know. I suppose he is. I therefore prosecuted my further researches to find out what this Association was. I looked up old files of the "British Medical Journal," and I found in the number for the 1st of April, 1882, on page 476, that Sir William Jenner, at a meeting, concluded by proposing the following resolution: "That, with the view of bringing the legitimate influence of the medical profession more effectively to bear on the promotion of those exact researches in physiology, pathology, and therapeutics, which are essential to sound progress in the healing art, an association be formed, to be called 'The Association for the Advancement of Medicine by Research.'" Mr. Spottiswoode, then president of the Royal Society, moved also, at that same meeting apparently, "That the Association consist of representative members of the medical profession, and of other persons desirous of promoting the above objects." That is how it was formed. Then I find that on the 8th of April, 1882, a few days later, Mr. Hallowes, writing in the "British Medical Journal," on page 157, alludes to "The formation of a society to resist the opponents of scientific research," which, he says, "will be most welcome to the members of the British Medical Association." Therefore, he calls it a Society to Resist the Opponents of Scientific Research—that means us, I suppose, which is a misnomer, because we do not object to scientific research at all. Then in the "British Medical Journal" of the 22nd of April, on page 599, which is a fortnight later (I am doing this seriatim), I find a letter from the treasurer, who happened to be a Mr. Samuel Wilkes.

10678. What is this on?—I am showing you what the Association is, to which every application for a licence is referred.

10679. Could you not come to something which does show us what the Association is? All this history has very little to do with what the Society is now. This is the same Society that advises the Home Office?—Yes, this is the Society.

10680. But all this history of how you came to know what it is is immaterial?—I am only telling you what it is.

10681. I think you are telling us a great deal more than that?—This is a letter from a member of the provisional committee of it when it was first started, which contains these words, "The working physiologists"—that, of course, means the vivisectors—"of the three kingdoms have expressly stated that they do not desire (at least for the present) to attempt to abolish the Act, of which we are all ashamed, but to secure its being harmlessly administered. To speak with authority to public opinion, and to bring effectual pressure upon officials, needs other means than those which are suited to the arena of controversy."

10682. This is from a gentleman who is a member of the Association?—He is a member of the committee.

10683. In a letter written to the papers?—Yes; and he says that the object of this Association is to bring effectual pressure to bear upon officials—meaning the Home Office. Then the first report of the Council, dated the 29th of April, 1882, says that the first duty of the Association is "to promote those exact researches in physiology, pathology, and therapeutics which are essential to sound progress in the art of healing, and to remove any hindrances which obstruct these researches." Sub-committees were appointed to report (1) on the present hindrances to research due to the working of the Vivisection Act; (2) to report on the best ways in which exact researches in medical science (i.e., vivisection) can be encouraged"; and on the 13th of May we find a £500 prize offered for an essay on "The usefulness of vivisection as a method of scientific research, the conditions to be approved by the Association."

10684. Is not this rather beating the air? We all know that this Association is one that was formed for the advancement of research by vivisection, amongst other methods, and is very strongly in favour of it?—Yes.

10685. There is no doubt about that?—But I do not think that the Commissioners knew before that one of its objects was "to bring effectual pressure to bear upon officials."

10686. No, I am not aware of that. Is that a resolution?—That is what one of the provisional committee writes to the public press to say is one of their objects—"to bring effectual pressure to bear upon officials."

10687. In a large society, what one person who is a member of it writes to the press is not its best interpreter?—No, but further on you will see in the evidence that has been given before you that any licensed vivisector, on payment of 10s., may become a member of it, so that I think I am right in saying that this Association is an Association formed for bringing pressure to bear upon the officials of the Home Office, in order, if possible, to have the Act "harmlessly administered."

10688. Is there a resolution of the Society, or is there any printed rule of the Society saying that that is one of their objects?—My Lord, I have written to them in vain to get it, and I am thrown back upon this.

10689. I do not say that it is your fault that you cannot produce that rule if it exists, but certainly if that is the suggestion we shall have somebody from the Society (we have had one witness), and we will ask the question from him to ascertain it. But I think it is really taking up time unnecesarily to try and convince us that these gentlemen are all strongly in favour of vivisection—quite as strongly as you are against it?—Very well, if that be accepted, and that their object is to bring pressure to bear upon the Home Office——

10690. I do not accept that. I understand that somebody has written to the Press to say that that is his view.

(*Sir Mackenzie Chalmers.*) The pressure has not yet been felt.

(*Witness.*) Pardon me, I cannot accept that, if I may say so. I wish to point out in answer to that *obiter dictum*, that it is a fact proved before the Commission that the applications for licences are laid before this Association for the promotion of vivisection before they come to the Inspectors. Therefore the Inspectors receive these applications under prejudice, as they have already been before this Association of Vivisectors, before they reach the Inspector, who has to report upon them, and therefore it is put upon the Inspector.

10691. (*Chairman.*) I believe that is so; there is no dispute about that, is there?—Very well, my Lord, I think that answers Sir Mackenzie when he says that no pressure is brought to bear. I call that most distinctly pressure brought to bear upon the Inspector. Having finished my charges, I wish just to say in conclusion that I hope I have shown the Commission that the Home Office have repudiated the duty of protecting animals from unjustifiable suffering; that they have constituted themselves the consistent injudicial defenders of the vivisectors, protecting them from the punishment which it is their duty themselves to see inflicted upon them; that they have appointed biassed Inspectors; that they have given licences to men who have shamelessly and publicly proclaimed their personal inhumanity! that they have improperly allowed themselves, in our opinion, to be guided for years by a private society of vivisectors, formed for the very purpose of bringing pressure to bear upon them, if I may say so, to neglect that which they should do for the protection of animals; and I invite this Commission to acknowledge that I have laid before them ample evidence to show that the Home Office officials have for 30 years betrayed the solemn trust placed upon them by Parliament.

10692. The next point is the ethical ground upon which your Society objects to vivisection. I hope that you will place that before us very shortly; we have had it at extreme length already?—Perhaps the simplest way would be to submit myself to cross-examination on that statement.

10693. You say in a sentence that you think what is done is cruel?—Yes, I think that we have a right to kill animals, and that the duty is laid upon us in killing them to do so with as little accompanying pain as possible.

10694. Then you have in the course of your evidence already dealt with some of the statements made by previous witnesses. I do not know whether you have finished that part of your case?—I have some more statements of witnesses to deal with, but perhaps I had better keep that. I will do exactly what you like. I have the materials to go on with, but I will subject myself now to cross-examination on these charges or do anything you wish.

10695. You do at the end of your *précis* refer to certain alterations of the law?—Yes, I have my Bill.

10696. I think we had better deal with that, that is very much more practical, of course, than anything else?—I think perhaps it would simplify the discussion before the Commission if I were shortly to give you the alterations in the law which this Bill really would carry out.

10697. If you please?—I put them under 11 headings. The first alteration would be (1) the animal during the experiment to be under an anæsthetic of the nature of a respirable drug or gas.

10698. With regard to the present Act, is this draft which you have put before us by way of amendment to the Act, taking the Act as it is, and amending it?—Yes.

10699. It is not entirely a new Act repealing the present Act?—No, it is an amending Act. The second heading is (2) the animal to be killed before recovering from the anæsthetic. That is really re-instating the Act without Certificate B. The third, and this is really the important thing, is (3) the Inspector to be present throughout each experiment—each physiological experiment, of course, I mean; I do not mean experiments in the nature of inoculation. Provided that in the case of an "experiment not calculated to give pain from the act or operation by means of which the experiment is initiated." (By that I mean an inoculation experiment.) These three provisions shall not apply, but in lieu thereof (1) directly the animal feels pain it must be placed and kept under an anæsthetic, as above mentioned, or killed. (2) The Inspector shall frequently make surprise visits to all places where such experiments are taking place. The idea here is that the Inspector shall be present at all serious physiological operation experiments, but he is not necessarily present at inoculation experiments, which may, of course, last for weeks or months; but his duty will be to go at frequent intervals and inspect the condition of the animals which have been inoculated, and if they are in serious pain it will be his duty to order them to be killed.

10700. I thought you said that these three heads that you have given were none of them to apply to what you call inoculation experiments?—That is so, but in lieu of them there is this further direction that the

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Inspector is often to make visits, and have them killed if they are in severe suffering. Then the next condition is (4) the total abolition of the use of curare, and after that the next one is (5) no application of matter to the eye of an animal for the purpose of observation through the conjectival membrane. (6) Abolition of certificates, and experiments depending thereon, of course.

10701. This is all confined, I presume, to experiments for the purpose of acquiring knowledge?—Yes. The rest of the Act stands.

10702. (*Sir Mackenzie Chalmers.*) You have dealt with lectures upon another point. You would prohibit all lecture experiments?—Yes, I am coming to that.

10703. (*Chairman.*) By providing that no application should be permitted to the eye, you did not mean that no oculist was to apply any application?—No, I mean for the purposes of experiment. Then 7. Licences to be granted only for one experiment or for one series of not more than six connected and consecutive experiments and must specify the time and place at which such experiments are to be performed and the nature of such experiments. (8) The licensee must make within seven days of completion of experiment a detailed chronological report in writing to the Secretary of State of the description, course, and result of the experiment, and is liable to penalties of £20 and £50 for first and subsequent failures to comply with these requirements.

10704. Within how many days?—Within seven days. Of course, I should have no objection to that period being lengthened, if it is found impracticable. I must put in some number of days. The next one is (9). That three copies of every licence granted by, and three copies of every report made to the Secretary of State shall be deposited respectively in the Library of the House of Lords, the Library of the House of Commons, and such other place accessible to the public as the Secretary of State may determine. (10) In the appointment of inspectors and licensees special regard is to be paid to the applicant's reputation for humanity, and (11) prosecutions may be instituted at any time within two years of the offence without the assent of the Secretary of State, the prosecutor, however, being bound to deposit the sum of fifty pounds as security for costs.

10705. (*Colonel Lockwood.*) To prevent any vexatious indictment?—Yes. Those are broadly the main provisions of the Bill.

10706. (*Chairman.*) Under No. 6, Abolition of certificates and experiments depending thereon, you mean the abolition of all certificates?—I meant Certificate B really. Certificate A remains.

10707. Do I correctly understand that that is what you put before as the minimum which your Society desires?—Yes, that is the Bill which I drafted which my Society supports, and which has been before Parliament with some small alterations for several Sessions.

10708. (*Colonel Lockwood.*) Are you able to differentiate at all between the ethical and practical views held by various people in this matter, or do you look upon the two as indivisible?—I look upon this question solely from the point of view of the animal. I do not deal with, and I do not address myself to the question of the utility of the practice of vivisection, if that is what you mean.

10709. What I mean is this. Do you hold the opinion that, if it was proved that certain operations lead to discoveries to alleviate human suffering, you would not object to them; or do you object to them altogether because you believe them to be immoral?—Not at all. My objection to vivisection begins and is centred in the question of pain. If an animal be placed under complete anæsthesia and destroyed before it recovers consciousness, personally I have no objection to that vivisection at all; and anything that might be discovered thereby would be to the benefit of humanity and welcomed by myself.

10710. We have had, as you know, various witnesses before us who refused to deal with what I call the practical side of the question, and dealt entirely with the ethical side. I will not go so far as to say that you dissociate yourself from them, but that is not the opinion that you hold?—I ought to say that I am not against the total abolition of vivisection, because in my opinion the total abolition of vivisection would be the most certain way of preventing any possible torture to animals and saving them from all possible pain; but as it is the pain that I object to, if the pain be really and truly eliminated from vivisection I personally have no objection to it.

10711. In the beginning of your remarks you said that you regretted that the Press were not admitted here because much that the public have a right to know (I think that is about it) would not be known by them?—Yes.

10712. Would you tell me and the Commission exactly what was in your mind when you said that in view of publishing the evidence?—First of all I should have liked to have heard the debate which was conducted among the Commissioners on the question of the introduction of the Press and the permission of myself and others to be represented by counsel. I think that would have been very illuminating to the public.

10713. You mean the preliminary talk of the Commission?—Certainly. On what ground it was that the Commissioners determined to refuse my application to appear by counsel.

10714. You alluded in the early part of your evidence to Queen Victoria's opinion being against vivisection?—I have Her Majesty's letter here. It was sent to the secretary of the London Society who kindly let me see it.

10715. (*Chairman.*) Is that from Her late Majesty?—It is from Sir Henry Ponsonby on her behalf. It is as follows:—"Windsor Castle, May 8th, 1889.—Sir,—I duly laid all the documents forwarded by you before the Queen, who was much interested in the report of 1888. The object of your Society is to effect the total abolition of vivisection, by which, of course, is meant the removal of laws which sanction it. It would be impossible for the Queen to take any part in petitioning Parliament or herself to pass new laws, and therefore, while heartily sympathising with your efforts, Her Majesty is compelled to decline the office of patron.—I am, Sir, your obedient servant, HENRY PONSONBY. The Secretary, the London Anti-Vivisection Society, 32, Sackville Street, Piccadilly, W."

10716. (*Colonel Lockwood.*) In the beginning of your evidence you alluded to the gross inhumanity of the medical profession, I think?—No, I never said that.

10717. I wanted to make quite sure. I made a note at the time, being very much struck by it?—I know what you are thinking of. I was very careful of what I said.

10718. Here I have it at Question 10262. "We are prepared to show that under the existing law there is no limit to the agony that may legally be inflicted upon animals, and we abominate a Statute that authorises gross inhumanity"?—Yes.

10719. Could you elucidate that a little for my information?—Yes; that is a plain statement of fact. under the present Act there is no limit. If a man has Certificate A, there is no limit to the agony he can inflict. I do not say that it does happen. I say that under the present law it can happen. I am not saying for a moment that at the present time provisions are not added to that Certificate which may prevent it under the law. But the Home Secretary might be a vivisector.

10720. When we were talking about Dr. Bayliss's operations not having been performed under an anæsthetic, would not the animal have cried out if it had not been under an anæsthetic?—I never said that. What I said was that "The anæsthetics during these prolonged and fearful mutilations can legally be applied by an automatic pump in another room."

10721. Would it not have been the case that if the pump had not acted properly and the dog was not insensible, there would have been some evidence of pain on its part?—Yes, I have no doubt, but then there was evidence that it did struggle, and the vivisector said it was St. Vitus's dance. They admitted the struggle, but said it was St. Vitus's dance. I do not know what they call it, but it was St. Vitus's dance.

10722. (*Mr. Wilson.*) Chorea?—Yes, chorea.

10723. (*Colonel Lockwood.*) At Question 10,264 you go into the question of fact, that the inspectors appointed by the Home Secretary have no testimonial as to their humanity?—I do not think I said the inspectors, did I?

10724. "Nor the inspectors appointed by the Home Secretary, testify to the personal character or humanity of the applicant"?—Yes, of the other people. I said nothing about themselves.

10725. Yes, it is the applicants. How are you going to meet that in your Bill?—I say that in the appointment of inspectors special regard is to be paid to the applicants' reputation for humanity. I say that certainly men like Dr. Klein would not get a licence under my Bill. His reputation is the other way.

10726. I mean how could you possibly arrive at getting a humane character for anybody excepting from an interested party?—I think it might be done.

10727. It seems to me very difficult?—I think inquiries might be instituted about a man's personal character, his character for humanity.

10728. You think by your Bill you could work it?—I do specify it. I say "a certificate recommending that the application be granted on account of the applicant's reputation for humanity, which must be signed by two or more of the following persons. That is to say, a Justice of the Peace, a minister of any religious denomination in the United Kingdom." That is as near as you can get.

10729. That you think would be sufficient safeguard?—Yes.

10730. And guide to humanity?—As much as one can expect.

10731. And you think that would ease the public mind as to the humanity of the person who got a licence?—Yes, always provided that the Inspector was present at every vivisection. That is what I look upon as really comforting the public mind.

10732-3. The presence of the Inspector invariably?—The presence of the Inspector invariably at serious cutting operations.

10734. In answer to Question 10265 you say, "It will be observed that my Society has been forced to the conclusion that, by placing the interests of the animals in the hands of the Home Secretary the framers of the Act of 1876, unwittingly, no doubt, made a fatal mistake. The Home Secretary has practically delegated much of his responsibility to the Home Office officials, who have, in their turn, delegated their responsibility," and so on?—Yes.

10735. You will acknowledge, I suppose, that the Home Secretary himself, beyond being, of course, the authorised responsible minister for these things, must delegate his work to somebody?—Yes.

10736. To whom would you delegate it?—I say that under the safeguards of my Bill, if it is passed, first of all any person may prosecute without leave of the Home Secretary, provided he is a serious person, and puts up £50 to cover the costs. That, in my opinion, takes the protection of the animals very largely out of the sole care of the Home Office, and allows such a society as mine, or any serious society, to do what they can to safeguard animals in the event of any breach of the law. But at present, I say it deliberately, I have no reason to suppose that any illegal act in the way of torturing an animal or anything of that kind has ever taken place hitherto in the presence of a Government inspector. I therefore rely (I am not a fanatic), upon the presence of a Government Inspector to protect the animal from illegal treatment.

10737. Do not you think that if this power was placed in the hands of practically anybody to prosecute, it would lead to a certain amount of carelessness on the part of the real official of the Home Office, the Home Secretary, because he would say, "Well, after all, anybody in the street, or in a society can take up this matter; I need not be extra careful on the subject"?—I do not think that. I think we can go by the analogy for instance of the children's society, of which I sit on the Council. In practice it is the society that institutes prosecutions; but therefore because the society institutes prosecutions I do not think (I have never observed) that it makes the police any more remiss in their readiness to assist or carry out the law.

10738. In answer to a previous question you said that you were not against experiments, provided they were not cruel to the animals?—That is so, as long as there is no torture of animals I do not object to it.

10739. And you do not dispute the fact that there have been discoveries made by these experiments which have been of use to human beings?—That is a question for experts.

10740. You would not like to express an opinion?—No, neither one way nor the other. What is my opinion against the aggregate opinion of the scientific men of this Commission? I do not set it up for a moment. Perhaps I might add, to elucidate that position of mine, that with regard to the results of vivisection, I see no objection to human beings benefiting by the results, provided that those results are not continually produced by fresh infliction of pain, and serious pain.

10741. You have probably read over Professor Starling's evidence?—Yes, I have read it through.

10742. You will have noticed, of course, that, as he would be expected to do, he traverses the various statemade by Mrs. Cook and various other people who hold anti-vivisection views. Have you any remarks to make upon the statements which he makes as traversing those statements?—No.

10743. In one part I remember he said, "That is absolutely false." For instance, he said of one of the witness's statement at Question 3672, which refers to some of Mrs. Cook's statements about cruelty to animals, "these scalped animals with mutilated nerves were allowed to live and then they were again anæsthetised. This is entirely false." Of course, I had read this statement of Mrs. Cook's before, and I confess to having been considerably horrified; but then, when I come to a skilled witness who says "This is absolutely false," it leaves rather an unpleasant impression upon my mind that I have been misled. Have you formed any opinion about it?—I have formed the opinion that as neither Mrs. Cook nor Professor Starling were present when these experiments were performed, they are both equally at liberty to form their own conclusions from what they have read.

10744. But you see Mrs. Cook's account was published to the public, and this evidence, of course, is published to the public, but the public will not necessarily hear of it?—I should like to go back if I am to be asked this, to see what this was. I do not know what Mrs. Cook was talking about at the moment.

10745. If you would just look over it?—I do not know what it is. If this quotation, "for three or four weeks these scalped animals with mutilated nerves were allowed to live," is a fact, it sounds rather horrifying. That is a question of fact. I do not know what Mrs. Cook is quoting from. I would rather she dealt with it herself.

10745. If you would just look over it?—I do not wish to ask you questions about which you have no personal knowledge?—No, I have no knowledge of it.

10747. (*Colonel Lockwood.*) Professor Starling says at Question 3772, that his view of experiments on living animals is "That science is advanced by experiment; that physiology is a science of living things, and that physiology will therefore be advanced by experiments on living things." Would you give me your answer to that?—I presume that, other things not being considered, physiology would be advanced and might be advanced at any rate by experiments on living animals, but whether physiology thus discovered is of any service to mankind is another question.

10748. (*Chairman.*) That is a point upon which I understand you do not wish to express an opinion?—That is so.

10749. (*Sir William Collins.*) I do not know that I have many questions to ask you in reference to your statement in regard to the Home Office. Do I correctly understand that your Society approves of the draft Bill, which you have laid before the Commission?—Yes.

10750. Then their legislative project is rather in the direction of amending than repealing the existing Act?—Yes.

10751. And though you might perhaps as a counsel of perfection desire abolition, I think your Society and you would be content with this amending Bill?—We think that to bring in a Bill for the total abolition of all vivisection in the present condition of public opinion would be a waste of time, and that meanwhile we think much good might be done by such a Bill as this to protect animals more than they are protected at present. And we should like to see how this Bill works if it is passed, if it ever becomes law.

10752. From a hasty perusal of your Bill, I do not see any provision in it for certificates such as those obtained under the present Act?—We leave Certificate A alone, but not certificate B. Certificate B is practically abolished in the sense that you must not allow the animal to recover. You may vivisect it as long as it is kept under anæsthetics and destroyed before it recovers consciousness, under my Bill.

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10753. Do you make any distinction as to what animals should be used under your Bill?—No.

10754. Provided that the conditions of your Bill are carried out, you do not object to the use of dogs, cats, and monkeys?—I do not want to make any distinction between vertebrate animals.

10755. Have you formed any opinion as to the number of inspectors who would be required if your Bill became law?—Yes, if the Bill became law my hope would be that, instead of persons now vivisecting all over the country, when they had a serious object which would be recognised by the Home Office as a serious object, it would be worth their while to come to some central place, there to perform the series of operations under the eye of the Inspector which they wished to do; and I should look to a large reduction in the number of registered places, thereby saving the time and the travelling of the Inspectors. I see no reason for a very large increase in the number of Inspectors, provided that their whole time is occupied in inspecting, and provided that the places where they inspect are not too far distant from one another.

10756. Is there anything in the Bill which limits the number of registered places?—No.

10757. I do not think you told me the number of Inspectors that you contemplate?—No. I leave that to the exigencies of the case. At present the two Inspectors do not give their entire time, and inasmuch as they are not required to be present at every vivisection, they spend a good deal of their time in travelling about and seeing nothing, whereas under my Bill their services would never be wasted; wherever they were there would be a vivisection going on for them to inspect, so that an enormous saving of time would be made under my Bill.

10758. You set a good deal of store on inspection?—Yes, I do absolutely. Inspection is the whole thing in my Bill. In my view the present Act contemplates the necessity of inspection, but does not provide that it shall be efficient.

10759. On the ethical ground I understand that you direct yourself against the infliction of suffering, but you do not object to the utilisation of animals for scientific purposes, provided that it is painlessly conducted?—Quite so.

10760. Is it in your opinion immoral to use knowledge which has been obtained as the result of painful experiments on animals?—No, no more than it would be illegal or immoral for me to enjoy the results of a fortune accumulated in the slave trade by my great-grandfather. I need hardly say that he did not accumulate any such fortune, but if he had I should feel myself perfectly justified in enjoying that fortune. Although I should condemn the slave trade, and should certainly condemn its continuance now, or its resuscitation in any shape or form, I do not think it would necessarily morally preclude me from enjoying the results of the fortune. Nor do I think if any valuable invention has been discovered by painful vivisection of animals we are thereby precluded from using it.

10761. I understand that you do not contest the position that useful knowledge has been acquired by painful experiments upon animals?—That I neither admit nor deny, not being an expert.

10762. I do not know whether you are contemplating giving further evidence or whether you have dealt with that heading in your evidence with regard to statements by previous witnesses, dealing with the value of vivisection, both in physiology and in therapeutics?—I rather reserved myself for cross-examination upon that. Of course, though I assert nothing as to the usefulness of vivisection, as an ordinary layman it is open to me (as a layman) to criticise, if I can, any statement that is brought forward asserting great benefits to mankind in the way of lowering the death-rate from any particular disease. If the vivisectors were to claim that they had lowered the death-rate from any particular disease, that is a statement that, as a layman, I am as qualified to criticise as the greatest expert; and therefore in any case of that kind I should certainly not preclude myself, but simply for what it is worth I should examine the death-rates at Somerset House and think myself not precluded, though I am not an expert, from bringing them before the public.

10763. There are many previous witnesses who have called our attention, for instance, to the value of antitoxin as a mode of treatment for diphtheria, indicating that it has been discovered as the result of experiments on living animals, and that the serum is now obtained by a process involving the use of living animals. Have you any remark to make upon that evidence?—I have looked into the death rates from diphtheria before and after the introduction of antitoxin.

(*After a short adjournment.*)

10764. (*Sir William Collins.*) I think I was asking you whether you desired to say anything with regard to the claims which have been made by previous witnesses, as to the beneficial effects upon the diphtheria mortality arising from discoveries due to vivisection?—I have made a table here from the death rates per 1,000,000 persons living, and I think the fairest and best way to do it is to group it in quinquennial periods so as to avoid the slight inequalities of various years. I begin with 1876, and I come down to 1905. In the first quinquennial period, from 1876 to 1880, the average mortality per 1,000,000 persons living from diphtheria was 121; in the next quinquennial period it was 156; in the next 170; and in the next 253. That quinquennial period reaches down to the year 1895, and the "Encyclopædia Britannica" tells us that the antitoxin treatment began in the latter part of 1894; so that it falls just at the end of that quinquennial period. In the two quinquennial periods subsequent to the introduction of antitoxin in the first the death rate is 272, and in the second it is 204. That comes down to 1905. So that both the averages, taking the last two quinquennial periods covering ten years since the introduction of the antitoxin, are considerably higher than in the period from 1876; higher than the three periods during which the antitoxin was not used. I do not set any great store by that, except that there is the fact, when they claim to have cured this disease by this particular remedy, that the death per 1,000,000 persons living, appears to have gone up.

10765. May I revert once again to your draft Bill, and may I ask you how you deal with cases under that Bill, should it become law, in which animals have been inoculated with various morbid products with a view to ascertain the pathological effect?—Under my Bill if an animal is inoculated with a substance that creates a painful disease in it, it will be the duty of the Inspector to go and see that animal frequently, and if it be in a severe condition of suffering it will be his duty to order it to be killed painlessly.

10766. Of course in some cases, I suppose the painful symptoms might be expected to supervene somewhat rapidly after an inoculation?—As to that I presume that as the Inspector will probably be a man of some experience in these matters, and as he will know what the inoculation is from information sent in by the vivisector, he will know whether the painful period is likely to supervene early or late; therefore it will be within his power to protect that animal from unnecessary suffering.

10767. Do you think, under the provisions of your Bill, and with the inspection it provides, vivisection could be carried on without inflicting suffering upon animals?—I should hope so, yes. That is my hope.

10768. (*Sir Mackenzie Chalmers.*) I think you told us that you have never seen an experiment yourself?—No.

10769. And you never wish to see one?—No.

10770. And you have not any intention of seeing one?—No.

10771. Have you ever seen any animal after an experiment in a physiological laboratory?—No, I am wholly unacquainted with the thing from personal knowledge.

10772. Your knowledge is derived from reading?—Absolutely, and from what I have been told.

10773. Have you had communications with licensees, and discussed the question with them?—I know licensees; I have met them and talked to them.

10774. Do you accept their statements?—That is rather a large question.

10775. I will not press it?—It depends upon the man.

10776. I take it that your position is, that it is not what has been called the exploitation of animals that you object to, but the infliction of pain?—Yes, broadly that is it.

10777. Does the question of the amount of pain enter into your consideration?—Yes.

10778. Let us take one or two instances. You know that at present we are losing a million lives a year in India from plague?—I did not know that.

10779. And at home and in India certain experiments have been carried on to test the mode of transmission of plague—experiments on rats and guinea-pigs?—Yes.

10780. Do you think that these are justifiable, or not?—It depends entirely upon the amount of pain involved.

10781. The amount of pain involved in these experiments is to inoculate an animal with plague, which sometimes is a painful and sometimes not a very painful disease. Do you think it is justifiable?—Under my Bill it would be the duty of the Inspector to have that animal destroyed as soon as it manifested signs of severe suffering.

10782. You would not prohibit the experiments?—No, they would be permissible under my Bill.

10783. Take the case of the cancer research which is now going on. You agree that if any further information as to the nature or causation of cancer can be obtained, it is most essential?—Yes, but they have been some years at their researches now, and they have placed an almost inconceivable number of animals under test and under experimentation, and so far they have had no result whatever for the benefit of mankind; if the death rate per million persons living is to be any test at all.

10784. Do you think those experiments and the continuation of those experiments unjustifiable?—I should think it is about time that some period should be put to the numbers and the quantity of suffering that is inflicted for that purpose.

10785. Do you think that the induction of cancer in mice, which are chiefly used, is a painful experiment?—That is a matter for experts. If it be painful, I say it should be dropped as soon as the animal shows signs of severe suffering; it should be destroyed. There are cancers I believe in the human family which are not painful and some that are. I presume it is probable that it is the same with animals. I do not know.

10786. Then take the case of vaccination. The calf from which the vaccine is prepared suffers no doubt a considerable amount of inconvenience if not severe pain. Do you think that is justifiable or not?—I am an anti-vaccinator on the ground of the liberty of the subject quite apart from any other question.

10787. I am not dealing with compulsory vaccination?—I thought you meant that.

10788. I am dealing with vaccination which people are at liberty to adopt or not. We must have vaccine if people are to be voluntarily vaccinated?—Yes.

10789. Would that come outside your Bill or not?—That would not come under the purview of my Bill. That is a trade product you may call it. It is not experimentation for the purpose of scientific research; and my Bill only covers experimentation for the purposes of research.

10790. Any improvement in vaccine I suppose would be a matter of experiment?—Yes. Vaccine has been going some time. I presume it is a well-known product, is it not?

10791. Then to take the antitoxin for diphtheria, you were going to say something about that. Do you object to the preparation of antitoxin for diphtheria?—That I presume is also a trade product. It would not come under the purview of my Bill.

10792. I am not quite sure, but has not the antitoxin to be standardised by means of fresh experiments?—I do not know. I thought it was like vaccine, a thing that is now ascertained and is produced like a trade product. I thought you could buy it by the bottle.

10793. A good many experiments, as you know, are authorised for the purpose of standardising some of these preparations which cannot be standardised by chemical means?—If the Home Office issue licences for the purpose merely of what you may call standardising that is not an experiment, nor a new discovery; I consider that in issuing that licence under the present law there is no question of a new discovery there; therefore, I think such a licence is absolutely outside the Act, and ought not to be granted.

10794. If we are not justified in causing pain to animals for the purpose of alleviating human suffering and the advancement of knowledge, are we justified in killing and maiming animals in sport?—I am not a sportsman myself; personally, I have never shot a bird.

10795. I was not accusing you of being a sportsman; I was asking you whether, if the law deals with one, it ought not to deal with the other?—It does not seem to me to come within the purview of my Bill, but I am ready to give my own opinion, if it is of any interest.

10796. I want to know the view that your Society takes on this question?—My Society has no opinion on that matter; it is simply a Society for the prevention of vivisection, an anti-vivisection society. It has nothing to do with anything else. I should be absolutely going outside my rights and committing a breach of trust as regards its many subscribers if I devoted any of its funds for agitation against anything else.

10797. But may we have your private opinion; you are a very important member of your Society, and I should like to have your private opinion?—My private opinion is that you have no right to inflict unnecessary pain on animals in killing them, and therefore if any sport inflicted unnecessary pain on animals in killing them, I object to it myself personally. I should not do it myself. I do not bring any accusations against others who do, but I should object to it myself, and, so far do I object to it that I have done my best strenuously to support the Spurious Sports Bill now before the House of Commons.

10798. Take, for instance, rabbit shooting. No doubt a rabbit can be killed in a painless way, but in a good day's rabbit shooting a good many rabbits get away maimed, and die in their holes?—I never went out for a day's rabbit shooting, and I do not know.

10799. I was wondering whether it is a consistent attitude that you take up?—My consistency is perfect, because I do not shoot. I have never even hooked a fish. Nobody can say anything against my consistency. I can speak for myself; you asked me only to speak for myself.

10800. Is it a consistent attitude for your Society to take up, only to deal with one form of pain to animals, leaving probably infinitely worse forms of unnecessary pain untouched?—There is the Royal Society for the Prevention of Cruelty to Animals, whose purview is the whole cruelty performed in the world in all forms to animals of all sorts. My Society has taken up one particular form of cruelty to animals, and devoted themselves to it alone, because in their opinion the Royal Society for the Prevention of Cruelty to Animals has neglected it.

10801. You specialise?—Yes, and we think we can do it better than the Royal Society for the Prevention of Cruelty to Animals, even if they did do it, because we have devoted out entire attention to it.

10802. Colonel Lockwood called your attention to your evidence. Your Society publishes detailed accounts, does it not?—What of?

10803. Of its income and expenditure?—Oh, dear, yes.

10804. Do you know whether the other societies that you referred to published detailed accounts or not?—I think I have most of them here. I asked them all to send me their annual reports. I think they are here, if you like to see them.

10805. (*Dr. Wilson.*) You gave the annual incomes?—Yes; there they are, such as I have got (*handing in the same*).

10806. (*Sir Mackenzie Chalmers.*) I was not quite sure of the object of your statement in comparing the income of your Society with that of others?—My object was purely egotistical, to persuade the Commission, if I could, that mine was the most important Society in the anti-vivisection world, and to claim greater attention for my evidence than for that of small obscure societies which have hardly any address and no income.

10807. The point which occurred to me is this: What we have to judge of is not the income of the Society, but the validity of your arguments?—Yes, quite so. I hope mine have been more valid than those proposed by other societies.

10808. The smallest society might send a very important witness?—Yes; but every other society except mine, so far as I know, confines its attention solely to advocating the total abolition of any vivisection whatever, and they all differ from my Society, and somewhat

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acrimoniously on occasion, because we do not confine ourselves to that utopian policy, but are anxious and ready to advocate anything we can for the amelioration of the condition of vivisected animals. And I think it is a very valid argument in favour of our policy that the vast majority of anti-vivisectionists of England support us, and not them. That is why I brought those figures before the Commission.

10809. It will be convenient perhaps on that point to take some of the points in your Bill. Taking Section, Sub-section (3)?—I must apologise for the language "prior to." I do not like it; but that is prepared by a draftsman.

10810. "The animal must, prior to the commencement, and thenceforth during the whole course of the experiment, be under the influence of a general anæsthetic of the nature of a respirable drug or gas, such as chloroform or ether, or alcohol, chloroform, and ether combined, or sufficient power to prevent the animal feeling pain." That would abolish the present certificate B, would it not?—Yes.

10811. Because an experiment under certificate B consists first of the operative procedure, and then allowing the animal to recover from the anæsthesia?—Yes.

10812. That you would prohibit altogether?—That is my desire.

10813. The effect of that would be to prohibit Professor Starling's work on the digestive juices?—If you say so, I accept your statement.

10814. What he told us was that in what he considered important work on the digestive juices, the animal was put under chloroform, was operated upon, and a drainage tube was put in, and after the animal recovered, at stated intervals the digestive juice was drawn off. Those experiments would be absolutely prohibited?—It takes a long time to do that, does it?

10815. Weeks perhaps?—Then certainly I would prohibit it.

10816. Although he said that the animal was suffering really no appreciable pain?—I do not feel myself bound to take his statement on that point.

10817. The practical result of your Sub-section (3) would be to reproduce the existing licence and abolish Certificate B?—Yes. Certificate A I retain to a certain extent. Certificate B would be abolished.

10818. In fact, you draw no distinction between an experiment and an initial operation?—No, I think not. Our view, of course, is that it is under Certificate B that the animal has the greatest risk of suffering under the present law.

10819. Then by Sub-section (5) you would prohibit all demonstration experiments?—Yes, I should, on another ground besides on the ground of the animal, though I do not particularly object to it on the ground of the animal—that is to say, the animal is under complete anæsthesia, and destroyed before recovering consciousness. I think, on the whole, those conditions are more likely to be carried out in the presence of a lot of people than in the solitude of a laboratory. But I do think, from the evidence before this Commission, and from the evidence brought before us, that it does not tend to edification. I do not think it is right that young people should witness these things if they can be prevented. I do not think it is good for or tends to the edification of youth to witness these operations upon animals.

10820. Do you draw any distinction between witnessing operations of that kind on an animal and an ordinary surgical operation?—Yes, I draw a very large and grave distinction. In my opinion, when students go into the theatre of a hospital, what they are witnessing there is a beneficent operation undertaken with gravity by the surgeon for the benefit of the patient who lies there, and a totally different atmosphere of feeling must be present generally from that which obtains in a laboratory, where the simple thing is to find out whether the spleen does this and the blood pressure does that, and has no relation to the benefit of the animal of any sort.

10821. In fact, you disagree with the surgeons who have been here and given evidence on this question?—I decline to put myself up against anybody else. That is my opinion, and the reason I hold it. It is a reason which I hold with a great number of the persons whom I represent.

10822. Experiments should not be performed, you say, for the purpose of obtaining manipulative skill. That is the existing law?—Yes, I think it is.

10823. "The substance known as curare shall not be used or administered for the purpose of or during the experiment"?—Yes.

10824. Curare, if used now can only be used, you know, when a general anæsthetic is given?—It cannot be used, of course, under the present Act as an anæsthetic.

10825. We have been told that for certain investigations curare is essential; but you say that those investigations must stop?—My objection, of course, to curare is one which you must appreciate; that the most careful inspector cannot tell, if the animal has curare in a certain quantity injected into it, from any manifestations on the part of the animal when the real anæsthesia passes off. That is the sole reason I object to it. It is for the protection of the animal. It is impossible to protect an animal from the extremest torture if curare is permitted to be used at all.

10826. That is to say, the animal would make no manifestation; but it is quite possible to keep up the anæsthetic certainly and surely while the animal is under curare?—No, not certainly and surely. No one can tell whether an animal is suffering if it is under curare, except by manifestations which it makes itself of sensation.

10827. Not by knowing the amount of anæsthetic that is given?—You cannot know the amount of anæsthetic that is given from a bottle in another room. There may be a mistake, and every animal differs from every other animal. One animal will take more anæsthetic than another, and I should have thought that you yourself would have recognised the extreme difficulty in protecting an animal from pain if it has had curare as well as another anæsthetic. I feel sure that you must.

10828. I cannot give evidence, but I put to you this question. If a person who is skilled in giving anæsthetics gives the anæsthetic properly, surely there is no danger then?—What does the most skilled anæsthetist in the world do when he is administering anæsthesia to a patient? He does not merely watch a bottle and see how much he administers; he watches the patient for signs of life, and sensation, and it is by the signs of life and sensation in the patient that he is guided. I am sure that any anæsthetist would tell you that.

10829. Is not that for another purpose? Is not he afraid of the patient dying?—Very likely, but he is also afraid of the patient suffering, I hope.

10830. Perhaps some medical witness will deal with that. Coming to your Bill again—this refers to Certificate A: "Directly the animal begins to feel pain it must be placed, and thenceforth while it suffers or is likely to suffer, must be kept under the influence of a general anæsthetic of the nature of a respirable drug or gas such as chloroform or ether, or, alcohol chloroform and ether combined, of sufficient power to prevent the animal feeling pain"?—Yes.

10831. "Or if the animal is not so placed and kept under the influence of such anæsthetic as aforesaid, it must be forthwith killed"?—Yes, it practically comes to its being killed.

10832. That would prevent experiments which have the effect of producing any painful disease where it may be necessary for the experiment to let the disease run a certain course?—Of course, if it is a painful disease, yes.

10833. For instance, plague?—Yes, if it is a painful disease. I believe there are some forms of cancer, as I say, which are not painful—that people have it and suddenly die, never knowing that they had it. I have a cousin who died of it and never knew it.

10834. It would be rather difficult with an animal, would it not, to tell whether cancer is painful or not. Animals show very little sign of pain in case of chronic diseases?—I did not know that; I should have thought that they did. That is for the inspector to decide. If he says that an animal is not suffering at all, I am ready to take his word for it.

10835. You would stop any experiment?—Take such a thing as tetanus—that is what my eye is upon. If you inject tetanus into an animal it gets dreadful sufferings. I think that such an animal should either be placed under anæsthetics when it begins to manifest severe signs of suffering, or be killed.

10836. It struck me that your clause goes a great deal further than the actual cases you had in view?—I hope it will stop all severe suffering. That is the object of my Bill, and that is the object of those whom I represent, to stop suffering if we can—a laudable object, I should have thought.

10837. This will stop not only acute suffering but any suffering?—You cannot have a measure of pain. There is no such thing as a pain gauge after all. I have made it as wide as I can because I want to protect animals as much as I can. You see why I have done it—to protect animals as much as possible.

10838. Not being concerned, of course, with scientific results?—In no way whatever. If science can be advanced contemparaneously with painless vivisection I am glad to hear it; but I do not see why science should be advanced through suffering any more than any art. You would not allow any art to be advanced by suffering, or any other pursuit of any kind. You would not allow astronomy to be advanced by suffering. Why should you allow physiology to be advanced by suffering?

10839. I only wanted to get your view. There is no question of proportion. If any suffering is caused you would stop even what skilled people say is a most useful investigation?—I am not an unreasonable man. When I say suffering I mean real severe suffering.

10840. Would discomfort incidental to certain diseases come within your definition?—If the discomfort amounts to what is really serious, so that the animal is utterly wretched, I say that the animal ought to be killed.

10841. I think your clause goes beyond that?—I consider that the poor dog at Khartoum in that picture reduced to a hideous condition of emaciation, is cruelty. I think that animal ought to have been destroyed.

10842. I have not seen it?—There was a photograph of it, and therefore it must be true—at least I suppose so.

10843. Your third clause says: "There shall not be applied to the eye of an animal by way of experiment any matter or substance for the purpose of absorption through the conjunctival membrane." That would prevent, of course, the injection of cocaine?—It is not meant for that, of course.

10844. I am only taking the words?—I have seen books and pictures of wretched creatures' eyes in a hideous condition, and no doubt you have vivisectors who do use the eye, I believe, as the place for cultivating these dreadful germs, which produce a terrific state of suffering.

10845. I do not know whether your object was to prevent cocaine being used as an anæsthetic?—No, that is not the object of the clause; it is to protect the eye from being used as the nursing ground for bacteria.

10846. (*Chairman.*) You do not say there "any matter or substance likely to cause pain"; you say "any matter or substance"?—I daresay that is an error on my part; that is my intention.

10847. (*Sir Mackenzie Chalmers.*) I see what you mean. Then in Clause 8 you mention the persons who are to sign these certificates?—That is the old Act.

10848. Have you put in any obligation there that the names of the persons should be disclosed?—No, but I will now you mention it. I think that is most important. I quite agree with that.

10849. Under the existing law there is no such obligation?—I am not quite certain about that. I think the name certainly must be on the certificate.

10850. Yes, on the certificate?—That is all the clause says here. There is no obligation for you to publish a yearly report at all in the Home Office, so far as I know.

10851. Would it not be rather inconvenient for these people, who may be continually shifting and taking each other's place, to require their names to be published?—No, I think that anybody who signs a certificate ought not to be ashamed of having his name published in your report. Nobody looks at it except humanitarians.

10852. What is the object of having the names published?—I think that a man would think twice, or even perhaps three times, before he signed such a serious matter as a certificate exempting a man from using anæsthetics, if he knew that his name would be known to everybody as having done it. That would not preclude him, if he thought that the applicant was a proper person for a certificate; but I think it would make him a great deal more careful if he knew that his name would be published. I do not think that anybody would have been found year after year to sign a certificate for Dr. Klein if he knew that his name would be published for the world to see.

10853. On that, Dr. Klein has done nearly the whole of his work for the Local Government Board, has he not?—I do not know. If you say so I accept the statement.

10854. And under the instructions of the medical officer for the time being of the Local Government Board?—I suppose he is not the only person who could do that work.

10855. You are asking me a question that I cannot answer?—I think that some one else would have been better than a man who had proclaimed himself in public as inhumane, as he has.

10856. Are you aware that at the time when he gave that evidence he was, at least he said he was, somewhat imperfectly acquainted with English?—Yes.

10857. And that he asked to correct his evidence?—I know that, and I know also that the Royal Commission declined absolutely to have anything to do with his corrections.

10858. They published both his evidence and the corrections?—Yes. If you like to have this out I will give it you. Anything more condemnatory of his whole conduct cannot be conceived, in my opinion, than the whole thing. I have it here. Here it is, Dr. Klein's letter to the Secretary: "Dear Sir,—I return you my evidence corrected. I may mention to you that when under *vivâ voce* examination, the fact of my being a foreigner made me often not able to appreciate all the purport of the questions which were asked of me, and that therefore my answers were not always such as I would have desired to give if I had quite understood the questions. In revising the evidence I have endeavoured to remove the effect of any such misapprehensions.—I am, etc., E. KLEIN." Mr. Baker, the Secretary, replied: "Sir,—I beg to acknowledge the receipt of your letter of the 16th instant, together with the proof of your evidence, and to reply that, as your corrections appear to me to have exceeded the latitude usually allowed to witnesses, I must lay it before the Commission, and take their directions as to the course I am to pursue. I am, etc., NATH. BAKER (Secretary). P.S.—I must ask you to return me the proof I originally sent you, which you damaged, for, as I told you when I sent you the duplicate, I am answerable for all the copies that are supplied to me." Then Mr. Baker writes again: "In my letter of the 12th inst., in which I forwarded you a second proof I told you that I must have the original proof back. On the 17th instant I again asked you to return it to me. I regret that I must now formally request you to return it to me at once." Dr. Klein now replies: "Dear Sir,—I am very sorry to have given you so much annoyance, as it appears from your letter. As I have stated on the occasion of my asking for a second proof of my evidence, the first proof has been accidentally damaged so much that it was quite useless. I therefore destroyed it, and am not in a position, consequently, to comply with your request. I beg again to apologise if I should have put you to any inconvenience.—Believe me, etc., E. KLEIN." To which he gets this answer: "Sir.—Your letter of the 16th November has been considered by the Commission, with the proposed alterations of your evidence. These appear to the Commission to be so much at variance with the letter and the spirit of the answers you gave at your examination, that the substitution cannot be permitted. The Commissioners direct me to say that they observe with regret that after my letter of 12th November you should have destroyed the proof I originally sent you, and requested you to return. If you desire it, the Commissioners will include in the appendix your letter and the proposed alteration, with the correspondence which has passed with reference thereto." And they did it. In these circumstances, as I have been asked to produce that correspondence, I once more ask to be allowed to read Professor Huxley's letter to Mr. Darwin.

(*Chairman.*) I do not think that reading what you have now read makes any difference on the question of Professor Huxley's letter.

(*Witness.*) I only ask to read it, and it is refused.

10859. (*Sir Mackenzie Chalmers.*) We have not had Dr. Klein's corrections. I do not know what he wished

to say?—He watered it all down of course, but I should have thought that as the Commission declined to receive them it would not be material. But you can have them.

10860. (*Chairman.*) I do not understand from these letters that the Commission declined to receive his corrections; they said that they would publish them as his statement of what he wished his evidence to be altered to?—Yes, but they declined to accept them.

10861. No, they declined to strike out the evidence?—They declined to accept his corrections.

10862. As corrections?—Yes.

10863. They published both the evidence and the corrections?—Yes.

10864. (*Sir Mackenzie Chalmers.*) They published both and left the public to judge?—Yes. The Secretary said of the corrections: "These appear to the Commission to be so much at variance with the letter and the spirit of the answers you gave at your examination, that the substitution cannot be permitted."

10865. (*Chairman.*) The substitution?—Yes.

10866. (*Sir Mackenzie Chalmers.*) At any rate Dr. Klein himself wished to withdraw a good deal of his evidence?—No doubt he did.

10867. And, rightly or wrongly, he put it on the ground that he was a foreigner, and not very conversant with English at the time?—That is all quite true.

10868. Do you know whether it is a fact that he had done any operations except inoculations?—When?

10869. At the time when he gave his evidence?—I do not know in the least. How could anybody know?

10870. Was he not examined on it?—That must be a fact that no one can know.

10871. At any rate, since the Act, do you know whether he has done anything under his licence for the Local Government Board except inoculations?—I have not the slightest idea what he has done. He has the run of the laboratory at the Brown Institute, and as the Inspector is not there, I do not suppose that anybody knows what he has done. He has a licence.

10872. (*Chairman.*) Do you say that he is in the habit of conducting experiments at the Brown Institution?—I say that he is licensed to perform experiments there. I think he is still licensed.

10873. (*Sir William Church.*) But Lord Selby meant, would you please say when he ceased his connection with the Brown Institution?—That I will look out; I do not know; I am only speaking from my own memory, but it is easily looked up.

10874. (*Chairman.*) I do not know anything about him myself. Sir William Church tells me that it is a good many years ago, possibly 20, he does not know how long exactly, since Dr. Klein ceased to have any connection with the Brown Institution?—I do not think that is so. Here you are—"1904. E. Klein, Brown Institution, Wandsworth Road, London."

10875. (*Sir William Church.*) That was when he was acting under the Local Government Board?—I can only say that he is licensed to perform experiments at the Brown Institution. There is nothing in this Report about the Local Government Board; I can only go by what is published by the Home Office.

10876. I beg your pardon if I misunderstood you, I understood you to say that he was connected with the Brown Institution itself?—No.

10877. He was performing certain experiments there, but they were being done for the Government?—I do not know what they were done for. My sole statement is that Dr. Klein is licensed to perform experiments at the Brown Institution; that is all I know, that is all I state, and all that I ever meant to state.

10878. (*Sir Mackenzie Chalmers.*) You do not think it is very material what experiments he is authorised to perform?—I think it is very material what experiments he is authorised to perform. I may say in passing that I do not find his name in any medical directory. I suppose he is a foreigner, because he does not seem to have taken any degree in England. I am told he is a Fellow of the Royal Society. I did not know whether I was right to call him Dr. or Mr., that is all.

10879. Turning to your evidence in answer to Question 10262, Colonel Lockwood referred you to the sentence in which you say that you are "prepared to show that under the existing law there is no limit to the agony that may legally be inflicted upon animals," and that you "abominate a statute that authorises gross inhumanity"?—Yes.

10880. You know that the Council of the Royal Society have given evidence here by their President, who, on their behalf, pressed upon this Commission that the existing law and practice should be maintained?—You mean Lord Rayleigh?

10881. Yes. Do you charge him with inhumanity?—That does not arise out of my statement.

10882. I am asking you now?—I thought you were appending it to my statement.

10883. No, I ask you the question. The Royal Society support a statute which you say has this effect. Do you charge them with inhumanity?—Let me see what Lord Rayleigh did say. Will you tell me what it is you refer to, and I will look it up? I have the very greatest respect for Lord Rayleigh.

10884. You will find it in the second volume of our evidence, on page 53: "So much has already been gained from the application of experiments on animals, both for the progress of physiology and for the alleviation of human suffering, and so much more may be confidently expected in the future, that the President and Council trust that nothing will be done that would hamper he legitimate employment of the method"?—What has that got to do with my statement that the present law admits of any amount of torture? I do not want to be hypercritical. You seem to raise it on that statement of mine. I say it is not *ad rem.* Of course, if Lord Rayleigh says that a great deal has been gained from the application of experiments on animals, that is his opinion, and you are right to give it the greatest weight here, of course. I am not here to deny that.

10885. You are here to suggest an alteration in the existing law, which the Royal Society deprecate?—In any alteration of the existing law I trust that nothing will be done to hamper the legitimate employment of the method. Nor does my Bill do so. The legitimate employment of the method is not hampered by my Bill, but I consider that torturing animals is an illegitimate method—and I should hope that everybody would agree with me.

10886. (*Chairman.*) Do you say that the Royal Society suggest any alteration in the existing law?—No.

10887. That is all Sir Mackenzie Chalmers asked?—I do not say that they do. They have not seen my Bill; they have not had my Bill before them; they do not know what the suggestions are; they do not say anything about the law as a matter of fact; they simply say that they trust that nothing will be done that would hamper the legitimate employment of the method. There is no word about the law there or any alteration of the law. I find nothing of the sort to support that suggestion. On the contrary, in the very next sentence they say, "While precautions should undoubtedly be taken against improper use of experiments on living animals, it is not the province of the Society to suggest what safeguards should be adopted." As to that I suggest the safeguards which should be adopted to carry out Lord Rayleigh's intentions.

10888. (*Sir Mackenzie Chalmers.*) Then they go on: "It is, however, the bounden duty of the President and Council to urge that those safeguards should be so framed as not unnecessarily to interfere with that advancement of knowledge to promote which the society exists"?—I entirely endorse that.

10889. "Such restrictions would not only cripple or arrest the growth in this country of an important branch of biological science, but in so doing would reduce the efficiency of both physician and surgeon to mitigate or cure disease"?—I endorse every word of that. I entirely agree with Lord Rayleigh, but I maintain—and I expect that he would agree with me—that if the sole hamperings that I suggest are to prevent animals being tortured, provided that science is left free, those hamperings should be applied at once. I do not imagine that he would be so inhumane as not to agree to that, and if you ask me whether Lord Rayleigh is humane or not, I see nothing in this statement to suggest that he is not as humane as myself.

10890. Coming to the Royal College of Physicians, you have read Sir Douglas Powell's evidence, I suppose?—I have read it through.

10891. Is there any point that you wish to criticise on that?—Really I do not know that I have anything.

10892. I thought you said that there were various points in the evidence which you wished to criticise?—Yes, I have a list of them. I have got them in one packet here. I am ready to deal with them at any moment, if I am invited to do so. (*After referring to notes.*) Yes, I think I have something with regard to Sir Douglas Powell. At question 5,606 he was asked: "You do not believe that any cruelty, as we should understand it, is practised?" To which he replied: "I should say emphatically that it is not." Then "Have you ever seen any experiment in which the animal was allowed to recover from the operation without being killed? (A.) Not in recent times, not since the Act, certainly." I say that any statement, therefore, on the part of Sir Douglas Powell as to whether cruelty takes place under this Act is wholly out of court. He cannot speak of what happens when he is not there any more than I can; and I can, with equal emphasis, assure the Commission that cruelty is practised on equally good grounds. We were neither of us there.

10893. Are you aware that he said that he always considered carefully before granting a certificate as President of the College?—Yes, I daresay. If you say so, I accept it. He only considers the physiological fitness and the scientific eminence of the applicant; he does not pretend, nor does anybody, so far as I have seen throughout the evidence, that he takes into consideration the humanity of the applicant.

10894. Would it not be rather an impossible task to get evidence of humanity or inhumanity?—I should think not; at any rate, the effort might be made.

10895. I have heard of certificates of honesty, but I have never heard of certificates of humanity; I do not know on what evidence any such certificate would be founded?—I have suggested in the Bill how it should be done.

10896. You have suggested two clergymen or two justices?—No, one justice of the peace and one minister of any religious denomination.

10897. How could they know whether a man is humane or not; how can anyone know?—I presume that a man has friends who know something about him.

10898. (*Chairman.*) Is it essential in your Bill that he should have a minister of religion to speak for him?—The certificate must be signed by "Two or more of the following persons." Two justices of the peace will do perfectly well without any minister of religion.

10899. (*Sir Mackenzie Chalmers.*) You draw in your evidence a distinction between the Home Secretary and the Home Office?—You have done so yourself in one of your letters to me, which I read this morning.

10900. The Home Secretary speaks for his Office, of course?—Yes. I draw the distinction which everybody I suppose, with common sense, would draw, that the Home Secretary himself as the head of the Office passes away with the Government, and the permanent officials who are there always must of necessity take into their care such an Act as this, and I have no doubt that they do so.

10901. You know that the permanent officials are nearly as fleeting as the Home Secretary?—Oh, no.

10902. You will find it is so?—I hope not.

10903. For instance, in this way. Vivisection is in one branch of the Office?—Yes.

10904. A man is continually transferred from one branch to another, as a man gets promotion from one branch to another. The only permanent person is the Under-Secretary, for the time that he is there?—Oh, yes.

10905. Anyhow, what I want to come to is this. Many of the cases that you have referred to must necessarily have come before the Home Secretary himself. He has to answer the questions in Parliament?—Yes, but I assume, and I have no doubt it is the fact, that those answers are almost invariably prepared for him by the permanent officials of the Office; he does not write them of his own initiative. If you are prepared to tell me that he does——

10906. I cannot give evidence, but I can put the question in this way. The draft is prepared for the Home Secretary, of course, of the answer to be given in Parliament; that draft he considers himself, alters himself, asks for further information himself, as he wishes and pleases?—No doubt.

10907. And he ultimately is the person responsible?—Yes; that is a technical matter, which I must leave to you and your chief. It is for you to say who is responsible. I bring my indictment against the Home Secretary—and his Office, if you prefer it; only I think myself, and I have always thought, that it is the Home Office officials who really are the persons who frame these answers. The Home Secretary cannot be a master of all the details of an office of that kind.

10908. Certainly not?—Naturally.

10909. But if you take the people who have been Home Secretaries since the Act of 1876, the whole series of them—Lord Cross, Sir William Harcourt, Sir Matthew White-Ridley, Mr. Akers Douglas, Mr. Asquith, Mr. Gladstone—do you really think that they have not exercised independent control?—I am afraid I must answer that question very likely in some cases in the affirmative. I think it was under Sir William Harcourt that the habit grew up of consulting this Association of Vivisectors before a certificate was granted.

10910. That was instituted by Sir William Harcourt himself?—Yes. I dare say in big questions of policy of that sort the Home Secretary is the person whose word is taken, but in all matters of detail and all questions of routine, it is not so.

10911. Do you think that with all the attention that has been drawn to these matters in Parliament, each Home Secretary has not considered and laid down his own line?—No, I think they are immensely influenced by the officers behind them. I think it would be extremely difficult for the Home Secretary to take an entirely new line inside the four corners of the Vivisection Act against the settled habit, if I may say it, of the permanent officials, because settled habit it has been, as I have shown, I think. I think it is almost incredible that the same settled habits should be manifested under so many different heads, unless it were that the permanent officials were almost supreme in that matter.

10912. Have not the permanent officials, as I asked you before, altered almost as fast as the Home Secretary?—I have had a great number of letters signed by the same person, I know, through many years.

10913. You object to Sir William Harcourt's proceedings in making a reference to the Society for the advancement of Medicine by Research?—Most decidedly.

10914. Each licence and certificate goes to them. Are you aware what happens when they come back? Are you aware that certificates are continually altered and modified, and sometimes refused?—My point is that, so far no evidence has been offered here before this Commission to show that the Home Secretary has ever gone contrary to this association till the Commission was sitting.

10915. And, of course, there is no evidence, equally, that he has followed those recommendations?—The next thing I want to ask you about is this. You referred to Dr. Schäfer's drowning experiments. You are aware that his object was to verify the researches of a particular method of restoring the drowned?—I am not aware of it. I accept your statement. The object does not concern me.

10915A. It was a research carried out by him, I think, for the Royal Chirurgical Society, who wished to know what method ought to be recommended, there being four or five different methods of resuscitating the drowned. That, you will allow, is an important matter, as to what is the best method of restoring drowned persons?—Oh, dear, yes.

10916. Are you aware that Professor Schäfer performed 36 experiments?—I am not in the least aware of that. I will accept your statement.

10917. Thirty-four of those were under anæsthetics?—You say so.

10918. Does that affect your opinion of his experiments. You say that his experiments were regarded with particular detestation?—I did not say that. I said your permission to him was regarded with particular detestation.

10919. Does it make any difference in your opinion that 34 of those were under anæsthetics?—It may be that Professor Schäfer was much more humane than

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the Home Office. The Home Office may have given him permission to do things which he did not do. My complaint is that the Home Office permitted him to do them, and they gave him a certificate that he might do them. I do not know whether he did them or not. My complaint is that he was permitted to do them.

10920. Thirty-four of those experiments were licensed under anæsthetics. He then applied (will you take it from me) for eight experiments without anæsthetics?—You say so; I know nothing about it.

10921. But he only performed two—will you take that from me? He considered those experiments unnecessary to control the conditions under which the anæsthetic experiments were performed?—The *gravamen* of my charge is that the Home Office gave Professor Schäfer leave to drown and re-drown the same dog without anæsthetics. That is the *gravamen* of my charge. What he did is not known to me nor to anybody but himself.

10922. Will you take from me what he did and what he wanted to do: that the two animals that were not under anæsthetics were not revived, but were simply drowned straight away?—What is your question?

10923. Will you accept his version of what actually happened: that the two animals which were not put under anæsthetics were drowned, and not allowed to recover?—You challenge me to make a charge of cruelty against Professor Schäfer. That is not my intention. I do not know Professor Schäfer.

10924. I am asking you about the actual experiments which were performed under the permission of the Home Office?—If your statement is correct it only proves to me that Professor Schäfer was more humane than those who gave him permission. You gave him permission to do something far more cruel than he actually did, if your statement is correct.

10925. There was a question asked about it in the House?—Yes, there was; that is what I am basing my answer upon.

10926. Will you read the question and answer?—I am afraid I have not got it here at the movement. (*The question was postponed.*) You do not traverse that statement of mine, that permission was given by the Home Office to drown and re-drown dogs?

10927. To test the means of resuscitation?—There was nothing in the certificate to prevent his drowning and re-drowning the same dog. I had that question asked in the House (I am sorry I cannot find it you now) and the answer was that Professor Schäfer had informed the Home Secretary that he had not done so.

10928. I do not think the certificate would ordinarily be in those terms. However, we can get the original certificate?—Yes. I am perfectly convinced that the permission given him was to drown and resuscitate dogs without anæsthesia.

(*Chairman.*) It must depend upon the document.

10929. (*Sir Mackenzie Chalmers.*) Anyhow, whether that is so or not, for the purpose of saving innumerable human lives, would not that, in your opinion, be justifiable?—I have not heard that one single person has been resuscitated in the country by these experiments yet. I have read the account, and so far as I can see the sole thing was, that you must put the person on his face so that the gravitation might act on the tongue and it would come out of the mouth; whereas the other way it would fall back and choke him.

10930. (*Chairman.*) That is not a full explanation of what he stated?—I read the account.

10931. I remember that he said more than that. One particular thing that he impressed was that a single person could do it with very few movements, whereas if you carry it out with the present methods it requires two persons if you are really to do it effectively, and it is a much longer process to set about.

10932. (*Dr. Gaskell.*) And the amount of air exchange was greater, and that any unskilled person can do it. You can describe by a photograph the exact method, whereas you can only teach the Sylvester method by means of a skilled teacher?—I can produce the question and answer in the case of course. I have not seen the certificate. We can get it to-morrow.

10933. (*Sir Mackenzie Chalmers.*) You think, anyhow, that even with the prospect of saving innumerable human lives such an experiment, if performed, was not justifiable?—My position is a very clear one on that. I say that the loftiest motive professed by persons who inflict pain and suffering upon animals does not justify their inflicting pain on animals. If so, as I said before in my opening statement, you might steal to devote the money to good purposes. An act is either right or wrong, and it is quite apart from the motive of those who perform that act.

10934. Do you object to drowning dogs as a means to getting rid of superfluous dogs?—I prefer the lethal chamber at Battersea.

10935. But if a dog is drowned right out it suffers no more if it is drowned in the course of an experiment?—I have always been told that the proper way to kill a dog is to shoot it—that that is the most merciful way, and I presume it is the way in which most people do it.

10936. In Edinburgh, at all events, when Professor Schäfer performed his experiments, and until recently, the mode of getting rid of superfluous dogs was by drowning, was it not?—I do not know. You know it is the resuscitating the dog and then drowning it again which was what I said was the *gravamen*.

10937. That as a matter of fact we were told was not done except under anæsthesia?—That may be.

10938. Now then we come to some experiments which certainly read as very horrible experiments, by Dr. Crile. You know nothing about those experiments except from Dr. Crile's own published report?—That is so.

10939. Would you accept his own explanation?—Certainly not; of course not. I read his book, and see what he says, and I accept that; but subsequent explanations when he is called upon to explain himself by the Home Office, if he was, I certainly do not accept.

10940. He was called upon, and perhaps I may read his explanation as given by himself?—I do not accept it for a moment.

10941. Whether you accept it or not, perhaps you will kindly listen to what he says?—Certainly.

10942. Dr. Poore wrote to him, The first letter miscarried, because he was in some out-of-the-way part of America. Then Dr. Poore wrote again, and he replied.

(*Chairman.*) What is the date?

10943. (*Sir Mackenzie Chalmers.*) There is no date of the year here. "Dear Doctor,—Replying to your favour of June 19th" (we can get the date from the previous letter) "I beg to say that in the entire series of experiments no animal suffered pain. The reference to the corneal reflex was correctly made, but the animal was not conscious. In the remark to Protocol XVI., page 31, 'At one time the anæsthetic was overlooked' simply meant that the animal had recovered far enough from the state of surgical anæsthesia to exhibit a corneal reflex, and begin a slight voluntary movement. By surgical anæsthesia I mean the administration of the anæsthetic to the same degree of anæsthesia as in the human subject in the performance of surgical operations. I wish to emphasise that in no instance did any animal suffer pain. In nearly every experiment I had a special anæsthetizer. As I am engaged in an active surgical practice, and have been for years, I feel myself quite competent to state that the animals I have subjected to vivisection have not suffered any more than the patients upon whom I have performed operations. And the latter, I know, have not suffered at all. The experiments were all performed as nearly as possible under the same conditions as operations on patients. I shall at once send a book to the Secretary of State. The delay in my reply was due to my being abroad—just returning yesterday. Thanking you for writing me, and if there is anything more that I can do, please let me know. Very respectfully yours, GEORGE W. CRILE, to G. V. Poore, M.D., 32, Wimpole Street, London"?—It is rather strange that he quotes that one statement that "the anæsthetic was overlooked" in the letter, and then goes on to say "The animal was coming out of anæsthesia, and the corneal reflex appeared." He did not read his own experiment, because the next words in the experiment are "The dog became profoundly under its influence," which, of course, meant that the dog had got too much, not too little, "Causing a very great fall in blood pressure of 40 millimetres in 85 seconds." So that in the book the overlooking was that it got too much, and in the letter the overlooking is that it got too little. You can look at the book for yourself. The first thing he

says in his letter defending himself shows that he is not accurate. So that he answered you without looking at his own book.

10944. Possibly?—In such a hurry was he to defend himself, in my opinion. There was no allegation against him in that experiment about anæsthesia, because it was there stated that the animal became profoundly under its influence.

10945. His letter refers to the whole experiment?—No, he quotes that particular one in that letter. That is the very one he picks out.

10946. "I wish to emphasise that in no instance did any animal suffer any pain." That refers to the whole?—Yes, quite so.

10947. Are you aware that Sir Victor Horsley saw a good many of these experiments?—I do not know, I am sure.

10948. Are you aware that he was of opinion that no pain was caused to the animal? We shall have his evidence—he is coming here?—No doubt he will say that.

10949. Are you aware that Dr. Goodbody also saw these experiments?—No, my whole position is that the Inspector should be there. If the Inspector had been there I should have been prepared to take the Inspector's word that these animals suffered nothing. The Inspector was not there, and I decline to accept the bare statement of the performer, particularly when this man himself, in his letter of excuse, clearly shows that he is not accurate.

10950. You accept his statement in the first instance, and you will not accept it in the second?—I will not accept Dr. Crile's statement at all.

10951. Will you accept Sir Victor Horsley's statement as a spectator?—Certainly not. I do not accept a person's account of his conduct in an experiment when his conduct is impugned, for one moment. I say that there should have been an inspector there. Parliament in its wisdom did not trust these men. If it had it would not have told you to appoint an inspector there to watch them. As the Inspector was not there I say I do not trust them. Parliament did not trust them; why should I? Parliament was not satisfied with their uncorroborated word as to what happened in their laboratories. Why should I be?

10952. We may have inspection, but still we may ask a person of character, when he saw the experiment, what his opinion of it was. You will not accept that?—Certainly not, because I think that all these experimenters have the greatest contempt for the Act of Parliament. They would deny a breach of this Act just as I should deny a breach of the Motor-Car Act. I drive a motor-car, and when I go beyond the speed limit, and a policeman asks me, I say, "No, I am not going beyond the speed limit." Nothing would keep me from going beyond the speed limit except the presence of a policeman in the car; and nothing will keep an experimenter within the four corners of the Act except an inspector in the laboratory.

10953. Surely if you were asked about the speed limit, and gave your word that you had not exceeded it, you would not expect to be disbelieved?—No, I do not say so. I said last year that, of course, I did, and I exceed it every time.

10954. (*Chairman.*) You are apparently not very ethical about motor-cars. If you apply your principles as regards motoring to the physiologists, you have very little to say against them?—What I have to say is that they regard the Vivisection Act of 1876 with the same contempt that I regard the Motor-Car Act as regards the speed limit. Every motorist who has a motor-car, of course, breaks the Act.

10955. What you said was that if you were going more than 20 miles an hour, and were stopped for it, you said that you were going less than 20 miles an hour?—No; on the contrary, I was asked, and frankly answered that I was going beyond 20 miles an hour. I did not mean that. I always frankly admit it. When I am summoned, and get to the law court, I always admit it.

10956. Then you must not be quite so hard on a physiologist as to say that men are not to be believed on their oath?—I did not mean that; I meant that they regard the Act in the same sense as we motorists regard the Motor-Car Act. We do not treat it with very great respect as regards the speed limit.

10957. (*Sir William Church.*) Some of us?—Some of us.

10958. (*Mr. Ram.*) You think that these people will tell a lie about this because they have a contempt for the Act?—I did not say that.

10959. That is what I gathered. You despise the Motor-Car Act, which says that you may not go more than 20 miles an hour. Therefore on the face of your evidence you would not mind saying what was untrue with regard to it. You say that these men have a contempt for the Act, and therefore they say what is untrue?—No, I say that they will break the Act with the same facility as I break the Motor-Car Act, for the same sense of any lack of morality about it.

10960. If one of these physiologists says, "I performed an operation, and I say the animal did not suffer," is that any reason why they should not be believed?—I do not think it is unprejudiced evidence. What do you expect them to answer? What would you expect Dr. Crile to say—"I, of course, torture the animals"?

10961. (*Sir John MacFadyean.*) The same as you do when you are asked about the speed limit?—The difference there is very great. Nobody regards anybody a pin the worse for exceeding the speed limit. Nobody, so far as I know or ever heard of, would hold it up against anybody as a moral fault to exceed the speed limit, but merely a technical breach of the Act; but I think that the public would certainly regard it as a moral offence if Dr. Crile were to admit that he tortured these animals.

10962. (*Sir Mackenzie Chalmers.*) We shall have Sir Victor Horsley's evidence, for what it is worth, as you say?—Yes.

10963. Do you suggest that the Home Office could have made any further inquiries than they did from persons who were present and from Dr. Crile himself?—I do not say that. I say that the present Act of Parliament is most defective, because they cannot get at present any information except from the person whose conduct is impugned. Therefore I consider that all the evidence that they get must necessarily be prejudiced. That is why I want the Inspector to be there always.

10964. But you agree, do you not, that the Home Office have to carry out an Act of Parliament as they find it, whether they like it or whether they do not?—Yes, I suppose that is so.

10965. Some Acts they may like, and some Acts they may not like, but they equally have to administer them according to the words of the Act?—Yes.

10966. And you know that as a matter of legal construction, not even expressions used in Parliament can be referred to, to gather the intention of an Act. You cannot refer to the debate in Parliament as throwing any light on the meaning of words used in an Act?—Is that so?

10967. It has been laid down pretty often?—I suppose that is so.

10968. Still less can you refer to the Report of a Commission, which may or may not have had anything to do with the foundation of the Act?—No, certainly there is a difference between a speech made in the House for or against an Act and the report of a Royal Commission upon which an Act of Parliament is based; at least I should have thought so.

10969. You have a Royal Commission, and then the Minister who prepares the Bill takes or does not take its recommendations, and in construing the Act you only have to look at the words of the Act itself which is the final expression of the Legislature?—I think that extraneous considerations are allowed. For instance, I have here Maxwell's "Interpretation of Statute," the fourth edition, page 30, "The liberal construction of an Act of Parliament) has, in general, but *prima facie* preference. To arrive at the real meaning it is always necessary to get an exact conception of the aim, scope and object of the whole Act; to consider, according to Lord Coke (Haydon's Case, 3 rep. 7b; 10 rep. 73a):—(1.) What was the law before the Act was passed. (2.) What was the mischief or defect for which the law had not provided. (3.) What remedy Parliament has appointed. (4.) The reason of the remedy. According to another authority (*per* Lord Justice Turner, in Hawkins *v.* Gathercole, 6, De G. M. G 1) the true meaning is to be found, not merely from the words of the Act, but from the cause and necessity of its being made, which are to be ascertained not only from a comparison of its several parts, but also from extraneous circumstances." Therefore I think you may call

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the Report of a Royal Commission an extraneous circumstance which ought to be taken into consideration.

10970. (*Chairman.*) You may think that is good enough for us poor Commissioners, but in a Court of Law I think you would find some difficulty in making that assertion?—It is Lord Justice Turner.

10971. He never said that you can interpret a statute by the Report of a Commission?—No, he does not say so, and I do not say so. He says that extraneous circumstances may be taken into account.

10972. (*Sir Mackenzie Chalmers.*) Is not that when the language of the Act is ambiguous, and not when the meaning of the Act is clear?—"We have therefore to consider not merely the words of this Act of Parliment, but the intent of the Legislature, to be collected from the cause and necessity of the Act being made, from a comparison of its several parts and from foreign (meaning extraneous) circumstances so far as they can justly be considered to throw light upon the subject." Those are Lord Justice Turner's exact words. I think that the Royal Commission does throw light upon the subject, so far as I can see. It is not very important.

10973. Further on you draw a distinction between the Home Secretary and his Department, which you have already mentioned. Of course, you agree that the Department is under this difficulty: that it never can reply. No officer of the Department who is attacked can ever reply?—I do not attack any individual in the Office, of course, and I have never done so. I have never done such a thing at all.

10974. I am not referring to the present time; I am rather referring to your objection to Dr. Poore. When Dr. Poore ceased to be Inspector, he made a speech of which you disapprove?—Yes.

10975. At the time when he was Inspector you and your Society had attacked him, had you not, as Inspector. You had raised various points about his inspection and his administration of the Act?—Very likely, about his reports.

10976. Then when he became a free man he retaliated?—I do not think that is so.

10977. You do not accept that way of putting it?—I think that his making that speech, so soon after he was relieved from the trammels of office, showed that he must have had a bias while under the trammels of office; and besides, I fail to see why criticism by my Society upon the Inspector should produce any personal animosity of that kind, or why it should find expression in such violent language.

10978. As a matter of fact, what he was doing was hitting back when he was a free man, was it not?—I wonder if he was ever criticised by the vivisectors.

10979. Then after Dr. Crile's experiment I think you wrote some letters to various surgeons?—I did.

10980. How many did you write altogether, do you know? We have got answers, I think, from seven?—I wrote to a great number. The vast majority did not answer me at all. I know that many more did not answer me than did. I really forget how many.

10981. Seven are all the answers that you got, I understand?—One from Sir Victor Horsley's secretary to say that he was away, and one from Professor Schäfer, and the rest from surgeons.

10982. You wrote a good many?—Yes.

10983. And you got answers from seven?—I wrote to many more than seven, certainly.

10984. Then there was some subsequent correspondence from some of them, was there not—Mr. Timothy Holmes and Mr. Owen?—I dare say there was.

10985. That subsequent correspondence we have not had?—I will get it.

10986. For instance, in the "Times" of March 15th, 1902, from Dr. Owen?—Yes, there was one letter to the "Times," I remember now.

10987. Shall I read it?—If you read his letter, I hope you will read my reply, if you have it.

10988. Yes. I have your reply too. This is from the "Times," of March 15th, 1902: "Anti-Vivisection Methods.—To the Editor of the 'Times.'—Sir,—When Mr. Stephen Coleridge asked me some time ago if it was true that at St. Mary's Hospital operations were performed on patients under incomplete anæsthesia, I at once sent him a reply in the negative. For, knowing his methods, I was afraid lest he was dressing up some horrible bogey, wherewith to shock a generous but emotional public, at the time when we were raising money to finish our much-needed Clarence wing. And, standing as it did, my reply was perfectly correct. But when I find Mr. Coleridge using it in support of his attack upon men of science, who, in a humane and lawful manner, are doing a necessary piece of distasteful work, of which Mr. Coleridge and I, and his and mine, and the whole community, may reap incalculable benefit, I must beg you to allow me to qualify it. For closing a cleft in a child's palate, and in certain grave operations on the tongue and throat, to have the patient in complete anæsthesia would be to invite disaster. But why should it be necessary to spread out these hideous things before the public gaze?—I am yours truly, EDMUND OWEN." To which you reply on March 17th, 1902: "Sir,—With regard to Mr. Edmund Owen's letter in the 'Times' of to-day, I asked him a plain question, and he gave me a plain answer. Now he says he wants to qualify that answer, and in qualifying it he states that he made it originally in the form he did for fear I should stop subscriptions to some ward they were building at St. Mary's Hospital! Mr. Owen's qualification of his original statement is that incomplete anæsthesia is employed in certain operations on the tongue and throat. The experiment of Dr. Crile I cited, and which took place under incomplete anæsthesia in Mr. Victor Horsley's laboratory, consisted in repeatedly crushing a fox terrier's paw. Mr. Victor Horsley and his friends are not likely to succeed in this controversy if they continue to enter the arena without previously consulting each other as to what they shall say.—Your obedient servant (Signed), STEPHEN COLERIDGE." Then you also had a correspondence with Mr. Timothy Holmes?—If you have got it there, by all means, if you will, read my reply.

10989. This is from the "Times" of March 17th, 1902: "Anti-Vivisection Methods.—To the Editor of the 'Times.'—Sir,—I see in a letter from the Hon. S. Coleridge in your paper to-day that he quotes from one which I wrote to him some years since. Mr. Coleridge is a complete stranger to me; but he has sometimes written to me about hospital matters, and the sentence which he quotes was in reply to a letter in which he seemed to be labouring under the strange delusion that hospital surgeons (for what purpose I know not, but I supposed him to fancy that it was for purposes of experiment) were in the habit of performing operations while their patients were still conscious of pain. Now, I was a hospital surgeon for more than a quarter of a century, and have performed hundreds and witnessed thousands of operations. I never heard of a case in which the patient was operated upon while conscious of pain, nor can I conceive why the surgeon should so act, and so I told Mr. Coleridge. I have, as I say, no knowledge of this gentleman, and have no right to speculate on his motives for the attitude he assumes towards our hospitals, but it seems to me a mistaken and a mischievous one. The researches carried on in our hospitals (miscalled 'experiments' by persons of Mr. Coleridge's views) have resulted in an immense saving of human life and human suffering, and anything that withdraws from our hospitals the public confidence and support appears to me a crime against humanity.—I am, Sir, yours faithfully, TIMOTHY HOLMES." I am afraid I have not got your reply with me.

10990. (*Chairman.*) Perhaps if we adjourn now you will be able to bring it next time?—Yes.

10991. (*Sir Mackenzie Chalmers.*) And will you bring any of these letters that you have got which we have not had?—Yes.

TWENTY-FIFTH DAY.

Wednesday, 3rd July 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).
Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary.*)

The Hon. STEPHEN COLERIDGE recalled; and further Examined.

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10991A. (*Chairman.*) I think when he finished the other day you had been asked about some correspondence between Mr. Timothy Holmes and yourself?—Yes.

10992. And you said you would probably be able to bring some letters to-day?—Yes, I have them all here.

10993. (*Sir Mackenzie Chalmers.*) I think I had read the letter of Mr. Timothy Holmes dated the 17th of March, 1902?—Perhaps you would like me to give you the results of my investigation at my office as to this correspondence. I have a very large number of letters. I have the whole correspondence here now; every letter that has passed between me and all these people. I have looked through all my books and letter books, so far as I can, and I have here a list of the eminent surgeons to whom I wrote, so far as I can make out:—E. Owen, T. Holmes, E. Nettleship, T. Bryant, T. W. Nunn, T. Bond, Professor Schäfer, Wm. Allingham, Jonathan Huthinson, Lord Lister, Sir H. Thompson, Mansell Moullen, Sir Victor Horsley, T. H. Openshaw, J. A. Kingdon, Noble Smith, Herbert Snow, M. F. Rees, G. D. Pidcock, J. May, T. J. Walker, G. W. Gosling, T. Walker, T. R. Jessop, J. A. Dearden, and S. F. Barber. I wrote to those myself. Of those Nettleship, Nunn, Allingham, Moullen, Sir Victor Horsley, Kingdon, Noble Smith, Rees, Pidcock, Gosling and T. Walker made me no reply. T. Holmes' reply was marked "private." I also find that I asked Lady Grove to write for me to Sir Frederick Treves, because she happened to know him; and I asked Lord Harberton to write to Walter Tyrrell, because he knew him; and I have the replies forwarded to me from those two gentlemen. I have all the replies now.

10994. All I was concerned with was, that some of the people you wrote to afterwards protested, rightly or wrongly, against the use you had made of their letters?—I do not think that is quite the way to put it. I have the correspondence here.

10995. I read Mr. Timothy Holmes' letter of the 17th March, 1902. Is there any reply to that that you wish to put in. I propose afterwards to read a letter of the 27th of March?—That I think is marked "private."

10996. I am only referring to a letter in the "Times"?—That cannot be marked "private."

10997. That is all I am concerned with. Do you wish to put in any reply to that letter of the 17th March?—Yes, I do. I wish to read my reply:—"To the Editor of the 'Times.' Sir,—Mr. Timothy Holmes is entirely inaccurate in stating that my letter to him suggested that hospital surgeons operated on their patients under incomplete anæsthesia, and I ask him either to withdraw this inaccurate statement or to publish my letter, which will disprove it. Although a very august authority had informed me that hospital patients were operated upon under incomplete anæsthesia—*i.e.*, before the corneal reflexes were abolished—I did not believe it, and wrote to Mr. Timothy Holmes, hoping that he would authorise me to deny it, which he did." The rest of the letter is on quite another subject; I do not know whether you want it introduced. That is all I say about Mr. Timothy Holmes.

10998. (*Chairman.*) You are speaking about vivisection, but not about this case?—It is on quite another subject. I am ready to go into it. I am answering two letters, you see, in my letter, at the same time.

10999. (*Sir Mackenzie Chalmers.*) Then a third letter came from Mr. Timothy Holmes?—Yes, on the 21st of March he writes to the "Times": "Sir,—I enclose a copy of the letter of Mr. S. Coleridge which he asks me to publish, and will leave your readers to judge whether I was inaccurate in saying that it suggests that hospital surgeons operate on their patients under incomplete anæsthesia; and then he enclosed my letter to him, which is as follows: "Dear Sir,—I have been recently informed on very high authority that surgeons now frequently perform severe operations on man under 'incomplete anæsthesia,' that is, while the eyelid still blinks when the eyeball is touched. As you may possibly be aware, I vehemently believe that animals are tortured in laboratories attached to our great hospitals, but I hesitate to accept this information charging our medical men with conveying the inhumanity of the laboratory into the hospital wards and operating theatre. I should be much obliged to you if you would tell me whether your experience leads you to endorse or deny this statement. Very faithfully yours.—Stephen Coleridge." My comment on that letter is that my letter in no way suggests for a moment that I believe it; on the contrary, I say that I do not.

11000. Then Mr. Timothy Holmes writes the following letter in the "Times" of March, 27th, 1902: "Sir,—I judge by your leading article to-day that you think you have given sufficient space already to this topic; but I hope you will find room for these few lines in reply (final as I hope) to Mr. S.

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Coleridge"?—I have not seen that; but I am quite ready to hear it.

11001. "Mr. Coleridge (see his letter, March 20th) charged me with being entirely inaccurate in stating that his letter—a copy of which you published on March 21st—suggested that hospital surgeons operated on their patients under incomplete anæsthesia." As a matter of fact, what I had said (see my letter of March 17th) was that he appeared to have laboured 'under the strange delusion that hospital surgeons were in the habit of performing operations while their patients were still conscious of pain.' And if this was not the meaning of his letter, what could it mean? It is quite true that he says he hesitates to accept the information given him on very high authority to this effect; but as he also says that he 'vehemently believes' that animals are tortured in laboratories attached to our great hospitals, and talks of 'our medical men conveying the inhumanity of the laboratory into the hospital wards and operating theatre,' it is clear that he did believe it. Else why write to me—a perfect stranger—and, as it now turns out, to many others equally strangers to him, to settle a question that any surgeon of his acquaintance could have answered at once? It is quite evident that he confounds 'incomplete anæsthesia' with sensibility to pain, and that he had discovered the mare's nest which I alluded to. However, quite enough has been said about Mr. Coleridge and his opinions. I fully agree with the writer of your leader, that it is easy to attach too much importance to them; and, as far as the progress of physiology and medical science is concerned, I daresay they are negligible quantities. But at the present time our hospitals are seriously hampered for funds, and the misrepresentations of the anti-vivisectionists tend to frustrate our efforts to secure what is wanted. Only the other day a gentleman thus replied to the appeal from St. George's Hospital, which you, Sir, have so kindly supported: 'It would be impossible for me to give to any hospital of which I know that part of its income is used, not for the relief of the poor, but for a purpose which I consider immoral and detestable—the vivisection of animals.' I answered this gentleman," and so on?—I think if that letter is read and considered by this Commission, I am entitled to defend my entire conduct as regards the diversion of hospital funds to schools licensed for vivisection.

11002. (*Chairman.*) I do not see any object in going into it while we are talking about Mr. Timothy Holmes, unless you consider that you are attacked in any way?—I am quite prepared to defend every action I have taken.

11003. (*Sir Mackenzie Chalmers.*) I only read it for the purpose of showing that Mr. Timothy Holmes protested against the use made of his letters?—My answer to that, of course, is that I did not answer that letter; it did not need an answer. It is self-evident that he is wrong. I had never in my letters suggested anything of the kind that he said I did. On the contrary, I said in my letter distinctly the opposite, and asked him to confirm my opposite view.

11004. At any rate, he differed in effect as to that?—There can be no difference as to what English means. My letter meant one thing; he said it meant another.

11005. (*Chairman.*) I cannot accept that general proposition, I am afraid, upon a subject in which lawyers find great difference?—I fall back on Bishop Butler, who said "Words mean what they do mean, and not other things."

11006. He did not solve the question what they do mean?—No.

11007. (*Sir Mackenzie Chalmers.*) Probably you make an exception to your rule in favour of Acts of Parliament?—Well, that is rather sweeping.

11008. Then you read part of the Home Office letter to us last time. As the foundation of your letter to these surgeons, you read part of the Home Office letter of the 2nd September, 1899. I would rather have the whole letter on the notes?—My letter to the Home Office, do you mean?

11009. No; a letter from the Home Office to you. I would rather have the whole letter on the notes?—Certainly.

11010. You read a portion of it?—Did I? What is the date of it?

11011. Whitehall, 2nd September, 1899. It is in reference to Dr. Crile; you read a portion of it. Mr. Cunynghame wrote to you—"Sir, With reference to the sixteen experiments which were performed in this country by Dr. Crile, and to which you call the attention of the Secretary of State in your letter of the 18th May last, I am instructed to say that before these experiments were sanctioned the Secretary of State was advised that they would be wholly free from pain, and that their object was to study the phenomena of shock after surgical operations in spite of anæsthesia. Their object, therefore, would have been frustrated if the animals had been allowed to suffer pain. On receiving your letter, the Secretary of State caused investigations to be made into these experiments, and the information he has received from competent persons, who either witnessed most of the experiments or have read the account of them, is to the effect that in no instance had the animals experimented upon been subject to pain. At the same time he caused inquiry to be made of Dr. Crile, from whom the strongest assurance has been received that 'in the entire series of experiments no animal suffered pain.' The use of the term 'incomplete anæsthesia' has probably given rise to the misunderstanding. With regard to this, the Secretary of State is advised that there are various degrees of anæsthesia. In 'complete anæsthesia' the narcosis is very profound, and the 'reflex movements,' that is, the involuntary movements in response to an external stimulus, are suppressed. One of the last of these reflex movements to disappear is the blinking of the eyelids when the surface of the eyeball is touched, thence called the 'corneal' or 'conjunctival reflex.' But short of this is the condition termed by Dr. Crile 'incomplete anæsthesia,' in which the creature is quite insensible to pain, although the corneal and other reflexes can still be obtained. It is quite usual for even severe operations to be performed on man in this state of 'incomplete anæsthesia.'" (I read every word of this so far.) "Indeed in many cases complete anæsthesia could not be induced without great danger to life during the operation. But absolute freedom from pain exists, and the patients are wholly unconscious in this 'incomplete anæsthesia.' Under these circumstances the Secretary of State can find no reason to think that any offence has been committed against Section 3, Subsection (3) of the Act, which requires that the animal during the whole of the experiment shall be under the influence of some anæsthetic of sufficient power to prevent its feeling pain. I am, at the same time, to return the copy of Dr. Crile's work, and to thank you for having temporarily placed it at the disposal of the Secretary of State.—I am, Sir, your obedient servant. (Signed) Henry Cunynghame." I read the whole of that letter last time.

11012. Did you? I thought you only read an extract from it?—No, I read the whole of it, so far as I am aware. I had better read on now the whole of the correspondence, I think. To that a reply was sent from my office on the 16th September, 1899. This is written by myself.

11013. I never found the last part of that letter. I thought you stopped at the word "obtained"?—I may have done so; I do not think so.

11014. (*Dr. Gaskell.*) It is at No. 10,301?—The whole point is that last sentence on which I base my letter to the surgeons.

11015. (*Chairman.*) It is no good reading more letters about a matter which has been already passed?—(*After referring.*) I think you are right. I seem to have left out the important item of it.

11016. (*Sir Mackenzie Chalmers.*) You extracted the part referring to these medical men?—Yes, no doubt.

11017. Looking at it now, do not you think you would have saved correspondence with these men if you had set out rather more fully the Home Office letter?—I gave them the actual words: "It is quite usual for even severe operations to be performed on man in this state of 'incomplete anæsthesia'; and I gave the definition that you give me, namely, that the corneal reflexes were abolished.

11018. (*Chairman.*) What is the passage that was omitted in the letter that was sent to these gentlemen?—I shortened it a little, that is all.

11019. (*Sir Mackenzie Chalmers.*) You shortened it by leaving out "Indeed in many cases complete anæsthesia could not be induced without great danger to life during the operation, but absolute freedom from pain exists, and the patients are wholly unconscious in this incomplete anæsthesia." Those are the words which we should like to have seen in your letter to the surgeons?—I quite admit that. That is what you were

told by your advisers. I was not in the least inclined to place that advice before these surgeons. I wanted their independent view; I did not want their replies to be biassed by this advice from the Home Office. I placed the case naturally before them without any bias.

11020. You took a passage away from its context, and rather, perhaps unintentionally, altered the meaning of it?—No, I do not think so. That is only your advisers. It is a complete statement: "It is quite usual for even severe operations to be performed on man in this state of incomplete anæsthesia." That is the question I want to know, whether it was true or false according to the advice of these great surgeons.

11021. That sentence is very materially affected by its context in the reply to you, is it not? It is the introduction to what follows afterwards?—I think the only context which has any weight with it is "What is incomplete anæsthesia," and I used your advisers' definition of it, namely, "before the corneal reflexes are abolished." That is the only qualification necessary for getting an independent answer.

11022. I am not quite sure that I agree. The correspondence speaks for itself, I think, now?—Yes, I quite agree. I should like to go on with it.

11023. (*Chairman.*) With what?—With this correspondence; I do not want it stopped in the middle.

11024. (*Sir Mackenzie Chalmers.*) You quoted the letter, and all I wanted was the whole of it.

11025. (*Chairman.*) We may be going to inflict upon ourselves a great deal of correspondence that is not very material. We have had the letter that you wrote to the surgeons, and we have had their answers. You are now being asked questions as to whether certain of these gentlemen either had not the proper material before them or gave answers which contained passages which modified your view of their answers. That correspondence bearing on those points?—This is the correspondence between myself and the Home Office that I am alluding to at the moment. It is a letter that I wrote in answer to that to the Home Office. I do not press it.

11026. I do not know what it is?—I do not know either; I have not read it.

11027. The correspondence between you and the Home Office may be material. At the same time, I hope you will not read anything that is merely controversial correspondence?—It is on this question of incomplete anæsthesia, which is the whole point of this dispute between myself and the Home Office.

11028. But in the letter which has just been read they state the view of their advisers. The Home Secretary himself does not profess to know anything if he is not advised. We have your view about it?—This next letter shows what the views of those advisers are out of your own report, in which they define incomplete anæsthesia and complete anæsthesia themselves; and I thought it was *ad rem*: "Sir, I showed your letter of the 2nd September to Mr. Coleridge yesterday at his country house, where he, unfortunately, is laid up, and he desires me to point out that the distinction you now draw between 'complete' and 'incomplete' anæsthesia cannot possibly be unknown to Dr. Poore, the Government Inspector. Yet that official makes the following statement in his last report: 'In experiments performed under the licence alone, or under certificate C, the animal suffers no pain, because complete anæsthesia is maintained from before the commencement of the experiment until the animal is killed.' Mr. Coleridge therefore instructs me respectfully to inquire what is now the rule of the Home Office. Is incomplete anæsthesia permissible under licence alone, as you state in your letter under reply, or is only complete anæsthesia permissible under licence alone, as Dr. Poore states in his report? The other matters contained in your letter will have Mr. Coleridge's attention when he is better."

11029. I do not understand that Dr. Poore in that letter was drawing any contrast between complete and incomplete anæsthesia?—Yes, he says so most distinctly. He uses the words "complete anæsthesia," and defines it.

11030. How does he define it?—He says "The animal suffers no pain, because complete anæsthesia is maintained from before the commencement."

11031. That means that complete anæsthesia does produce it, but he was not contrasting complete with incomplete there. That was not the point he was on?—No. *The Hon. S. Coleridge.* 3 July 1907.

11032. Supposing he had used the words "The animal was in a state of complete insensibility to pain; that probably would have been expressing what he meant there. If you asked him to define the difference between complete and incomplete anæsthesia, and asked him whether the animals were always under complete anæsthesia in the strict sense of the word, or sometimes under complete anæsthesia without suffering pain, he might have given a different answer?—I think I am entitled, when a Government Inspector who is their adviser uses the phrase "complete anæsthesia" as being his way of defining a condition without pain, to deduce that when he uses the term "incomplete anæsthesia," he is defining a condition not without pain.

11033. He does not use it as a definition, I think. He says that complete anæsthesia includes a state of being without pain?—I do not think he says that.

11034. That is what it comes to. I do not think he is using it as a definition. We seem to be on very minute points with these verbal criticisms?—Very well, my lord.

11035. (*Sir Mackenzie Chalmers.*) I think I asked you whether you were aware that Dr. Crile's experiments were most of them seen by Dr. Goodbody?—You told me so. I was not aware of it before.

11036. Did you see a letter from Dr. Goodbody in "The Times" of the 13th March, 1902, who was present at the experiments, and gave his account of them?—No, I did not—not that I remember. I have something to say about Dr. Crile before I pass away from him.

11037. By all means say it?—I understand that one member of the Commission has a letter from other people, and has received evidence from other people who were present, all affirming that no pain was inflicted on the animals on this occasion. All I wish to point out to the Commission is this: There is no doubt that Dr. Crile in his own book admits that when he applied flame to the dog's paw, as he said, "The animal struggled on the application of the flame." That is a very definite statement by him.

11038. Was that one of the English experiments?—No, I quite admit that it was not an English experiment, so far as I know. It is not one of the first sixteen.

11039. (*Chairman.*) Then it is not one of those experiments about which you complained to the Home Office?—No; but I am only pointing out this, and I think it is *ad rem* in this way, to show that Dr. Crile is a person who does not hesitate to inflict what in my opinion is torture upon animals. He applied flame to the dog's foot, and the dog struggled on the application of that flame. I call that torture. There you have it under the man's own hand, the moment he gets out of England, that he does a thing which is undoubtedly torture of a most gross kind on an animal. I say under those circumstances, the whole book being before the Home Office, when they received that letter of excuse they ought to have taken into consideration what he did out of England.

11040. Had they got the book before them?—Yes, I placed that very experiment before the Home Office myself, and drew attention to the expression "flame was applied to the dog's paw."

11041. (*Sir Mackenzie Chalmers.*) When you called the attention of the Home Office to it, it was some time afterwards, and Dr. Crile had long left England?—Yes.

11042. What do you suggest that the Home Office could have done?—But my letter to you was before you consulted Dr. Crile as to what he had done; therefore, when you received his excuses you knew that he was a man who did torture animals, and his excuses, according to your own showing last time, were wholly incorrect, because he made a statement in the letter which you produced that was wholly false, showing that his evidence is not to be relied upon—first, because he was a man who did torture animals; and, secondly, because he was a man who, in his letter of excuse, was not a man incapable of making false statements.

11043. I should agree with you if Dr. Crile's evidence had not been confirmed by independent witnesses whom we have every reason to respect—Sir Victor Horsley

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and Dr. Goodbody?—I only wanted just to get that out.

11044. Then I called your attention to Dr. Noel Paton's experiments on fasting, and perhaps it would only be fair if I read his own explanation in a letter written to Sir James Russell in response to a question as to how and why he performed these fasting experiments: "Dear Sir James Russell,—I suppose the experiments referred to by Mr. Thane in his letter of the 14th inst. are those published in 'The Journal of Physiology,' vol. xix., p. 192, *et seq.* (1895-96)." That is what you quoted from?—Well, Sir Mackenzie, if these letters are to be put in against me, I ought to have had them before I came here. It is very difficult for me to conduct my case from my point of view if things are thrown at me without my knowledge. This is a private letter, apparently, from Dr. Noel Paton to the Home Office. How can I deal with it without having it before me?

11045. (*Chairman.*) It is quite unusual for witnesses to be furnished with all the letters to be put to them. You must do the best you can, and we shall do the best we can?—It is very difficult for me, is it not?

11046. (*Sir Mackenzie Chalmers.*) "The experiments there described are upon the influence of fasting on the fats of the liver. I have always been most reluctant to inflict suffering upon animals, and before performing these experiments I gave them most careful consideration. I had already in 1888 studied the progress of a fast of thirty days undertaken by a Frenchman, Jacques, the results of which are published in the proceedings of the Royal Society of Edinburgh in 1889. I found that Jaques throughout his fast suffered no pain or distress—except for a curious attack of gout in the middle of the period."

11047. (*Mr. Tomkinson.*) He had water to drink, I suppose.

11048. (*Sir Mackenzie Chalmers.*) Yes, certainly he had. "I had also read the accounts given by Professor Luciani of a similar feat undertaken by an Italian, Succi (Das Hungern), and the descriptions of the fasts of Dr. Tanner in America and of Cetti in Berlin. In none of these did the abstinence from food, though extending over prolonged periods, cause inconvenience or suffering. Further, it is well known that many warm-blooded animals undergo prolonged fasts voluntarily and without suffering. Among mammals the male fur seal, I am informed, remains on shore in the breeding season sometimes for one hundred days without eating. Among cold-blooded animals very prolonged fasts are undergone without suffering or impairment of function, *e.g.*, in the salmon, in fresh water. Food is simply required as a source of energy, and so long as the animal body is not depleted of its stored material there is no necessity of a supply of food from without. From these considerations it seemed evident that the experiments I proposed to perform were not in any sense painful, especially as the animals were kept warm and supplied with water throughout the period. I therefore did not think that they could be brought under the Act, and I did not apply for a certificate. Indeed I do not know what certificate would have covered them. For the purpose of my experiments more prolonged fasts were desirable, but my repugnance to inflicting even possible discomfort upon animals prevented my extending the period beyond the limit of four days. No one has a greater love of the lower animals than I have, and all through my physiological career I have never willingly inflicted pain. I am, yours very truly, (Sd.) D. Noel Paton." That is his explanation, whether you accept it or not?—I understand the case to be that Dr. Noel Paton said that he did not consider that these experiments were within the four corners of the Act at all, and the Home Office accepted that explanation. My answer is that if any one of the Commissioners starved his horse for four days he would be summoned, and properly so, by the Royal Society for the Prevention of Cruelty to Animals, and probably convicted and fined. The law of the land would call it cruel, and any magistrate in the country would call it cruel. Before you pass on there are two or three things which were left open when I went away last time, and I should like to deal with those if I might. You put to me, for instance, at Question 10,914 a question on the Association for the Advancement of Medicine by Research.

11049. Yes?—"Each licence and certificate goes to them. Are you aware what happens when they come back? Are you aware that certificates are continually altered and modified and sometimes refused?" and I say, "My point is that so far no evidence has been offered here before this Commission to show that the Home Secretary has ever gone contrary to this Association till the Commission was sitting." Then Sir William Collins, at Question 3,873, asked Mr. Starling: "Do you know whether the Home Secretary ever acts contrary to the advice of the Association?" and Mr. Starling says: "He has done so lately, I understand"; and it continues: "(Q.) In how many cases?—(A.) He has in two cases lately refused to allow certificates. (Q.) During the present year?—(A.) During the last three weeks." That is what I based my assertion upon. I based it upon that evidence (I could not find it at the moment); and it is upon that evidence that I said my point was that no evidence had been offered "before this Commission to show that the Home Secretary has ever gone contrary to this Association till this Commission was sitting." The three weeks was within the period of the sitting of the Commission. I also want to point out before I leave this that Professor Schäfer himself, I see, in his evidence at Question 10,172 admits that the confidential reference to this Association in his own words is "clearly outside the four corners of the Act," though he states very naturally that he does not object to it himself. The next point is that there was a question and answer about Professor Schäfer drowning and redrowning dogs, which I could not find at the moment. I have found it and have it here; it is in "Hansard" of the 29th June, 1903. You will find the allusion to this under Question 10,925, etc., down to No. 10,928. It is as follows: "Colonel Lockwood: To ask the Secretary of State for the Home Department whether the certificate given to Professor Schäfer, in accordance with the provisions of the Act 39 and 40 Vic., c. 77, permits him partially to drown and then resuscitate the same dog without any anæsthetics more than once; and if so, how many times; or permits him partially to drown and then resuscitate the same dog without any anæsthetics only once; and if so, how many dogs has he permission so partially to drown and resuscitate." That is the question. Then Mr. Secretary Akers-Douglas answered thus: "The certificates obtained by Professor Schäfer for the purpose of testing the best means of effecting resuscitation in cases of apparent drowning are limited to ten experiments. It is not stated whether the experiments shall be on ten separate dogs, or whether the same dog will be used for more than one experiment; but I have ascertained that Professor Schäfer does not propose to submerge any unanæsthetised animal more than once." That is the question and answer I had in my mind when I said that the Home Office had permitted the drowning and redrowning of the same dog, but that I had heard with pleasure that Professor Schäfer said he had not done what he had been permitted to do.

11050. There was another question and answer on the same subject. Mr. Perks asked a question on the general question of Professor Schäfer's drowning experiments. Have you got that?—I am very pleased to hear it. I do not remember it.

11051. "Mr. Perks: To ask the Secretary of State for the Home Department whether, as reported at a recent meeting of the Committee of the Royal Medical and Chirurgical Society, Professor Schäfer has obtained permission to conduct experiments on dogs without anæsthetics for the purpose of investigating the phenomena of death from drowning; is he aware that these experiments have hitherto been conducted under chloroform; and will he take steps to prevent the infliction of cruelty which the experiments authorised involved"?—Was this after the other one?

11052. No, this was before the 15th June, 1903?—About a fortnight before.

11053. And Mr. Akers-Douglas in reply said: "Professor Schäfer has obtained the certificates required by the Act to enable him to perform certain experiments on dogs for the purpose of investigating, not the phenomena of death from drowning, but the best means of effecting resuscitation in cases of apparent drowning. The experiments will be performed on behalf of the Royal Medical and Chirugical Society of London, who have appointed a committee to consider this question. I am aware that Professor Schäfer has performed experiments on the effect of immersion on anæsthetised dogs, but I am informed that the use of

anæsthetics frustrated the object of the experiments. In view of the great importance of the subject in connection with the saving of human life, and of the strong recommendation received in support of the experiments, I have not felt justified in disallowing the certificates"?—The next point I want to point out is this. I could not find the number, Volume XV., of the Journal of Physiology. At Question 10564, Lord Selby asked: "You spoke of keeping the animal under anæsthetics first while it continued lying on the table," and so forth. "And then, in another place, you added the words 'until the wound has been aseptically closed.'" I say "I have not got the volume here now, I am afraid." I have got it now. This is an experiment by G. N. Stewart: "Experiment 2. Bitch, 3,708 grams. 50 mg. curare subcutaneously. Artificial respiration. Exposed right internal jugular vein, right carotid artery and right vago-sympathetic nerve, below thyroid gland —carefully separated right vagus from sympathetic for some distance. Cannula in left external jugular vein." There is no specific mention of anything in the way of anæsthetics, but 50 milogrammes of curare subcutaneously injected. This experiment is described as beginning at 2.2 in the afternoon, and it goes on till 8.14, and the point I want to make is this. There are curious notes all along down the side; I should like the Commissioners to look at this. It has been going on since 2 o'clock, and at 6.31 there is a note at the side "Curare action passing off; twitching of muscles." It does not say anything more was done to the animal to stop those twitchings or to allay pain, which I presume was beginning to be severe, or was severe all the time for all we know; but at 6.53 "injected 25 mg. curare into blood"—no doubt to keep the animal still.

11045. (*Colonel Lockwood.*) Where was this experiment?—I do not know where it was. It was done by G. N. Stewart, a licensed vivisector. He wrote several accounts of these experiments, and the difficulty is that he puts at the heading of this paper, which contains a great number of them, "From Physiological Laboratories, Cambridge and Strasburg." As he does not say which were done at Cambridge, in England, and which were done at Strasburg, if we convict him of cruelty—I do not say we do, but supposing he is convicted of cruelty in any one of these experiments, it is very easy to say that the pea at that time was under the Strasburg thimble, and not under the Cambridge one.

11055. (*Dr. Gaskell.*) If you look at the end of that paper, I think you will see a statement by Dr. Stewart on that subject?—Yes. "The above paper is based upon experiments made by the author while holding the George Henry Lewes Studentship in the Physiological Institute at Strasburg. The results were embodied in a report to the Government Grant Committee of the Royal Society, dated March 22nd, 1892. The paper formed part of a thesis sent in for the Goodsir Memorial Prize in the University of Edinburgh, April 30th, 1892, of which an abstract was read before the Physiological Society, June 25th, 1892." He says it again. "The experiments in the above paper were made," and so on. "In all the experiments performed in the Physiological Laboratory at Cambridge, and in most of those done in the Physiological Institute at Strasburg, the animals were anæsthetised with chloral, morphia, urethan, chloroform, or ether. When curare was given, it was generally in addition to one of these anæsthetics"—generally, not always.

11056. And in all the experiments in Cambridge the animal was anæsthetised?—No. "In all the experiments performed in the Physiological Laboratory at Cambridge, and in most of those done in the Physiological Institute at Strasburg, the animals were anæsthetised." What with? "Chloral, morphia, urethan, chloroform, or ether. When curare was given" (this holds with all of them, I presume) "it was generally in addition to one of these anæsthetics."

11057. (*Chairman.*) But if that statement is taken as a whole, it means that it was never administered without anæsthetics at Cambridge?—I quite admit that it is impossible to prove that any one of these experiments was not done at the Physiological Laboratory at Strasburg, but my point is that anybody who reads the account of this experiment through would think—I do not set up to be a physiologist, but it seems to me, and I have been advised (just as much as the Home Office has been advised)—that this is a most dreadful experiment. There are two points that I want to make on this experiment. My chief point is that, even if this were done (as it was admittedly done somewhere) at Cambridge, this could be done under Certificate B with perfect legality. That is one point which I wish to raise, because Certificate B permits animals to recover from anæsthesia so soon as the initial operation of the experiment has been performed. The initial operation here is carefully described, and after that there is no further operation upon the animal, so far as I can see by the notes at the side, from 2.2 to 8.14. We do not know whether the animal died, or what happened.

11058. (*Colonel Lockwood.*) I want to know whether curare was administered, according to the paper, alone, or in conjunction with anæsthetics?—According to this specific statement, there is no mention of any other anæsthetic, but there is this general statement, which seems to cover them all, in a sense. I admit it is a covering statement.

11059. (*Chairman.*) The statement is that if it was done at Cambridge it was done with an anæsthetic? —No, I say it need not have been so.

11060. Unless you say he is not speaking the truth; because he says expressly that in every case anæsthetics were used at Cambridge?—Yes, no doubt it was for the preliminary operation—the initial operation.

11061. Therefore, if he says that in some of the whole mass of cases curare was administered, then that means that in all the Cambridge cases, wherever curare was administered, it was after the anæsthetic; because he says that in every case at Cambridge there was an anæsthetic?—If he says that an anæthetic was used in all the experiments at Cambridge, I am not prepared to deny it. But I say that if he had Certificate B, as I presume he had, he was only bound to use anæsthetics during the initial operation.

11062. That is not the point. I was speaking of Colonel Lockwood's point. He wanted to know whether curare was used without an anæsthetic. I say that if curare was used at Cambridge, the statement is that an anæsthetic was used with it?—No, not necessarily during the period of observation.

11063. I am speaking of one operation, which is all he is speaking of. I do not know whether he says anything about using one or other anæsthetic during the period of observation?—That is the point.

11064. (*Dr. Gaskell.*) Were not these experiments done under licence? There is no Certificate B, so far as I know?—I do not know.

11065. (*Chairman.*) You seem to me to be making too much of this, and trying to get more out of his statement than it will bear?—No, my Lord; I am upon this: that Certificate B, as now issued by the Home Office, permits of any amount of torture, and I cite this as an instance.

11066. I was on what Colonel Lockwood put to you, and you answered it by giving an answer which seemed to me to relate to another matter?—To resume, what I mean is this, that this experiment was done either at Strasburg or Cambridge; if done at Cambridge, if he had Certificate B, it is perfectly illegal.

11067. (*Sir Mackenzie Chalmers.*) But had he Certificate B?—I will look and see.

11068. (*Mr. Tomkinson.*) Is your contention that if the word had been "invariably" instead of "generally" that would have covered it?—No, because when he said that he used anæsthetics that is perfectly true as regards his statement, because he would use it for the initial operation if he had Certificate B.

11059. But he says that when curare was administered it was generally in addition to an anæsthetic. If the word "invariably" had been used, it would have satisfied you that curare was never used without an anæsthetic?—During the initial operation only.

11070. (*Chairman.*) You are losing sight of the other part of what he says—that anæsthetics were always used at Cambridge. If this was done at Strasburg, that is another matter; he may have used curare without anæsthetics. But if it was done at Cambridge, it was an operation under the Act, and there must have been anæsthetics?—I am not citing it for that point. I cite it as an illustration of my contention that under Certificate B any amount of torture can be inflicted on animals in England under the present law.

11071. That is not immaterial at all, but it is not the question in point?—That is what I brought it up for.

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(*Dr. Gaskell.*) That is a question which we shall come to afterwards.

(*Witness.*) My next point is this. Since I was here I have placed my Bill once more before my committee, and I have incorporated the various little alterations that were suggested. For instance, I have incorporated in Clause 3, about inserting matters into the eye, these words: "There shall not be applied to the eye of an animal by way of experiment any matter or substance." Then I put the words "calculated to give pain," which brings it up to and makes it more what I meant it to mean. Then also, in deference to something that was said, that in Clause 6, that the names of the gentlemen who signed the certificates would not necessarily by law be published, I have inserted, after the words "Secretary of State," in the third line, "and three copies of each of the two certificates hereinafter mentioned made in support of the application for the said licence if so granted"; and also in line 31, after the word "reports," "and certificates" should be deposited. That incorporates what Sir Mackenzie pointed out to me was an omission in the Bill. I laid it before my committee with those alterations on Monday, and they passed it with their entire approval. Then there was an allusion to the picture of a dog at Khartoum. Sir Mackenzie said he had not seen it; that is at Question 10842, so I have brought it here (*producing the same*).

11072. (*Sir John McFadyean.*) It was not done in England?—No, at Khartoum, under English Government.

11073. (*Sir Mackenzie Chalmers.*) The dog suffering from sleeping sickness?—Yes.

11074. (*Mr. Tomkinson.*) But that is put in as typical of the effects of inoculation, and as a case which may occur equally well in England?—Yes.

11075. (*Sir John McFadyean.*) Might I ask whether that picture is exhibited in order to convince the Commissioners that the dog must have suffered pain? Do you wish us to accept that as evidence that the dog must have suffered pain?—I should not care to have any dog that belonged to me reduced to that condition.

11076. That is not the question?—I should say that it was a condition of suffering, certainly. To the ordinary lay eye it is; it may not be so. To the ordinary lay eye it looks like a dog in a condition of suffering. I think that a man in England who reduced his dog to that condition outside his laboratory would be prosecuted by the Royal Society for the Prevention of Cruelty to Animals successfully, if the dog was seen in court.

11077. But, I suppose, as a layman, you would admit that there are absolutely painless diseases which reduce dogs to such a state of emaciation as that?—That is a question for an expert. I do not know. I daresay there are, if you say so.

11078. I ask whether you suggest the contrary?—I should myself say, as a layman, that evidence might be brought before the court that it was a perfectly painless condition, by experts. That would be for the judge and jury to decide.

11079. (*Dr. Gaskell.*) Is not that the characteristic of sleeping sickness—emaciation without pain?—That I do not know.

11080. (*Sir John McFadyean.*) So that it does not, as it stands, prove that this animal suffered?—I do not say it does.

11081. It is a perfectly reasonable supposition that this dog suffered no pain, in spite of the emaciation?—It seems always the supposition that any animal vivisected for anything suffers no pain. It is a supposition, I suppose, that can be accepted for purposes of argument.

11082. (*Sir Mackenzie Chalmers.*) With reference to your statements about Sir Victor Horsley, are you aware that he applied to the Home Office to be allowed to perform certain experiments with regard to disease caused by *filaria sanguine hominis*?—How could I possibly be aware of that?

11083. Which only arise at nighttime; and he asked, therefore, to be allowed to experiment outside laboratory hours?—You put that in the form of a question to me, but you know perfectly well that I know nothing about it.

11084. You complain of the permission being given, and I was asking if you knew the reason why it was given. You do not know?—Of course, I do not.

11085. (*Sir William Collins.*) Have these applications and the permission given thereon been published in any official return?—No, none.

11086. (*Sir Mackenzie Chalmers.*) You know the fact, but you do not know the circumstances which lead to it?—I am perfectly indifferent, and so are those whom I represent, as to what reasons may be assigned on which Sir Victor Horsley asked to be permitted to be placed outside the provisions of the Act. My complaint is that Sir Victor Horsley was given permission to vivisect in a place outside the region of inspection, and therefore outside the Act altogether, which contemplates that every place should be inspected.

11087. Are you aware that Mr. Asquith attached a condition to that leave that Sir Victor Horsley should report to the Inspector on each occasion when he performed any experiment at any but a registered place?—No, I am not aware of it.

11088. If you are not aware of it, why complain?—I have stated my complaint, and will state it again. It is immaterial to us, or to anybody whom I represent, for what purpose Sir Victor Horsley was placed outside the purview of the Act. Our contention is that no vivisector should be placed outside the possibility of inspection, and that the Act of Parliament did not contemplate anything of the kind, and did not intend it to be so.

11089. (*Chairman.*) Do you say that it is contrary to the Act?—We say that there is no reason in the world why Sir Victor Horsley should be placed by the Home Office outside the possibility of inspection. In the case of persons allowed to place grouse on a moor for the convenience of experiments, that is a different thing.

11090. This was not a case of that kind?—No, I understand that it was not.

(*Sir Mackenzie Chalmers.*) This was an inoculation experiment.

11091. (*Sir William Collins.*) Are the endorsements or conditions of a licence published in the official returns?—Never; the Act is administered, if I may say so, in the strictest secretary.

11092. (*Sir Mackenzie Chalmers.*) Then you raise a point as to Professor Schäfer's certificates. I can only put it to you for the purpose of clearing it up. You applied to the Home Office to know by whom Professor Schäfer's licence was signed?—Yes.

11093. I do not know that it is very material. May I put it to you in this way: That his application for a licence was signed by Sir Joseph Hooker and Sir John Burdon Sanderson, as you said?—That is a new name; I never heard of Hooker.

11094. And Burdon Sanderson?—The Home Office said that Sir John Burdon Sanderson signed his certificate.

11095. That is so?—That is my statement.

11096. Then his lecture certificate, Certificate C, was signed by the same people. Then he afterwards held various certificates from various people for special researches?—My contention was that it was impossible for the public to ascertain by any means published by the Home Office who signed these certificates. When we tried to investigate it the Home Office seemed confused, and could not tell us. The Home Office said one thing, and Professor Schäfer said another. The two statements are contradictory.

11097. They are not contradictory. One referred to Certificate B and the other to Certificate C?—There is a fourth man in now, Hooker. I never heard of him before.

11098. You objected to Sir William Harcourt asking the advice of the Society for the Advancement of Medicine by Research?—I do not put it upon Sir William Harcourt. It was done in his time.

11099. At any rate you objected to it?—Yes.

11100. I want to ask you this: Do you think it would be better if, say, a committee of the Council of the Royal College of Physicians was substituted for that society?—That is a very large question. I am not the adviser of the Home Office.

11101. I am asking you?—I am making the com-

plaint. If you ask me to say at a moment's notice I decline to give an opinion.

11102. I ask you whether, in your opinion, it would give a greater sense of security to you and your society?—The sole reason why all those who sign certificates sign them, so far as I understand it, is as testimonial to the scientific fitness of the applicant. Their scientific fitness is of no interest whatever to myself or my Society. Our interest is in the animals and their fate; and it is a matter of perfect indifference to us whether these gentlemen have scientific fitness or not.

11103. But the Society for the Advancement of Medicine by Research do not sign certificates; they are merely consulted. I am asking you whether you would have more confidence, for instance, in a committee of the Royal College of Physicians or a committee of the Royal Society of Medicine, if they were consulted in lieu of the Society for the Advancement of Medicine by Research?—I should have much more confidence if any body was consulted upon which there were no licensed vivisectors sitting. This society that you do consult is a society composed very largely of licensed vivisectors.

11104. (*Chairman.*) Then you would not approve of the Royal College of Physicians?—Not if the persons sitting on that committee to give that advice were licensed vivisectors.

11105. (*Sir Mackenzie Chalmers.*) But if you had a committee of the Royal College of Physicians who were not licensed vivisectors?—I should certainly regard it with greater respect. I think they would very likely take into consideration the humanity of the applicant. But even then they are not asked that. I have never had any evidence before me that the Home Office ever ask the persons to whom they refer what the humanity of the applicant may be.

11106. It is not the Home Office that grants certificates. The certificates are granted first, and the only question is whether the Home Office shall disallow them?—Yes; you can veto them.

11107. However, that suggestion does not approve itself to you?—I do not say that.

11108. Is there any body with expert knowledge that you can suggest in preference to the Society for the Advancement of Medicine by Research?—I should say that any body is fitter than the Society for the Propagation of Vivisection, which is really a better term for them.

11109. (*Colonel Lockwood.*) You have got no definite body in your mind at the present minute?—No, I am not interested in the scientific side of the matter. These men only testify to the scientific fitness of the person. If you ask me who are the right persons to ask as to the humanity of the applicants, I think that a committee of the National Anti-Vivisection Society would be a very proper body. It is just as public a body as this private association is, and just as able to give proper consideration to the humanity of the applicants as that other body is to consider their scientific fitness.

11110. (*Sir Mackenzie Chalmers.*) You raised the point, and it is a material point, about prosecutions under the Act. At present, as you know, all summary prosecutions are limited by six months?—Yes, by the Summary Jurisdiction Act.

11111. You suggest a longer period?—I suggest it, because, as you are aware, at present the yearly report, which is one of our sources of information, seldom comes out till about three or four weeks before that six months has elapsed; so that there is no possibility that a prosecution can be based upon that report, even if it revealed any breaches of the law. And, again, this short six months precludes us from taking advantage, as a rule, of any publications of the vivisectors, if by a slip they let out that they have broken the law, because they do not, as a rule, publish them until six months after the experiments.

11112. But it would, of course, be applying a special law to vivisectors which is not applied to the rest of the community; it would be an exception to the general law?—I do not see why. There is a definite reason that I give you, which, I think, is a reasonable one, and I think anybody ought to see is a reasonable one. I do not imagine that even the Home Office want to place vivisectors beyond the possibility for indictment for an offence under this Act.

11113. Certainly not?—I am not sure that putting them under the Summary Jurisdiction Act is not an exception in their favour.

11114. They are not put under it. It applies universally, does it not?—I do not know.

11115. As regards the prosecution of licensees, you do not think it is the duty of the Home Secretary first to give leave to prosecute, and then himself prosecute?—I think the provisions of my Bill are more desirable.

11116. But under the existing law you would hardly contend that the Home Secretary should give himself leave to prosecute, and then himself prosecute?—I think he should prosecute in cases where the law is broken.

11117. When he has given the licence?—Yes.

11118. Inasmuch as under the Act the prosecution requires his leave, do you think that the Act contemplated that he should give himself leave and then prosecute?—I should have thought it was possible for the Home Office to move in prosecutions. I should have thought that they could direct a prosecution against the man. I should be surprised if they cannot.

11119. Under that Act, wherever the Home Secretary has to give leave, do you think the Act contemplates that he should himself prosecute?—Why not? I see no reason agains it.

11120. That he should simply say, "I hereby authorise you, Herbert Gladstone, to prosecute," and then proceed to prosecute?—That is mere formality.

11121. I will take your answer upon that?—It would not be a formality which would frighten me if I were Home Secretary.

11122. You are aware that the Home Office has no legal department whatever?—I daresay.

11123. In how many cases have your Society asked for leave to prosecute? Are you aware of any case?—Not since I have been there, of course, because there was no possibility to get material for prosecution under the present law.

11124. You have not applied?—Naturally; how could we get the material to prosecute?

11125. There is only one small point I want to ask you about as to Dr. Grünbaum's experiment. Do you know that he was experimenting at that time under his certificates with antivenin?—How could I know that? That is your evidence. It is not a question to me.

11126. I am asking you whether you do know it?—Of course I do not.

11127. Was not all that happened this: that after a lecture in his own laboratory he went on with an experiment he was conducting, in the presence of certain chemists?—That may be your version of it. I was not there. That is not the account which was published in the papers.

11128. And, apart from any technicality, what is your objection to these chemists seeing the experiment?—My personal objection, you mean?

11129. You make it a ground of complaint?—I am here in an official capacity.

11130. I say (identifying you with your Society), is it not better, if experiments are performed, that they should not be performed wholly in secret?—Unfortunately, when I complain of that (because at present we are under the Act 39 and 40 Victoria, cap. 17), and I say that an illegal act was done under that Act, the Home Office take no notice of it. The Home Office cannot defend themselves by asking me whether, in my opinion, the law is wrong or right.

11131. I say, apart from technicality (I am quite prepared to deal with the technicality), you allege a substantial breach of the law?—Certainly.

11132. You said that it was illegal that these experiments should be seen by skilled persons—chemists. They are not the general public?—They are not physiologists, I think.

11133. They are persons who would sell antivenin if antivenin came into use?—My point is that an illegal act was committed, and the Home Office condoned it. That is my point.

11134. That is a matter of dispute, of course?—Yes. I only say that that is my point, for what it is worth.

11135. Then you complained of some experiments performed in Ireland. You know that the Irish Office

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and the Home Office have no connection?—I know that they are all under the Act.

11136. Under the Act—yes?—I suppose there is no connection under the Act. There is no connection in that sense.

11137. There is no connection whatever. Therefore, you do not hold the Home Office responsible for what is done under the Irish Government?—I am very glad to acquit you, Sir Mackenzie, of Professor Shaw. When I brought that forward, I was not bringing it forward as an attack on the Home Office.

11138. I only asked, because, as you would say, unfortunately, from your point of view, both the Home Office and the Irish Office take the same unfortunate view of their duties under the Act?—In that case they certainly did take the same unfortunate view. I think in that case, as a matter of fact, the six months' limit prevented the Home Office from doing anything. I do not think the Home Office knew about these experiments until we drew their attention to them.

11139. The Irish Office, if you please?—Yes. At that time the six months was out, and I do not think they could have done anything with the best will in the world. I think they said so in their letter to me. I am only judging by memory now.

11140. (*Mr. Ram.*) With regard to your attitude generally, as I understand, your attitude and that of your Society is one of regulating the practice of vivisection as much as you possibly can?—Yes.

11141. That, of course, is a view that we can appreciate and sympathise with much more than with some views which others have given us. With regard to that, your Bill, which I will deal with in a moment, proposes certain alterations of the existing law with that view?—Yes, with that view.

11142. With regard to the past, the present Act is 30 years old, and the law has been administered under that Act for that length of time?—Yes.

11143. Your Society is older even than that?—About contemporaneous; two months before, I think.

11144. And your Society, no doubt, during the whole of that time has been composed of persons active and anxious in every possible way to repress any cruelty or stop any illegality?—Certainly.

11145. Under the Act the powers granted to private persons to put the law in operation are found in Section 13, under which a person may apply to a Justice of the Peace if he believes that any act has been performed by an unlicensed person in any place not registered under the Act. Has any application ever been Section 13, under which a person may apply to a justice for such warrant to examine any premises under that section?—Not that I am aware of.

11146. Has an application ever been made under Section 21 to the Secretary of State to allow a prosecution against a licensed person who has been doing an experiment for any reason other than that allowed by the Act?—Not since I have been honorary secretary, certainly.

11147. Is that because you have never had any case which, in your opinion, would justify such a request being made?—Oh, no, it is because under the way in which the Act is administered, and within the four corners of the Act, it is impossible for us to find out what happens inside the laboratories.

11148. Let me sever the two things for a moment, under the Act and under the administration; I am coming to the administration shortly. Under the Act you say that you have never been able to ascertain any instance which would have warranted anyone on your behalf making an application for a prosecution?—I do not think I have ever known of any instance, which I thought was a breach of the law, which was published until after six months.

11149. Therefore there has never been any such application made?—No.

11150. With regard to publication being made six months afterwards, you have given us a list of five or six different cases in which there has been, in your opinion, a breach of the Act, and as to which you have found fault with the action of the Home Office; those are the cases that you gave us in your evidence the time before last. You will recognise them by name. There is Crile's case, Grünbaum's case, Starling and Bayliss' case, Cecil Shaw's case, Noel Paton's case, and Dixon and Brodie's case?—Yes.

11151. Those are the cases which you gave?—Yes.

11152. With regard to those cases, you believe that there has been some contravention of the Act?—I should like to look at each case. The *gravamen* of my attack is that in each case when I have complained to the Home Office of actions done which seem to me to be inhumane, the Home Office have accepted the *ipse dixit* of the person whose conduct was impugned. It may not necessarily have been a breach of the Act.

11153. I think in each of those cases you did allege that there had been a breach of the Act, and you called the attention of the Home Office to it?—If you would give me them one by one, I can answer you.

11154. Crile's case was the case in which you said that there was an operation performed under incomplete anæsthesia?—Yes.

11155. That would have been a breach of the Act?—In that case pain was inflicted under the licence.

11156. Yes?—I should say that that was a breach of the Act.

11157. In Grünbaum's case, he performed an experiment, as you allege, before the public?—Yes.

11158. In Starling and Bayliss' case we do not doubt that there was an allegation of illegality?—Yes.

11159. And that was established largely—I mean the allegation?—I ought to distinguish now. My assertion is now that Professor Starling broke the Act by not killing the "brown dog" before he gave it over to Mr. Bayliss. That has never been before the court.

11160. I quite gather that. In Dr. Cecil Shaw's case it was an alleged breach of the law; that is "my work"?—Yes.

11161. Then Dr. Noel Paton's was the case of fasting experiments?—Yes. There is a little distinction in the case of Professor Noel Paton. Professor Noel Paton asserted that these experiments were outside the Act.

11162. Quite so?—That is not a breach of the Act. His defence was that they were outside the Act, and I represented that they should be within the Act.

11163. And Dixon and Brodie's case was a case of incomplete anæsthesia?—Light anæsthesia.

11164. Those are the cases that you brought before us?—Yes.

11165. Were those all the cases that you reported to the Home Office as having been, in your opinion, contraventions of the Act?—I could not say that.

11166. At all events, they are all that you have brought before this Commission?—Yes.

11167. In all or in any of those cases, do you allege that any suffering was caused to the animal by reason of the alleged breach of the Act?—Yes.

11168. In which case?—In Dr. Crile's case, for instance, certainly.

11169. Dr. Crile's was a case of incomplete anæsthesia?—Yes.

11170. You are aware that in that case, whether the animal suffered pain, turns upon whether the anæsthesia was or was not complete to the extent of removing all such pain?—Yes.

11171. And if, as Dr. Crile asserted, and as the bystanders seem to have asserted, the animal was insensible to any feeling of pain, then there would be no pain caused by a breach of the Act?—If they were accurate.

11172. With regard to any other cases, do you allege suffering being caused by an alleged breach of the Act?—Most decidedly to the case in Ireland—shoving shot into a rabbit's eye and watching it for six months.

11173. The Irish case I have purposely avoided; I was dealing with the cases reported by you to the Home Office?—In Dr. Noel Paton's case, of course, I assert that to starve an animal for four days is a painful thing.

11174. Eliminating the case of Dr. Noel Paton for a moment, as to starvation, are there any other cases in which the alleged breach of the Act involves suffering to the animals?—You want to know which of those cases are both illegal and painful?

11175. Yes?—If you will rehearse them again, I should like to have them separately. I have answered as to Crile and Noel Paton.

11176. And as to Grünbaum?—Yes, Grünbaum's experiments certainly were painful.

11177. In what way were they painful?—He describes the rabbit after 30 minutes dying in convulsions of snake poison.

11178. In that case the animals were being legally operated upon under the Act, save in so far as the operation was public?—Yes, but you ask me whether they were painful, and I say that they were.

11179. What I put to you was whether the illegality increased the pain?—No, not the illegality.

11180. Then I think it comes to this, that except in the case of Dr. Noel Paton starving animals, the illegality did not cause pain to the animals—did not cause any increase of pain?—You mean, if you accept the defences?

11181. Yes?—That is so, if you accept the defences.

11182. May I take it that in all the years during which your Society has been investigating these matters, those are really all the cases, as to which you have brought before the Home Office allegations of impropriety or contravention of the Act?—I am not prepared to say that. I have brought forward a few of them. I do not think I could say that I have brought them all.

11183. Would you let us have any others which you consider important. I take it that even if these are not all, you brought before this Commission those which you regard as the most serious?—I have got instances, which, to my lay mind, seem to involve a great deal of suffering, but not illegality.

11184. I am pointing out that these are the only matters which you have brought to the notice of the Home Office as being illegal?—Possibly. I am not prepared to say that offhand. I have only brought a few of the cases.

11185. But you have brought, no doubt, the cases which seem to you the most important. You have not given us the worst of your cases.

11186. (*Chairman.*) Was it your intention, in going through them, to bring before us all the cases in which there was illegality?—I am constantly corresponding with the Home Office, I am sorry to say.

11187. If you say that it was not your intention, that is an answer?—I am not prepared to say that these are the only cases in which I complained of illegality.

11188. That was not the question. Are there others in which you complained of illegality?—I think very likely.

11189. Do you know of any others?—I have a very bad memory. I cannot say offhand. Sir Mackenzie Chalmers would be able to say.

(*Sir Mackenzie Chalmers.*) I quite agree that you did write about others; but these are like the strawberries—the best at the top.

11190. (*Mr. Ram.*) We can only deal with those that you have put before us. Everyone of these matters was, in your opinion, and in the absence of explanation, a technical breach of the Act?—Yes.

11191. With regard to the case of the chemists, your allegation there is that there was an illegality, because the experiment was performed before an Association of Chemists, which constituted the public?—Yes.

11192. You have been asked whether you had ever witnessed an experiment yourself, and you said No, that you would not assist the physiological experimenters in breaking the law?—That is so.

11193. Do you think it would be an advisable thing, in the interests of humanity, if some such person as yourself, say, who takes an interest in humanity, did witness some of these experiments from time to time?—Most certainly.

11194. Have you been precluded from doing that in the past by the Act?—Yes.

11194A. You think that it would have been a breach of the Act?—Yes.

11195. Let me refer you to the part of the Act which deals with that matter. It is Section 6, the words are: "Any exhibition to the general public, whether admitted on payment of money or gratuitously, of experiments on living animals calculated to give pain shall be illegal." Do you think that when you are finding fault with an experimenter, if he said to you "Come and see what I do," which I understand is what you have been asked?—I have been asked.

11196. In your opinion would it be a breach of the Act if you, as one of the public, went to see it?—Yes. I have always thought so.

11197. You do not arrogate to yourself that you are the general public?—I am one of the public.

11198. (*Chairman.*) The question is, whether it would be a breach of the Act on the part of the operator if he allowed it?—Yes.

11199. (*Mr. Ram.*) But you said that you would not assist him in that breach?—Yes, I feel that I should be assisting him in that breach.

11200. I can understand it, but I cannot agree with it. May I put it to you in this way, that if you had gone you might have done infinite good from the point of view of humanity?—I do not think that any animal would be subjected to torture in my presence by any vivisector. That is why I want the Inspector always present. My presence would have the same effect, I think, as the presence of a Government Inspector.

11201. At any rate, you would have known much more about what was done in these experiments if you had been present?—Yes.

11202. But I understand that that was your reason, and the only one?—If I was present they would know that I was present.

11203. May I ask whether, had it not been for this section of the Act you would have gone?—I do not think that I should have served any general purpose by going, unless I had a right to go when I liked. To go by invitation would mean nothing. Of course, I should see no infliction of pain on an animal, being a more or less well known anti-vivisectionist.

11204. You would have learned a great deal of the circumstances under which experiments took place?—No doubt.

11205. Your evidence, which I have gone very carefully through, is something like, I may say nine-tenths of it, an indictment, and a long and acrid indictment, not of the Act but of the administration of the Act by the Home Office?—And of the Act also.

11206. I leave one-tenth to the Act?—Yes.

11207. There is an indictment with twelve different counts against the administration of the Act by the Home Office?—Yes.

11208. In your Bill you also propose that licences should be granted by the Home Office and the inspectors appointed by the Home Office and so forth?—Yes, I leave some of the Act of 1876 alone.

11209. But I think you leave all of it, so far as that goes. You safeguard it when they are appointed, but you leave their appointment to the Secretary of State?—Yes.

11210. The same Secretary of State whom time after time, man after man, you have distrusted so much in the past?—First of all in my Bill the certificates would not be merely as to the scientific fitness of the applicants, but also as to their humanity.

11211. I am coming to that afterwards?—And secondly, of course, the mass of operations will be performed in the presence of the Government Inspector.

11212. I am coming to that also. In the meantime the appointment and administration must, as you put it in your own Bill, be under the Home Secretary?—Yes.

11213. Now with regard to the certificate of humanity which you ask for in the eighth clause of your Bill, do you really attach importance to that?—Yes, very great importance; always provided that the names of those who sign the certificates are published.

11214. You will exclude, I suppose, any person who avowedly did not love animals?—Oh dear me no, I only want a person not to be unmerciful.

11215. I want, if I can, to get at something tangible. You want a certificate of a Justice of the Peace, or a Minister of Religion, for instance, that a person is humane?—Yes, that he is a person who would not torture an animal.

11216. There are a great many Justices of the Peace and Ministers of Religion. Should you really attach importance to two Ministers of Religion or two Justices of the Peace having signed a paper to say that they

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believed that the applicant was a humane person who would not torture an animal?—I should if their names were published, so that they were publicly made responsible.

11217. You suggested, I think, in answer to one question put to you by Sir Mackenzie Chalmers, that your own Society would be a useful body to give those certificates of humanity?—I did not say that; you are putting that into my mouth.

11218. If it was said *per incuriam*?—I should have put it in my Bill if I had thought it proper.

11219. Do you suggest that it would be a good thing?—I think that my Society is quite as fitted to advise on humanity as that other society is to advise on scientific fitness.

11220. Do you know and can you name any experimenter whom you would consider humane?—Yes, I believe there are some vivisectors who are quite humane, and if it was my duty to find out who were humane, I should do my very best to ascertain it.

11221. And the statements of such men as that you would take as being accurate statements when they were asked what they meant?—Yes, if I was satisfied that they are humane.

11222. Then may I take it that you regard untrustworthiness and, to a certain extent, unveracity and inhumanity as being almost identical?—I do not quite say that. I say that if a man is inhumane and is then asked questions by the Home Office or by any authority as to what he has done, if he has been inhumane it is contrary to human nature for him to admit it. No man would in that case. If a man is inhumane he is driven, if he is asked what he has done, into a certain amount of inaccuracy no doubt.

11223. I do not quite follow that. Sometimes even dishonest prisoners plead guilty?—No doubt.

11224. (*Chairman.*) The certificate is not to be that the Justice of the Peace knows that he is humane, but knows that he has a reputation for humanity?—Yes.

11225. (*Mr. Ram.*) How does a man get a reputation for humanity?—That is as near as I can get it.

11226. I am only anxious to know how much importance you really attach to that?—I attach very large importance to the fact that these persons who, by signing, more or less undertake in the public eye that these men are humane men, should have their names published so that everybody should know who they are. It is a responsibility which a man will not lightly take upon himself unless he is quite sure.

11227. There are two or three detached questions which I want to put to you with regard to your evidence. In answer to Question 10517 you said that you take no interest in the object with which any experiment is made?—That is broadly my attitude.

11228. You said it once or twice; you used those words?—Officially I mean to say that my Society is interested absolutely from the point of view of the animal.

11229. But you do recognise, because I see that you put it in your Bill, very wisely I think, in Clause 2, Sub-section (1), that "the experiment must be performed with a view to the advancement by new discovery of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering?"—That is in the old Act. I leave that.

11230. But you recognise there and you perpetuate, of course, naturally, that these experiments must have a humane object in view?—What I meant was, I may have been misunderstood, that when there is a question of severe pain, my Society does not recognise any justification of that pain by any lofty motive being asserted.

11231. I am glad you have answered the question, and I am glad to have that explanation. I want you now to look at your Bill, please, on two or three points. You say in Clause 2, Sub-section (4), "The animal must be killed while it is so under the influence of such general anæsthetic as aforesaid, and before it recovers therefrom sufficiently to feel pain." Is it your object to preclude all operations which, while painful in the inception, and therefore necessarily requiring an anæsthetic to be administered while they are being performed, are of such a nature that the result cannot be ascertained unless the animal be allowed to recover?—Yes.

11232. Then you wholly obliterate Certificate B and all cases under it?—That is our desire.

11233. That is your object?—Yes, we believe that under Certificate B most severe pain is inflicted.

11234. You do contemplate, and under your Bill you allow, the perpetuation of experiments not calculated to give pain at the initial stage?—Yes, those inoculations.

11235. But which may produce pain afterwards as the result of the inoculation?—Yes.

11236. Supposing that an animal has an operation performed upon it under anæsthetics, and is allowed to recover, and is treated, as we have been told here, as carefully with antiseptics, and generally treated as a human creature would be, so that there cannot be anything like severe suffering, do you think it is more objectionable to keep the animal alive under those circumstances than to allow the animal to live for a certain time, at any rate, while suffering from the effects of inoculation?—Under my Bill the moment the animal displays any sign of severe pain it is to be destroyed.

11237. I am coming to that in a moment. I quite see that that is your point. If the animal operated upon under anæsthetics and allowed to recover, making recovery under antiseptic treatment, was plainly not suffering severe pain, would you have any objection to allowing it to live?—It all depends upon the statement that the animal is not suffering.

11238. It does?—We are not prepared to accept absolutely the statement that all operations, however severe, leave no suffering behind.

11239. There has been no such statement made before this Commission or anything like it. The statements made before the Commission have been that in many of these cases, although the operation was painful, and therefore demanded anæsthetics in the performance of it, there was no subsequent suffering at all. But you say that you are not prepared to accept that statement?—I am not prepared to accept it.

11240. But under your Bill you provide that the Inspector should often see such animals, in the last sub-section of Clause 2, and that the animal is to be killed if suffering?—Yes.

11241. My object is to see how far your Bill would help us. Would it not be sufficient, in your opinion, to allow such operations as I am now indicating, the animal recovering, if the Inspector who saw the animal repeatedly satisfied himself that it was not suffering, or, if it did suffer, caused it to be killed?—I think that is outside.

11242. You see, my anxiety is this: You have said yourself repeatedly, and that is what has made your evidence, I think, on many points valuable, that you do not want to stop research?—That is so. We are quite misunderstood if anybody thinks that we want to stop science. We wish to promote science.

11243. You would wish, therefore, to make such provisions as, while safeguarding the animal, will secure the continuance of research?—Yes.

11244. I may take it, then, that you agree with me that if the Inspector sees the animals and certifies that they are not suffering, or causes them to be killed, if they do show signs of severe suffering, there might be operations under what we call Certificate B for short, which still might be permitted?—Yes, with this proviso. We are now talking of Certificate B, and under the present law my contention is that long before that period to which you now allude when the animal is healed——

11245. Not healed?—Is healing; and long before that under Certificate B, as now worded, atrocious suffering, in our opinion, can be inflicted upon it.

11246. At the moment I am on this point, whether you can suggest such a modification or, in your view, such an addition to Certificate B as will grant its existence in future, although perhaps modified in the way that you yourself wish?—If Certificate B could be so framed as to preclude such experiments as I have instanced of Mr. Stewart's—I am not agreeing for a moment with the description of Mr. Stewart's operation—it might be that the animal was under curare, for I do not know how many hours, and nothing else. It is such experiments as those that I am aiming at destroying in this Bill.

(*Dr. Gaskell.*) Those operations are under licence, not under Certificate B.

11247. (*Mr. Ram.*) That does not touch the point in one sense, but I want to follow this point. If you obliterate Certificate B altogether you do prevent an enormous number of investigations, the results of which cannot be ascertained, unless the animal is allowed to live for some time?—Yes, it must be so.

11248. And those operations, so far as this Commission has been instructed, seem to be the bulk of the operations which in the past have thrown most light upon the different humane objects which it has been desired to carry out. You accept that?—I accept that you have been told so.

11249. Then what I am anxious to get you to assist me in, if I can, is some such alteration or modification in Certificate B, as in your opinion might make it safe to allow it to continue safeguarded, rather than put a stop to all the investigations which at present can take place only under Certificate B?—Of course we are very suspicious of Certificate B, and it is very difficult for me, as representing my society, to say. I should only agree to its being continued provided that the animal after the operation is as much safeguarded as it would be under inoculation.

11250. That is the very point I wanted to bring out. Now with regard to inspectors, you, I suppose, realise that you are asking one of two things, either for an army of inspectors or for a very great diminution in the number of experiments that will take place?—Not so, necessarily. At present the Inspector, as I have said, wanders about on the chance of seeing anything, and wastes an enormous amount of time in that way. If every experiment had to be performed before an inspector he would never waste a minute; he would go from one place to another and witness experiments, and all his time would be occupied in actually seeing experiments, instead of wandering about the country seeing nothing as now.

11251. At all events I gather that there should be an inspector present at every physiological experiment?—Yes, but there are not so very many. If you leave out the inoculations the number is not very large.

11252. (*Chairman.*) Upon that point would it satisfy you if, instead of the operator being forbidden to perform an experiment unless the Inspector was there, a notice was given always beforehand that an operation would be performed at such and such a laboratory of such and such a kind; because there might be some purely insignificant operation which the Inspector need not attend. At any rate, there would always be knowledge on the part of the Inspector that it was going to be done at a particular time, and the operator might expect to see him coming in at any minute, but at the same time he might not be there in every case. For him to be there in every case would seem to me a tremendous interference?—We view it from the point of view of the animal. We feel that the animal is entitled to the presence of the Inspector when it is subjected to a severe operation, whatever it may be.

11253. A notice would not meet your view?—No.

11254. (*Dr. Gaskell.*) Do you mean that you would not allow more than one experiment to be made in a laboratory at the same time. There are often three or four experiments going on?—Do you mean in one room?

11255. No, in one laboratory?—Certainly not.

11256. You would not allow that?—No, I think that the Inspector should be present at every severe physiological experiment on an animal. He might go from one room to another.

11257. (*Chairman.*) You do not say severe in your Bill?—I mean any physiological experiment.

11258. You do not mean to include every inoculation?—No.

11259. Or every operation for which a licence was necessary?—No, a licence is necessary for inoculations.

11260. You mean any operations for which anæsthetics are necessary?—Any operations for which anæsthetics are necessary, certainly, because the presence of an anæsthetic means suffering, and therefore whenever there is suffering or the chance of suffering, our opinion is that the inspector should be present.

11261. (*Mr. Ram.*) While we are on the question of suffering I think you want some modification of your Bill at the bottom of page 2. In the last two lines you provide that the animal may be allowed to recover, although inoculated, and you say, "Directly the animal begins to feel pain it must be placed, and thenceforth while it suffers or is likely to suffer must be kept under the influence of a general anæsthetic of the nature of a respirable drug or gas, such as chloroform, or ether, or alcohol, chloroform, and ether combined, of sufficient power to prevent the animal feeling pain; or, if the animal is not so placed and kept under the influence of such anæsthetic as aforesaid, it must be forthwith killed"?—Yes.

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11262. But in your evidence I observe that you say, and you have said just now, severe pain; you mean, I take it, appreciable pain?—I am not unreasonable. There is no pain gauge that I know of.

11263. You have said repeatedly, and said reasonably enough, in your evidence, for instance, in answer to Question 10765: "It will be the duty of the Inspector to go and see that animal frequently, and if it be in a severe condition of suffering it will be his duty to order it to be killed painlessly"?—Yes.

11264. You must therefore in your Bill modify that?—Where the suffering is really trifling, of course, I do not want to interfere.

11265. But you see we are or may be advising legislation here, and I want to see whether you can assist us in any way?—May I say serious pain.

11266. You would be willing to insert "serious pain" in line 36 at the bottom of page 2 of your Bill?—Yes.

11267. You have put it so clearly in your evidence in reply to Question 1078: "It depends entirely upon the amount of pain involved," and in the next answer, "Under my Bill it would be the duty of the Inspector to have that animal destroyed as soon as it manifested signs of severe suffering"?—Yes.

11268. That is what several Inspectors who have come before us here have said in answer to myself and others, that as soon as they saw any signs of appreciable suffering they would cause the animal to be destroyed. With regard to Clause 3 of your Bill—I remember the experiment which you told us of about the eye—do you think it is necessary to have a clause dealing with the eye expressly?—There is a very large feeling among the humane that it is a necessary clause in the Bill, because the eye is so frequently used, I believe, for the purpose of breeding bacteria and such things. That is the ground upon which we put it; and the eye is a very sensitive organ.

11269. Is it suggested that now in England matters which cause pain are inserted into the eye of anæsthetised animals?—I think so. I have seen pictures of diseased eyes.

11270. Would it meet your view to say that no operation of any sort shall be performed on the eye, save when the animal is under anæsthetics?—That would be so already. The eye is not put outside the Act. If an operation is performed on the eye it is as much under the Act as if it is performed anywhere else.

11271. I wanted to see whether you still think it necessary to except the eye from any experiment at all?—From inoculations we think that they should be eliminated altogether. Our view is that inoculations in the eye must always be painful—we may be wrong.

11272. Under Clause 4, Sub-section (2), you provide that every licence shall specify the time and place at which the experiment or series of experiments is or are to be performed. Is that with the object of letting the Inspector know?—Yes, and the public and everybody else; to lift up the veil of secrecy from these things.

11273. We have heard something about the veil of secrecy; but why on earth should the public know when an operation takes place?—Because this is part of the thing that is put up; this is part of the notice. It is the licensee who is to say when he is going to do it.

11274. I ask why?—In order that the Inspector may know.

11275. Then you eliminate the public?—Yes; I was not looking at it. "Every licence granted hereunder

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shall specify the time and place at which the experiment or series of experiments is or are to be performed." That is in order that the Inspector may know for certain when it is going to be performed. I was thinking, and no doubt you were, of the putting up of the notice somewhere.

11276. I was. One other question on the Bill, and, I think, only one. You propose in Clause 9 that proceedings may be taken at any time within two years?—Yes.

11277. And yet you propose that a report shall be sent in forthwith?—Yes.

11278. If a report is sent in forthwith, how can it be necessary to have so long a time as two years within which to take proceedings?—I put both those conditions in, so as to get the longest possible time.

11279. May I point out that the object of shortening the time, as you know, in all these Acts is to preclude a charge being made against a person, when perhaps the circumstances are forgotten, or the evidence is lost, or some person is dead?—I thought that would be met by the provision that the prosecutor shall put down the sum of £50.

11280. Would that affect your Society, with its large income?—We should be the last persons to bring an action.

11281. (*Chairman.*) This is a question of instituting a prosecution?—Yes.

11282. (*Mr. Ram.*) Seriously now, supposing you got your way in having a report sent in forthwith, as to which we should want a definition, I think, but, say, within fourteen days, would you want so long a time as two years?—Perhaps that report might have omissions which might be supplied in a subsequent publication by the vivisector.

11283. You would have five and a half months, then?—That would depend upon when the vivisector published it. In the report sent to the Home Office, it is conceivable that the vivisector might leave out something accidentally or intentionally, and might subsequently fill it in in a publication like the "Journal of Physiology."

11284. Would it be sufficient to say that any proceedings must be taken within six months of publication, or giving information of that which was impugned?—Yes, I would accept that at once. All we want is that we may have the possibility of doing it.

11285. What I want to get rid of is the oppressiveness of these proceedings hanging over for two years?—I accept that at once.

11286. There is one matter I should like to have your view upon, because you are aware that statements have been made which have been investigated before us in the "Shambles of Science"?—I have read the book some time ago.

11287. I wanted to ask you about that experiment on a marmot?—I cannot tax my memory with that.

11288. Then I do not think I will go into it at all?—I am not connected with that book in any way.

11289. There is one other point in your Bill. In Sub-section (6) of Clause 2 you say, "The experiment shall not be performed for the purpose of attaining manual skill"?—Yes, that is the old Act; I retain that.

11290. A good deal has been said about it; do you see any objection if it is limited to animals which are wholly under anæsthetics, and must die under anæsthetics?—It is hardly for me to suggest to the Commissioners to enlarge the Act.

11291. But you know so much about it, that I wanted to know your view. Have you considered that point?—Yes, I have. Personally, I am afraid that if this provision was taken out, it would immediately be taken to be a permission to students at once to set about vivisection for the purpose of getting manual skill.

11292. By a licence?—Yes, getting a licence for this very thing.

11293. Students?—Yes. Certainly they would if this clause was eliminated; there is nothing to prevent their applying for a licence for that very purpose, to get manual skill.

11294. You do not suggest that taking these words out would give them permission?—No, I say that they would apply for licences, and would get them from the Home Office. I believe that they get them now.

11295. There is one other matter only that I want to put to you. Several witnesses have been asked, but I do not think that you have, and I put the question to you, does your Society take any action at all, or any interest, in what has been termed a farmyard operation—namely, castration, and so forth, of animals?—My Society does not. It is a Society simply for the purpose of combating vivisection.

11296. Has your Society, or have you as an individual, any views as to the thousands of operations in every county in England, which must, I think, every day cause more pain than all the operations in all the laboratories?—No, my Society has no such view, of course. It is purely an Anti-Vivisection Society. If I were to use its funds for the purpose of advocating a crusade against any other suggested form of cruelty, I should be committing a breach of trust.

11297. Your Society, then, not only concerns itself with vivisection alone, but solely concerns itself with vivisection which is scientific?—With vivisection under the Act.

11298. Of course, all these operations are vivisection in the crudest and cruellest form?—If so, they come under the purview of the Royal Society for the Prevention of Cruelty to Animals. That is our view. That is the proper society to deal with any such cases.

11299. At any rate, your Society does not concern itself with that matter?—It is outside our Society.

11300. I think you have been careful to abstain from going into any of what are called the ethical considerations?—No, the whole of my evidence has been in that direction. I stated that I threw myself open to cross-examination.

11301. (*Chairman.*) I invited Mr. Coleridge at the beginning to express his views shortly on that point, as we have had it fully before us, and he agreed to that. He stated shortly that he was opposed to vivisection on ethical grounds.

(*Witness.*) Yes, in my opening statement.

(*Mr. Ram.*) Perhaps I misread what the noble Chairman put to you; but, at any rate, I will not go into it with you, your evidence having been of another sort.

11302. (*Sir John McFadyean.*) I am afraid that after a remark that fell from you in answer to a previous question, you may rather resent the suggestion that you ever use any language that is ambiguous; but it seemed to me that you did in your evidence make use of two or three words that are capable of more than one interpretation. One of them is the word torture. I submit to you that the word torture is employed in two senses. In the first place, it is employed to denote very severe pain, and in the second place it is employed in describing operations and acts where the main object of the operation or act is to cause severe pain. I want to ask you in which of these senses you use the word torture. For instance, to-day you said that in your opinion animals are tortured in laboratories attached to large hospitals. Do you mean simply that in vivisection operations conducted in these laboratories severe pain is occasioned to animals?—That is what I mean.

11303. That is all?—That is all.

11304. You did not mean at all to imply that it was the main purpose of the operation to inflict pain?—I was not thinking of the purpose. I am looking at it entirely from the point of view of the animal.

11305. I am asking you now because it is a very important distinction; that is to say, whether you merely make the charge that severe pain is occasioned to animals?—That is all.

11306. You do not mean that it is a deliberate design on the part of the operator to cause severe pain?—No; it is incidental to the investigation.

11307. Still, you admit that the word "torture" is used in both senses?—It may be used in many senses. That is not my intention at all.

11308. Then you make use of the word "cruelty"?—Yes.

11309. I do not know whether you admit that that is in any degree ambiguous?—I will define cruelty in the way I use the word.

11310. Will you tell the Commission what your de-

finition is?—When I say "cruelty," I mean the infliction of severe pain upon an animal, not for its own good. It is very difficult to define these words.

11311. It is not quite the definition that I expected?—That is what I mean.

11312. I thought you would probably say that all unnecessary pain comes under the head of cruelty?—No; that would imply that I judged what was necessary and what was not necessary for the purposes of experiment, of which I know nothing.

11313. But your definition does involve your judging the question, because you come to the conclusion that it is cruelty in all cases in which pain is inflicted, if it is not for the animal's good?—Yes.

11314. I am quite content with that definition. You will, I suppose, admit that the Act now in operation, the Act 39 and 40 Victoria, was not meant to prohibit painful experiments on animals, but merely to place certain restrictions on such experiments?—The Act as it left Parliament.

11315. That was the purpose of the Act?—Yes, it permits pain.

11316. It legalises pain?—It legalises pain; that is my objection to it.

11317. Your suggested amendment of this Act is intended to prevent the infliction of serious pain?—That is so.

11318. Or even serious discomfort?—Utter wretchedness.

11319. In connection with experiments on animals?—Yes.

11320. You wish to make it illegal to cause serious discomfort to the animal, even although, in the opinion of persons well qualfied to judge, the experiment might extend man's knowledge of preventing or curing disease or alleviating suffering?—I am not prepared to accept or admit, not being an expert, the claims put forward by the vivisectors as to those benefits.

11321. That I did not put to you. I asked you to assume that?—That there is benefit, do you mean?

11322. To assume that man's knowledge may be increased thereby?—That is a different question.

11323. Knowledge is power?—But the Act itself divides between the two, you remember; it says for benefit or for knowledge. There are two objects in the Act.

11324. Is it, or is it not, the purpose of your amendments to prevent the infliction of serious pain or even serious discomfort?—Serious pain.

11325. You admit that?—Yes.

11326. And you desire to have the infliction of serious pain or serious discomfort made illegal, even although, in the opinion of persons well qualified to judge, by carrying out experiments involving such pain, man's power of preventing or curing disease or alleviating suffering might be extended?—The evidence laid before the Commission has been almost unanimous in declaring that vivisection and experiments performed do not include pain.

11327. But I assure you that that has not been the invariable character of the evidence that we have had here, and I think it is within our own knowledge, as Commissioners, that some experiments involve pain at the outset. I have asked you to agree to the statement that the Act legalises pain?—Yes.

11328-9. Therefore, there is no reason why those who are licensees should not admit that they do cause pain. But I put it in another way. You do not admit that the fact that useful knowledge might be extended by inflicting pain on animals, with regard to the cure and preventing of disease, is any justification whatever for causing pain to animals?—No. I say that we have no right to inflict severe pain upon animals.

11330. I perfectly understand your position. My next point is this. You are, I suppose, quite well aware that no such restrictions as you propose to impose upon experiments performed on animals for the purpose of extending human knowledge are placed on man's dealings with animals for other purposes, although these involve severe pain and suffering?—I am not prepared to admit that.

11331. I shall have no trouble in getting you to admit it?—Very well.

11332. Will you take it from me that it is perfectly legal, and a common operation, to take a male animal, tie it to a table, or otherwise secure it for the safety of the operator, and while it is in complete consciousness cut out its testicles? As a laymen, do you feel able to express an opinion as to whether that is a painful operation or not?—I have no doubt that it is painful.

11333. Have you any doubt as to its being an exceedingly common operation?—I have no doubt it is quite common.

11334. I suppose you are also aware that it is not regarded as an illegal operation?—I do not know that there has ever been a test case on that. I think it is cruelty to animals generally.

11335. Then I will put it to you in this way. Would you encourage the creation of a society, such as your own, having for its main object to bring about legislation to prevent the castration of male animals?—I do not say that.

11336. But you admit that it is cruel?—Yes.

11337. Would you, then, be in favour of an agitation which would compel the use of an anæsthetic pushed to complete anæsthesia on every farm animal that is castrated?—Yes, if it was practicable, certainly.

11338. Then, take your own words, what is the meaning of practically? Does it take into consideration the profit of farmers?—I am not thinking of that for a moment. I am thinking of it purely from the point of view of the animal and cruelty.

11339. Quite irrespective of the fact whether you would render farming operations, so far as relates to the breeding of male sheep, for instance, entirely unprofitable?—I do not accept that suggestion for a moment.

11340. It is not a suggestion; it is a question?—You suggest that it would make it unprofitable. I do not accept the suggestion.

11341. I ask you to assume that it would?—I will assume it, for the sake of argument. What next?

11342. You would still insist upon every male animal that had to be castrated being put under anæsthetics?—Certainly, but I do not accept the premise, you know.

11343. I did not ask you to accept the premise. But what I asked you to assent to was that at the present time such restriction as you seek to impose in your Bill are not imposed on men in other dealings with animals which involve great pain?—I may, in passing, say that so far as I know there is no Act of Parliament that makes it cruel, or would in any way support a prosecution for the circumcision of Jews, which is, I suppose, a painful operation, but it is done in very extreme youth, and it is quickly over, and it is done, I presume, for some purpose that the Jews support. It is somewhat similar to that, I think.

11344. Will you forgive me for suggesting that one might, at any rate, contend that it is done for the benefit of the individual Jew operated upon?—Yes. Whether it is so or not I am not prepared to say.

11345. You would not seriously put that forward with regard to castration of lambs and pigs?—The castration of lambs and pigs I presume is inflicted for their benefit; they would not be permitted to live unless it was done to them.

11346. Do you suggest the same with regard to horses?—Yes.

11347. Are you not aware that in some countries a very large proportion of the working horses are uncastrated?—If that be so, and there is no reason against it, I am in favour of their not being castrated here. But I have always understood that they would be dangerous in the streets.

11348. I think it is unquestionable that they are more dangerous, but not to the point of being unworkable?—Then we should have to do without them, I suppose.

11349. You would be in favour of a society which would agitate for the suppression of the castration of horses?—I did not say that. I never said it.

11350. But I think you must say so in order to be perfectly logical?—No, I say that it should be painlessly done.

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11351. I will give you another example of man's dealings with animals in which severe pain is caused, and up to now, so far as I know, no restriction has been placed on these operations. Take, for instance, the trapping of rats and rabbits. I suppose you will admit that these are operations which cause pain which, without exaggeration, might be described as torture to the trapped animal?—That depends upon the sort of trap, does it not?

11352. I am speaking of the common rabbit trap. I only know the humane trap by name. I am well aware that the common trap is an exceedingly painful trap, which very often breaks the leg, and the animal may be in a state of torture for many hours?—I abominate that.

11353. It is not a case of your individual opinion?—I thought that was what you asked.

11354. I mention all that in order to lead up to this. It is not the individual feelings and actions of witnesses that have to be justified?—Are you asking me as representing my Society?

11355. No. You want this Commission, I take it, to approve of your Bill?—I do.

11356. Nothing would please you more than that we should recommend Parliament to pass it?—I should be still better pleased if you abolished vivisection altogether. I am afraid you will not do that.

11357. I am looking forward to the possibility of your Bill being recommended in Parliament, and I have foreseen that in Parliament these facts that I have just been bringing under your notice might be raised. I thought perhaps that you would help the Commission to find some clearly defined simple principle on which they could recommend Parliament to thus discriminate between painful experiments for the extension of useful knowledge, and the infliction of pain which is incidental to operations which are inspired by some other motives. So far we have not got that assistance from previous witnesses, except that some have said that no operation of a painful character on the lower animals is justified unless it is for the individual animal's good?—I would answer you in this way. I am not in Parliament unfortunately, but if I were in Parliament I should meet arguments of that nature, that you ought not to pass this Bill until some other cruelty was disposed of or dealt with, that you would never do anything in this world if you tolerated evil in one place until you stopped evil in another. If you must not stop a man beating his wife in Stepney until you have stopped another man jumping on his wife in Upper Tooting you would never do any good at all. You must take things in detail. You cannot put down all cruelty at once. The object of my Society is to deal with one branch of cruelty which we think very severe and which might be stopped with advantage to the community. It is no answer to us to say that there are other cruelties in the world. Of course there are. And in due course of time I should hope to see some of them mitigated.

11358. Would I be wrong in interpreting that answer as a confession that you find it impossible to state such a principle?—Not at all. The broad principle that I go upon is to put down all cruelty when I find it.

11359. I ask you if you could assist the Commission to find a principle on which they could ask Parliament to discriminate between painful experiments intended to extend man's knowledge and the infliction of pain in painful operations which are inspired by some other motives? I ask you if you can give us a simple principle?—No, I cannot put down a broad principle of that sort.

11360. Then I am afraid it is rather hopeless to ask Parliament to do so?—Not at all. I do not think persons in Parliament will be influenced by the argument that you are not to stop one cruelty until all cruelties are stopped.

11361. Would I be misjudging your reply if I took it to mean that as soon as you had disposed of this particular form of cruelty you would approve of going on to prevent the castration of animals except under the influence of anæsthetics, and that you would put down field sports, which, as you, I think, must admit, involve a great deal of infliction of pain?—Are you asking me now my own private views or the views of my Society?

11362. I will take both?—The views of my Society have nothing whatever to do with those other things.

11363. But, if you will pardon me, in a sense you have a great deal to do with them. You come here to advise us to take a particular course of action, and I think you will admit that we ought to be able to give a reason for advising Parliament to discriminate between pain which is inflicted in the course of operations intended to extend man's knowledge and pain inflicted in the course of operations inspired by any other motive whatever. Your Society ought, if it, I was going to say, presumes to advise us in the matter, to be able to help us in this case?—Our position is this, you see. These other cruelties that you justify have no Act of Parliament legalising them; they are open at the present moment so far to prosecution. It might not be successful, because public opinion may not have got up to the point at which such prosecution would be succuessful. But there is nothing on the Statute-book legalising these inflictions of pain that you are speaking of now.

11364. Would you be content to put what we call vivisection on the same footing exactly as these other operations?—No, because vivisection, in my opinion, under the present law certainly can be far greater cruelty than any of the cruelties you mention, because in the case of castration it is an operation that is done in a very few seconds, I presume; I do not suppose it takes a very long time, and some vivisections, at any rate, are very prolonged.

11365. I confess I am rather surprised that you should suggest that there are greater possibilities of cruelty in the one than in the other, because there are no legal restrictions whatever on the one?—Yes.

11366. You really, I take it, would not assert that the people who perform these operations on farm animals are on the average more humane than the licensed vivisectors?—In the very nature of things all the farmer wants to do is to get rid of the animal's testicles as quickly as he can. It must be a very short operation. With regard to the man in the laboratory, his object may entail a very long operation; the thing must take twenty to thirty minutes even to prepare the animal and the various vivisection arrangements and appliances.

11367. Are you taking into account that most of the seriously painful experiments at present performed for the extension of knowledge have to be done under anæsthetics?—Not under the Act as it stands necessarily. I say that the present law admits of any amount of torture. That is my objection to it.

11368. What I want to know is whether you contend that under the existing law a great deal of exceedingly severe pain is caused to animals?—I say it can be.

11369. That was not my question?—I dare say it is.

11369a. Do you say that it is?—I am not there. I will neither admit nor deny what goes on in laboratories. I am not there. All I know of what goes on in laboratories is what vivisectors choose of their own will to publish. That is all we know and can know of what goes on in laboratories.

11370. At any rate, you are not able to help the Commission by giving us any simple principle on which we could discriminate between painful experiments for the extension of useful knowledge and the infliction of pain which is incidental to operations which are inspired by some other motive?—I do not think any simple principle would discriminate.

11371. I quite agree with you. To pass to another point, the Act 39 and 40 Victoria legalises experiments for teaching purposes, and your amendment would make instruction in that way illegal?—Yes.

11372. In answer to a question put to you last week I think you grounded your objection to experiments for the purpose of teaching, on their being unedifying and bad?—Yes.

11373. Assuming that such experiments are painlessly conducted before medical students, will you tell us in what respect you think they are bad and unedifying?—You see you always put upon me an assumption that I do not grant.

11374. What is the assumption?—That there is no pain being inflicted upon the animals.

11375. I will allow the operation to be safeguarded by your own restriction that there is to be an inspector there the whole time. Will you tell the Commission then in what respect the operation would be un-

edifying and bad?—Because I think that vivisection of an animal not for its own good, merely for demonstrating some well-known principle of physiology, must be naturally approached by all the spectators in a perfectly different spirit from an operation upon a human being done for that human being's benefit. To me that is a perfectly self-evident proposition.

11376. But that should not be compared with operations on human beings. It ought to be compared with other legal painful operations on the lower animals. Is it less noble to use an animal in an absolutely painless fashion to demonstrate facts of physiology to a class than it is to kill an animal for food?—I thought the comparison was, certainly it was in my evidence, between a vivisection experiment in a laboratory and an operation in the theatre of a hospital.

11377. I never mentioned anything about that?—In my evidence that was the comparison put to me.

11378. You will forgive me if I am wrong, but so far as I could make out the only objection you have expressed to allowing painless vivisectional experiments to be performed for teaching purposes, was that they were unedifying?—Yes.

11379. I invite you to explain in what respect they are unedifying?—I am influenced very largely by the opinions of others upon the matter.

11380. By unedifying, do you mean that they are of no purpose, and are useless for the purpose of teaching?—I did not mean that. I meant by unedifying——

11381. Demoralising?—Yes, rather that. I did not use so strong a word. I used the word unedifying because I did not want to use such a strong word.

11382. I am sorry to repeat the question. Supposing the word to be used in its mildest legitimate sense, I want to know how it comes about that the operations are demoralising when the whole of the operations are painless and are known to be painless by the spectators?—I shelter myself under the opinion of others. The Inspector from Ireland gave that as his mature opinion after many years being Inspector. I do not know whether he used the word unedifying.

11383. I can assure you that he did not say they were unedifying. He expressed doubts whether they were of any real value, and he expressed a doubt as to whether they were always under complete anæsthetics. But I am going to have the experiment safeguarded by your Inspector. We were not able to put that to him.

11384. May I put it to you in this way. Do you see any reasons to modify your Bill by legalising operations for purposes of instruction when the Inspector is present and is assured that the animal is throughout the whole course of the proceedings completely anæsthetised. I may remind you that in answer to a question last time you said that you would place confidence in the opinion of the Inspector?—Yes. I am quite prepared to admit that at present I have no reason to suppose that any torture has ever been inflicted on an animal in the presence of the Inspector. That is why I am ready to trust the Inspector to see that it does not take place in future.

11385. Since the main purpose of your Bill is to prevent cruelty to animals (I mean you are not engaged in any religious crusade) why, if you were assured that these operations might be conducted without pain, should you expressly make them illegal in your Bill?—I may say that there is a very large feeling among the public whom I represent, that vivisection experiments, it may be only sentiment, are something peculiarly distasteful. For instance, there are many persons among them, and those whom I not the least respect, who would say that deliberately to place a dog under perfectly complete anæsthesia to throw it from the top of a high tower to see what happened to it when it hit the pavement was a brutal thing and ought not to be done even under anæsthetics.

11386. Such a thing I may say would never be done for teaching purposes?—You say that. I only say that there is a feeling among a large mass of people that they do not like an animal to be treated in that manner merely for the purpose of inquiry.

11387. But in this inquiry we are endeavouring to get at the moral or other justification for people's feelings one way and another; that is rather what I was inquiring after?—I think it is a respectable feeling on the part of people who would protest against a dog being put under complete anæsthesia and thrown from the top of a tower to see how many bones were broken or in what condition its interior parts were.

11388. That would be an experiment?—Yes.

11389. That would not be conducted at demonstrations?—The students might be all invited to come and see the demonstration conducted. It is very unedifying to a spectator, in my opinion, although the animal is under complete anæsthesia.

11390. I perfectly agree with you; but might not that be safeguarded in wording your Bill?—It would be very difficult.

11391. Would not the Home Office be apprised of the nature of the experiment that was to be done for teaching purposes?—In my opinion many of the experiments detailed by Dr. Crile, even if they were under complete anæsthesia, are repulsive to every decent person who reads them.

11392. Were those done for teaching purposes?—They might be.

11393. But they were not?—They might be.

11394. That is not the point?—Certainly they might be.

11395. They might be. But even supposing they had been absolutely painless, you are taking objection to Crile's experiment because you are not satisfied that the animal was completely unconscious? — Yes, altogether. I assure you I represent, I am not speaking for myself now, a very large number of people who, if every single experiment in Crile's book were granted to be completely under anæsthesia, would still say that such experiments were brutalising—to pour boiling water, for instance, into a dog's inside to see what would happen. There are very many people who, rightly or wrongly, think it is not the way in which dogs ought to be treated by human beings. They are the friends of man, and it is not the way to treat them; and for anybody to witness such a thing must be a brutalising spectacle and cannot be edifying. That is my answer to you about edification. And you yourself agree with me, because you say that you agree about the tower. You say it is a repulsive thing to do.

11396. For instruction purposes. My attention has been called to the fact that Sir Thornley Stoker did express the opinion (in answer to Question 761) that certain demonstrations would be of a demoralising character to young men and women who witnessed their performance, and that they seemed to him to be an offence against humanity. So that to that extent you are supported?—I thought he said some such thing.

11396A. Some doubt has been expressed as to whether the infliction of pain on animals for what might be called trade purposes comes within the purview of the existing Act. Do you think it desirable that it should?—That is really a question for lawyers. You mean whether it should be in my Bill. It would not be desirable in my opinion.

11397. Yes, I understand that your intention was to leave such cases outside the Bill; but I wondered why you discriminate between operations that are identical in every respect except the motive?—I have taken the present Act of Parliament and endeavoured to amend it. I have not drafted a new Bill altogether to deal with cruelty to animals. I find the Act as it is, and I have not changed it in that respect.

11398. Will you forgive me for saying that though in a sense it is an amendment, in another sense it is an absolutely new Bill; because you admit that the present Act legalises pain?—Yes.

11399. You want to make pain illegal?—Yes, I do.

11400. And in leaving out painful experiments incidental to trade purposes you are discriminating?—No, they are not experiments.

11401. That is only begging the question. I submit that you call the thing when it is done for the extension of knowledge an experiment, and when it is done for trade purposes not an experiment; and if you like to put the question in that way, why do you discriminate between experiments which after all are identical experiments in every respect except motive?—The difference, I think, is fundamental. I am dealing in this Bill wholly with vivisection as practised in laboratories for the purpose of either learning, or instruction, or whatever it may be. These trade products, as they

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call them, whether they be cruel or not, differ in no way from any other ordinary cruelty to animals in the streets by mankind, and ought to be dealt with in that manner. They have ceased to be anything but trade products. If in the production of any material there is cruelty involved, that is a matter for the Royal Society for the Prevention of Cruelty to Animals; it has gone out of the purview of experiments. I am dealing solely with vivisection here. I am not going on a crusade against all experiments now; it is not my opportunity, and I did not come for that. I am not even on the committee of the Royal Society for the Prevention of Cruelty to Animals.

11402. So that it is a mere matter of convenience?—Not at all. If I was on the committee of the Royal Society for the Prevention of Cruelty to Animals, I should try and galvanise them into greater activity against many cruelties.

11403. You mean that you would try to prevent painful operations for such a purpose as the manufacture of serum?—I should if I could possibly do so.

11404. Can you assist us with a definition of what is an experiment. I noticed that you were inclined to classify the operations connected with the standardising of sera as outside the Act?—That is a question for the lawyers. It is outside the Act I presume.

11405. Why?—I do not know, I am not here as a lawyer.

11406. Do you think they ought to be outside the Act?—I do not know whether they are or not. I am not here to give a legal opinion.

11407. You have already given one?—I believe that some of the standardising experiments have been placed inside the Act. Sir Mackenzie Chalmers can tell you that.

11408. You are aware, I suppose, that a good many materials, such as milk, are tested by animal experiment as to the presence of bacteria and so on?—I was not aware of it; I will take your word.

11409. Have you never heard that medical authorities have been endeavouring to control that?—Yes, I have heard of it.

11410. Do you think that that is covered by the Act, that it comes within the Act?—If it is not painful I have no objection to it.

11411. But supposing it is as painful as other things which you say would be done for trade production and are outside the Act, what is your opinion?—If it is a painful experiment I object to it; of course, I am not here to give an expert legal opinion whether certain things come within the present Act or not—certainly in the presence of Sir Mackenzie Chalmers I would not venture to do so.

11412. I thought it was permissible to ascertain your intentions. It would fall to others, of course, to interpret your Bill if it became an Act?—Yes.

11413. Then in Clause 4 of your Bill, page 3, Subsection (1), you say that no licence is to be granted to perform more than one experiment, or one series of not more than six consecutive experiments; and in the next paragraph, that the licence is to specify the time and place at which the experiment or series of experiments is or are to be performed. Can you tell me what is the object of that? It is not, is it, to make experimentation difficult?—Oh, no.

11414. What is the object?—The idea I had in framing that was to prevent a man getting leave to experiment from the beginning of the year to the end on as many animals as he likes, without any reference to the Home Office again.

11415. Surely you might find the happy mean somewhere between a licence for each experiment and a licence extending over a year. I put it to you that there are medical officers of health, and, I believe, veterinary officers also, who daily have to deal with infective matters, or matters suspected of being infective and dangerous, and in their case it would be quite impossible to carry out experiments under an Act framed like your Bill?—What you are describing sounds to me as if it was outside the Act altogether.

11416. No, you have just admitted a minute ago that the testing of milk, for instance, ought to be considered as inside the Act?—I do not give an opinion of that kind. I said nothing of the sort. I should not say so before Sir Mackenzie Chalmers. I will not give an opinion upon it at all.

11417. But nobody has ever suggested the contrary. It is within the Act?—I do not accept your statement that it is within the Act. I do not know what the case is. I want a specific case before I admit anything tangible.

11418. I will give you a simple case. Veterinary inspectors in the employ of the London County Council have to make periodical visits to cows within the metropolitan area, and when they come across a cow whose udder appears to them to present indications of its being tuberculous, but they are not absolutely sure, then they take a sample of the milk, which is taken to a bacteriological laboratory, and there and then injected into guinea pigs; and that is done pretty frequently. There are many similar operations where the thing is coming in daily, and has to be dealt with at the time it arrives. I suggest that the provisions of your Bill would make that sort of work practically impossible. Was that your intention?—I am not quite clear that it is legal, as it is under Section 3, Subsection (1) of the Act. "The experiment must be performed with a view to the advancement by new discovery of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering."

11419. (*Sir Mackenzie Chalmers.*) Sub-section (4) is the material one. All these, I think, come under Subsection (4)?—Yes, but you never issue that certificate; you have not for years. That is just my complaint. I quite agree with you, that if these experiments are to be performed they ought to be under Certificate D, which has not been issued for years.

11420. (*Sir John McFadyean.*) After all, you are expressing legal opinion?—I am on that point most distinctly. I have quite a clear opinion upon it. I accept Sir Mackenzie Chalmers' view.

11421. It was that that I had in my mind when I asked you to define an experiment. I put it to you that when a sample of milk, taken in the circumstances I have mentioned, comes before a bacteriologist, he wants to extend his knowledge with regard to it, and the only means of extending his knowledge is his experiment which he performs upon the guinea pig?—You are putting that upon me. I do not admit it.

11422. Will you admit that his object is to extend his knowledge?—Yes, but not that that is the only way to extend his knowledge.

11423. That is not the material point, if you will forgive me for saying so. It is the extension of knowledge by new discovery?—Yes.

11424. Surely he must extend his knowledge by a new discovery with regard to this particular sample of milk?—Well, I will accept that.

11425. Lastly, in answer to Question 10,952, you said that all these experimenters have the greatest contempt for the Act of Parliament. You may read the rest of it if you like?—No, I do not in the least wish to do so. Yes, I see that is my one little slip, I remember.

11426. Will you forgive me for saying that it seemed to me a very hasty generalisation? Would you desire to qualify it?—Perhaps the whole answer was a slip; part of it was, I remember.

11427. I did not want to go into that?—It is rather sweeping, I admit.

11428. It is too sweeping?—It is too sweeping, I admit.

11429. Will you go so far as to say that not more than a small minority have a contempt for the Act?—No, I think a good many have a contempt for the Act. They have called it "an Act of which we are all ashamed," as I have quoted once already.

11430. You ask the Commission to believe that the majority of licensees hold the Act in contempt?—I think they do.

11431. Have you any evidence of that?—I read out one letter, at any rate, in which it was alluded to in public as "an Act of which we are all ashamed."

11432. You do not possess any evidence probably in that direction that you have withheld from the Commission?—No, except that I have heard it in various ways myself.

11433. (*Chairman.*) You said just now that you knew all that you know, and all that you can know, from what is published, and that you cannot see these things

yourself?—Yes, that is with regard to bringing prosecutions.

11434. But these experiments are attended by a great number of students, are they not?—Not the research experiments.

11435. But the other ones are?—Most of those under Certificate C, yes.

11436. (*Sir Mackenzie Chalmers.*) Under licence?—Yes, demonstrations under Certificate C.

11437. (*Chairman.*) And the research operations are witnessed by skilled men, I suppose?—You mean brother vivisectors?

11438. Yes?—Yes.

11439. Does it occur to you that if anything took place at demonstrations which was cruel and negligent, I do not say that you would hear of every case, but amongst the students there would be some who would resent it?—No doubt.

11440. And who would make complaint?—No, I think very likely not. I should think that the general feeling in the profession would be much too strong for them. I do not think that a student would risk his future by making complaints against the demonstrator.

11441. You have heard no complaints, at all events?—Yes, I have, and indeed you have had them before you.

11442. From students?—Yes; you have had Miss Lind-af-Hageby.

11443. But she was a student simply for the purpose of fitting herself to inquire—to follow this matter up, as she told us. I was speaking of ordinary students who are being educated for the profession?—I should like on this to show you the kind of evidence that is constantly brought before me. I have had letters from students myself—of course, I do not give their names away.

11444. Have you made complaints upon them to the Home Office?—No, I have never done anything at all with them naturally; I cannot do anything. I should very much like to say on this question, that in this very last week I have received a letter signed by five or six undergraduates in an Oxford College, directly opposite the laboratory at Oxford, in which they complain of hearing the most dreadful howls of agony from dogs in that laboratory. Here it is, I now bring it forward to show you the kind of evidence that is brought before me constantly.

11445. These were not persons present in the laboratory?—No; but they lived opposite, and heard screams of what they say are the vivisected animals.

11446. If dogs are kept there, they may have been the screams of animals under operation; but really on this subject we have so many people who bring very strong feelings to bear that I should like something more if I am to be satisfied that these were under operations?—I am only showing the sort of evidence that comes before me. I did the obvious thing that a gentleman ought to do. I wrote at once to the head of the laboratory, told him the information that I had received, and asked for his explanation; and he gives the lie to the six gentlemen at once. All I can say is that in a Court of Justice the six witnesses would go against the one.

11447. (*Sir John McFadyean.*) They could only prove that the dogs howled?—Yes, that they heard the howls. But the answer is that there are no dogs there at all; there are only puppies.

11448. And you can always tell the difference between a howl of pain and any other howl?—Yes, that is what these young men say—that they know dogs well enough.

11449. (*Mr. Tomkinson.*) May I ask, would it be a very awkward thing for a student to give evidence against the demonstrator?—These young people are not medical students. They do not mind their names being given at all.

11450. I am speaking of students who have witnessed demonstrations, and have complained to you about them; would they not be marked men if they came forward?—Very much so.

11451. Might it not interfere with their profession hereafter?—I have not the slightest doubt of it.

11452. They would be looked upon as disloyal to their profession?—Certainly. If the Commission like to have this letter, it is very interesting.

11453. (*Chairman.*) It does not touch upon our inquiry, apparently?—It touches upon it in this way: the extreme difficulty from our point of view of instituting any proceedings or taking any possible means of protecting animals.

(*After some deliberation.*)

11454. (*Chairman.*) A member of the Commission would like to hear the letter?—"Keble College, Oxford, June 12th, 1907.—To the Honble. Stephen Coleridge,—Sir,—I (an undergraduate member of Keble College, Oxford), knowing how keenly you feel about the practice of vivisection, wish to draw you attention to the following facts with reference to this practice. My rooms are exactly opposite the University Museum. Every day, almost without exception, I hear the piercing yells of a dog in agony. There can be no doubt that it is a dog in agony and not simply one in confinement. I have ascertained as a fact that vivisection is carried on in the museum from an official to whom I spoke on the subject when I went over on Sunday last in the morning (June 9th) after hearing these same yells. Being a keeper of dogs myself, I am in a position to know the difference between a dog in agony and one in confinement, and that it is not the latter I am perfectly confident. It is always the same, and I am convinced that it is something more than merely the beating of some dog, because that would not happen every day, whereas students' scientific classes constantly take place. I wish something could be done to put a stop to these devilish practices condoned in the name of science. To one who is a lover of animals it is perfectly excruciating to have to listen to a dumb animal being put to the torture, as I hear it every day, by these scientists who, being apparently devoid of all feelings of humanity, don't care what pain they inflict to gain their object. I should be so glad if you can suggest anything to be done. I should have thought at least they were bound to do these things in places where every sound could not be heard outside. Putting aside the fact of the inhuman cruelty which this practice necessitates, it is, to speak mildly, a public nuisance. Please use my name or letters as much as you wish.—I am, Sir, yours faithfully, HAROLD S. SHARPE." And then: "We, the undersigned, desire to state that we have also heard these yells, and wish to corroborate all Mr. Sharpe has said.—ARTHUR H. M. PEAT, CYRIL A. BONSER, ALFRED C. C. HARNEY EVANS, GEOFFREY HEALD, BASIL W. TRUMAN." I at once addressed a letter "To the licensees for the performance of experiments upon living animals, The University Museum, Oxford," dated the 14th June: "Gentlemen,—I have received from a correspondent, whose credibility I have no reason to doubt, an account of cries of dogs or of a dog in agony that emanate from the buildings of the Museum at Oxford. A portion of the buildings in question being a place registered (under the Act 39 and 40 Vic., c. 77), for the performance of experiments upon living animals, and you, gentlemen, being under the same Act licensed to perform such experiments in that portion of the buildings, I beg leave to inquire whether these cries come from your laboratories, and, if so, whether you feel disposed to offer any explanation of these most distressing sounds.—Yours obedient servant, STEPHEN COLERIDGE." Then I got this letter from Mr. Francis Gotch: "Physiological Laboratory, Oxford, June 16th, 1907.—Sir,—In answer to your letter of the 14th inst., I beg to state that no vivisection experiments on dogs have been carried out in this laboratory during the present year. Four puppies were born and kept in the adjoining yard, and the shrill yelps of these animals when at play were presumably the sounds heard by your correspondent. This might have been ascertained by making appropriate inquiry.—Yours faithfully, FRANCIS GOTCH." I sent this letter with my compliments to these young gentlemen, and this is the answer that they sent back to me: "Dear Sir,—I am sorry I have been away and only just returned and found your letter. Professor Gotch is a liar." These young gentlemen write like young men. "A man in Keble who is in for anatomy told Mr. Heald (one of those who signed my paper) that they did cut up rabbits, and that lately they had cut up a small dog, and this had been yelling for a week. It is perfectly absurd to say that the yells I hear are anything else than dogs, or possibly some other animals in agony.

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The idea of puppies is ridiculous. I have heard the puppies yelp very frequently, and I have also heard a dog scream, and I would swear to it anywhere. Mrs. Herford, writing to me to-day, says: 'It is distressing to pass the Museum for the yells one frequently hears.' Would two people be such fools as to mistake a painful yell for a puppy's yelp? It shows that Mr. Gotch is a liar, because one dog has been cut up this term. The man who told my friend (Heald) this said they did cut up rabbits. That is sufficient. Mr. Gotch says I might have ascertained the truth by making appropriate inquiries. I asked an official, as you know, and he said vivisection was practised. I would maintain, in spite of Mr. Gotch's statements, that animals have been cut up this term. If a man is devil enough to be a vivisector he can easily reconcile his conscience to a lie or two, especially when Mr. Coleridge writes to him, for he knows matters might be made hot for him." (This is very youthful.) "I am unable to get more facts than these: (1) One dog for certain has been vivisected this term; (2) also rabbits; (3) the cries I hear are *not* puppies'—Yours sincerely, HAROLD S. SHARPE." That is the sort of thing that is constantly coming before me, and that is the result that I get.

11455. It seems to me that the letter is about as reasonable as if Mr. Gotch were to answer it by saying: "No such thing has happened as you say, and as to your saying you heard it, all antivivisectors are liars, and I do not believe them." It does not carry it much further?—I do not think that is quite it, my Lord. We cannot carry it further. Here are six young gentlemen who write to me with no motive whatever but the motive of humanity. I admit that the language is the language of boys.

11456. (*Dr. Gaskell.*). What is the date that Mr. Gotch gave when he asserted that no animals had been experimented upon?—"No vivisection experiments on dogs have been carried out in this laboratory during the present year." The date is June 16th, 1907.

11457. "This term" meant distinctly during the time that Mr. Gotch was there and must have known?—It must have meant that. They give Mr. Gotch the lie.

11458. (*Chairman.*) It is possible that Mr. Gotch may be a witness here?—The difficulty is that under the present Act there is no Inspector, and nobody knows what happens. You get these opinions outside and inside, and who can judge?

11459. (*Sir John McFadyean.*) After all, the only direct evidence produced in that correspondence is that the cries that they heard are interpreted as cries of pain?—Yes; but these young fellows say that they know the difference between cries, and that they were the cries, at any rate, of dogs.

11460. (*Dr. Gaskell.*) Was there any definite date given when that small dog was cut up?—"Because one dog has been cut up this term"—it is "this term."

11461. (*Sir John McFadyean.*) That is not direct evidence?—No.

11462. (*Mr. Tomkinson.*) Does he not say that he called at the door, and an official admitted it?—"I asked an official, as you know, and he said vivisection was practised." That is his statement.

11463. (*Dr. Gaskell.*) He does not even say whether that dog that was cut up was alive or dead?—He does not. The context rather indicates that it was alive.

11464. You do get them for dissection dead?—Yes; but the suggestion in the context here is that the dog was alive.

11465. That is the suggestion, certainly?—Yes.

11466. (*Sir Mackenzie Chalmers.*) Did you send that second letter to Mr. Gotch, and did you get a further reply?—No, I left the matter there.

11467. There is a contradiction of fact?—Yes, a flat contradiction. It was no use carrying it any further.

11468. (*Dr. Gaskell.*) I do not quite understand your position with regard to Certificate B. I should like to have a little more explanation about it. I understand that you are desirous to abolish it in your Bill altogether, because you look upon it as entailing a greater amount of torture than anything else?—I think that more pain is inflicted under the present law under that certificate.

11469. Certificate B, of course, you know is for the purpose either of cutting out some organ or cutting some nerves and keeping the animal alive afterwards; that is the kind of operation?—Yes, doing some serious operation and allowing the animal to recover; that may be one of the objects.

11470. That is the object?—No, not the object; it is one of the objects of Certificate B.

11471. I understood you to assert, in answer to Question 10319, that Certificate B dispenses with anæsthetics immediately after the initial operation is over?—Yes, the new wording does do that ever since 1899.

11472. That is your main point about the torture?—Yes.

11473. Do I rightly understand that it is the initial operation that you think torture may be inflicted in especially?—No, you have mistaken me. My point is that under Certificate B, as now worded, anæsthesia must be applied during the initial operation of opening the animal and fixing the electrodes to different nerves, or whatever it may be. Then I say that under Certificate B the animal can be kept in that condition on the board, and allowed to recover its consciousness, and the animal can be kept there disembowelled, or whatever it may be, as long as the operator chooses, without any anæsthesia to allay its sufferings.

11474. That is your interpretation of the word "initial"?—That is what the certificate means.

11475. Pardon me; I think not. That is your interpretation of the initial operation?—Yes.

11476. If such certificate were worded differently, you would have no objection to it. Supposing, instead of being worded as it is here, "Being during the whole of the initial operation of such experiment under the influence of some anæsthetic," and so on, it was worded, "During the whole of the operative procedures in connection with such experiment," would that meet your difficulty?—No; because my objection is that, after the operative procedures are over, during the period merely of observation of the animal, it may be lying in dreadful pain without anæsthesia.

11477. That is still part of the operative procedure?—No; looking at a board and watching the blood pressure is not an operative procedure.

11478. Stimulating the nerves is operative?—No, you need not touch the dog. To operate is to touch, certainly.

11479. Then we will have any form you like. There is no question whatever that that initial operation is included under Certificate B, and is intended to include the whole procedure while the animal is under anæsthesia?—You say that.

11480. That is so?—I will not admit it for a moment. Why were the words "During the initial operation of such experiment" put in? It used to run "During the whole of the experiment."

11481. The whole point of Certificate B is that the animal should have recovered from the initial operation; is not that so?—The initial operation, yes, of such experiment.

11482. And the initial operation means the cutting of the nerves or the removal of the organ, or what not?—Yes.

11483. And then after that till the animal is sewn up—the whole thing?—Not necessarily at all. It may be for hours on the board in that condition before it is sewn up.

11484. How can it be so?—There are such experiments.

11485. Would you mind showing me a single experiment of that sort under Certificate B?—I say that such can be done.

11486. Will you show me one that has been done under Certificate B? I have never seen or heard of such a case?—I say that the words of the certificate admit of it.

11487. Are you aware that in order to make the thing quite clear, in every Certificate B of late years (I know it in the case of Mr. Ritchie in 1902, and I know it in the case of Mr. Akers Douglas in 1904) there is always this inserted: "All operative procedures in connection with any experiments, except experiments under Certificate B, plus EE, to be carried out under an anæsthetic of sufficient power to prevent

the animal from feeling pain"?—I quite admit that during the operative procedure—that is, using the knife.

11488. No?—I think it is obvious.

11489. Surely everything up to the stitching up is an operative procedure?—No, my contention is—I may be right or wrong ——

11490. I think you are absolutely wrong?—That is on the knees of the gods.

11491. No, it is a fact?—Here are the words of the certificate—this is the old certificate I am going to read you: "Such animals being during the whole of such experiment under the influence of some anæsthesia of sufficient power to prevent their feeling pain"; then the animal is allowed to recover under that.

11492. During the whole of what?—During the whole of such experiment; that was the wording of the certificate before October, 1899.

11493. Does that not confuse the term "experiment"? The experiment is not simply the operative experiment to begin with; it is the whole thing?—My point is that in 1899 the words were put in, "During the whole of the initial operation of such experiment." My contention—I may be right or wrong—is that the whole experiment may consist of first opening the animal and fixing electrodes to its cut nerves, or putting tubes down its arteries, and then it may be left on the board in that condition, fixed tight, so that it cannot move, and in that condition it may be allowed under this certificate to recover consciousness, and lie there bound at the will of the operator. I say that is part of the experiment, and it is not an operative procedure.

11494. But it is not Certificate B at all?—Yes, it is.

11494A. Certificate B is always for the purpose of keeping the animal alive afterwards; and for that purpose you would certainly not begin by putting tubes in a cut up animal in that sort of way?—I asked the very question of the Home Office, and I got a reply which confirmed my view of it.

11495. I should like to hear that reply?—I read it last time. Here it is. I asked, "Whether conditions are invariably added to this certificate" (that is Certificate B), "enforcing the employment of anæsthetics ensuring insensibility to pain from the commencement of the first operation until the conclusion of all manipulations of the animal, and until the last wound is closed aseptically." That is a very plain question, which obviously is directed to the very point on which you and I are at issue.

11496. Quite so. What is the answer?—The answer is: "Whenever the experiments would involve a series of cutting operations (which is very infrequent), conditions are added to the licence enforcing the employment of anæsthetics ensuring insensibility to pain during all operative proceedings." I maintain that that question and answer leave my contention untouched; that the animal may be opened and operated upon, and then left upon the board as long as no fresh cutting operation is performed (they absolutely say "cutting" here) without anæsthetics, under the conditions of that certificate.

11497. I should have thought that any commonsense person would understand that "all operative procedures" meant the operations right up to the time that the last stitch was put in?—That is the question that I asked.

11498. That answer implies it?—The question is: "From the commencement of the first operation until the conclusion of all manipulations of the animal and until the last wound is closed aseptically." I asked whether the anæsthesia really covered the whole of that period—the initial operation, the period of observation, and the final sewing up—and I did not get a reply which supports your view.

11499. Have you ever asked anyone who has operated under Certificate B?—I asked the Home Office the plain question what the certificate means. I give them the opportunity of giving me an answer, and they give me an answer which satisfies me that your contention is wrong.

11500. But that answer is distinctly in accordance with my contention?—I think not.

11501. Most emphatically?—I do not agree with you; "during all operative procedures"—that is all.

11502. The operative procedure I should have thought, certainly, so far as my knowledge goes, as recognised by all persons in laboratories, does include the whole of that initial thing?—The animal may be opened, as I have said, and be prepared. What is there in the wording of this answer to prevent that animal being allowed to recover, and lie on the board for hours before they begin to sew it up?

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11503. The words "operative procedure"?—There is no operative procedure. The animal is lying there, and nothing is done to it; it is not being touched; it is only being looked at. To look at an animal is not an operative procedure. I say the animal might be only looked at for an hour of two, or three hours, in that condition without any anæsthesia, under that certificate.

11504. You cannot give me an instance, I presume?—I am not a vivisector; I do not go into laboratories. I am only on the law.

11505. Have you examined the various Certificates B, which have been issued, to see the nature of the experiments?—They are not sent round to my office to be examined.

11506. Can you not get them?—No, I cannot. I have asked for them, and have met with a flat refusal. I have been given a courteous reply, but they will not submit them to me.

11507. You know that it is stated on the certificate that the man must describe the nature of the operation and his object in performing the experiment?—I daresay.

11508. Then it goes on to say, "All operations in connection with the experiment to be carried out under anæsthesia of sufficient power to prevent the animal feeling pain." My contention is undoubtedly that that does include the whole thing. You say that in your opinion it might be used, but you can give me no evidence that it has ever been used in the way you indicate?—I did not come here to give evidence of what I have seen in laboratories. I have never been in one.

11509. You suggest that?—I say so.

11510. To my mind it is a most extraordinary suggestion. I never heard of such a suggestion before?—I daresay not.

11511. It is impossible for me to conceive how it can come into your head?—Here is the law.

11512. No, the operative procedure takes in the whole of it. Then, apart from that, I presume that that is your main objection to Certificate B?—That is a very considerable one.

11513. Then that part of the objection would go if it was shown to your satisfaction that such an occurrence as you indicate has never taken place?—You could not possibly inform me that such an occurrence has not taken place. You are not in the laboratories any more than I am. It may be your opinion that such things do not take place, and it may be my opinion that they do; neither of us has a right to say that it has or has not occurred.

11514. Do you not understand that the very kind of experiment that you are putting down is an experiment that would be conducted under licence alone?—No, I do not say that for a moment.

11515. You do not understand it?—Yes, I do perfectly. I understand it too well.

11516. Then, I presume that you also consider that under Certificate B there is pain or considerable liability to pain afterwards?—What do you mean by afterwards?

11517. I mean to say after the whole thing?—What is your interpretation of "the whole thing"?

11518. After the operative procedure is finished with, and after the animal is kept in the laboratory?—When it is sewn up?

11519. Yes?—My answer to that is that I should think there may or may not be pain. In some cases there might not be pain, and in some cases there might be. I suffered considerably after I had an operation upon myself, I know.

11520. Are you aware that it is the custom now to add to that Certificate B these words: "The animals experimented upon under Certificate B, plus EE, must be treated with strict antiseptic precautions, and if these fail, and pain results, the animal must be killed under anæsthetics?"—I hear you say so.

11521. That is usually put on the certificate?—I accept your statement. I do not see these certificates.

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Sir Mackenzie Chalmers ought to answer that question, not I.

11522. Is it not the fact that in consequence of asepsis, as used in hospitals, a very large number at all events of the operations are followed by painless results?—The suggestion was made, I think, by Mr. Starling that the surgical wards of hospitals are a kind of merry place for everybody, laughing and joking. I have asked several people, and I have not had that extreme view of the happiness of surgical wards confirmed. I do not deny what you say. I daresay that under asepsis very many operations may heal without very much pain. I imagine that others will be painful.

11523. In answer to Question 10,839, you said that by pain you meant real severe suffering?—Yes.

11524. Not small discomfort?—No, serious pain was the term I used, which I have agreed to add to the clause in my Bill.

11525. And you also said, in answer to Question 10,834, that you were willing to accept the Inspector's verdict as to whether an animal is in pain or not?—Yes.

11526. Of course, you know that one of the main duties of the Inspector when he comes to a laboratory is to examine all the animals which have been subjected to an operation under Certificate B?—I do not know what the instructions given to him are. The only definite instruction that I recollect having been given here in evidence is that he is not to consider himself a detective. I do not know of any other definite instruction which has been given to him, so far as I can gather from the evidence.

11527 We have had it from the Inspectors themselves that they invariably examine these animals?—If they say so, I have no reason to doubt it.

11528. Consequently they do see all the cases of animals which have been operated upon under Certificate B?—No, I am not prepared to admit that, for very good reasons. I have brought information before the Home Office that an Inspector had been to a registered place, and had not seen a very considerable number of horses which were in great suffering in another part of the building; and he was not taken to see them. I have that evidence with me, for what it is worth. I submitted it to the Home Office at an interview. Therefore I am not prepared to admit that the Inspector always sees every animal in the building.

11529. The Inspectors told us that they do go, at all events to all the places in the building where they can find animals?—They might have found these horses, but they did not. They go where they are taken by the vivisectors.

11530. At any rate we have had evidence from Professor Thane and Sir James Russell, that again and again—in fact, in all cases when they have examined these animals, they have found that they were not suffering pain?—But that evidence of mine is before the Commission.

11531. So that I do not see why you are willing to retain Certificate A, and are determined by your Bill to abolish Certificate B. Is there not much less suffering, from the evidence of these Inspectors, under Certificate B, than there is under Certificate A?—If that is the evidence of the Inspectors, I am not prepared to combat it; but what I have pointed out, and what I think I have made perfectly clear, is, that my objection to Certificate B mainly is as to the conditions that are inflicted upon animals when there is no Inspector there.

11532. After the initial operation?—Yes, that is my point; that is my chief objection to Certificate B.

11533. We may have more evidence upon the question, no doubt, but so far at present as the evidence goes I have no reason to suppose that such an operation as you suggest has ever been made under Certificate B. I wish you could produce some evidence that it has been, because it is new to me entirely?—You must remember, when you put up the evidence of the Inspectors against anything that I have said, that I have already asserted my opinion, and I think I have supported it by a large amount of evidence, that the Inspectors are not unbiassed.

11534. Still, you have asserted definitely here that you are willing to accept the Inspectors' verdict?—The verdict of future Inspectors. I would certainly not have accepted the opinion of Dr. Poore on the question of the humanity of vivisectors.

11535. I thought you said just now that the future Inspectors were still to be appointed by the Home Secretary?—Yes, but there is that consideration about their humanity under my Bill.

11536. (*Chairman.*) No; it is the humanity of the operators that you get a certificate for, not the humanity of the Inspectors?—I think I have provided for the Inspectors too. It is in the proviso to Clause 7: "Provided always that in the appointment or assignment of such Inspectors by the Secretary of State, special regard shall be paid to the applicant's reputation for humanity."

11537. That is not a certificate, but "special regard"?—Yes.

11538. (*Dr. Gaskell.*) Would you consider that Certificate B might be retained if it applied only to animals which were operated upon under strict aseptic conditions. Do you consider that aseptic conditions before operation make a great difference with respect to the after question of pain to the animal?—That is a matter for experts. If the experts say so I am ready to accept, certainly, that asepsis does make a difference.

11539. What I am thinking of is this: I can understand your objection to experiments like those of Dr. Cecil Shaw, where septic material was put into the eye—that is, undoubtedly, I should think, a painful experiment; but in the case of a large majority of these experiments under Certificate B the Home Office directly insist on their being done aseptically for the purpose of preventing pain. Would you not allow Certificate B to exist if you were sure that that was always done?—My objection to Certificate B is that in its present form it permits something very much worse than that. You see what I am after.

11540. May I ask you, will you describe Certificate B in such terms as to avoid that initial operative difficulty of yours?—It is not in my province to be a draftsman, I have not done it, but I think it might be drafted in such a way as to preclude that possibility. I think it might be done with a little care.

11541. And if it was so drafted, would your contention still be that Certificate B ought to be abolished? If it was drafted so that the operative procedure, as I understand it, was really put in plain English so that you could understand it, would you still object to Certificate B?—That is the same question that Mr. Ram put to me, I think.

11542. I want to make it quite clear, because I want to understand why you make such a dead set against Certificate B when, so far as I can judge from what we have been told in evidence already, there is very little pain involved under it.

(*Sir John McFadyean.*) Mr. Coleridge has already said that he would put the animals under the same conditions as they are under Certificate A, because then he would be assured that they were anæsthetised throughout the whole of the operative procedure.

11543. (*Dr. Gaskell.*) What I put to you is this: What the vivisectors generally consider, you would say that you do not accept it, is that Certificate B implies exactly the same thing as occurs in every operation in a hospital?—I cannot agree to that.

11544. It is just like a surgical operation under anæsthesia, the animal recovering just as the patient does in a hospital?—I cannot admit that.

11545. (*Chairman.*) I think we all know what Mr. Coleridge's view is upon that point. (*To the Witness.*) As I understand, it is that you say that after that animal has been allowed to come out of the anæsthesia there is an interval, sometimes long, sometimes short, in which it suffers great pain?—That is my objection to Certificate B.

11546. (*Sir Mackenzie Chalmers.*) Whether that happens or not the certificate allows it, that is your point?—Yes, that is my point.

11547. (*Chairman.*) How far Certificate B does cover that is a question that we can settle for ourselves. It is either a question of the construction of a document or it is a question of fact as to whether the animals suffer pain?—Yes.

11548. (*Dr. Gaskell.*) In speaking about slight anæsthesia you stated in answer to Question 10387,

"You cannot be slightly without feeling. You either feel pain or you do not"?—I have taken the word in its proper sense.

11549. Do you mean to say that you cannot be conscious and yet not feel pain?—"ἀναίσθητος" means "unconsciousness."

11550. But can you be conscious without feeling pain?—I never have been myself.

11551. Is it not the case often in battle?—I am not a soldier.

11552. When worked up to a state of excitement, is it not the case that people do not feel the pain of wounds when they are cut?—I believe it is a well-known fact that when you are shot you do not feel the pain at once because of the shock to the nerves all round.

11553. Is it not the case in hypnotism that you can still be conscious and not feel pain?—I have not been hypnotised. I do not know about that.

11554. Do you also know that in lunatic asylums patients burn themselves and the sun burns them, but they do not feel pain?—I really do not know what lunatics feel; I have no experience of lunacy, I am glad to say.

11555. But you have told us distinctly that slight anæsthesia means that the animal feels pain?—I do not know that they have lunatic animals in laboratories. The parallel of lunacy does not seem to me to come in.

11556. I mean to say that in all these cases it is possible for consciousness to exist and yet pain to go?—That is your assertion. If you ask me to affirm it or to agree to it, I do not.

11557. You know, of course, I presume, that the corneal reflex is only one of many reflexes?—I am not a physiologist. I decline to discuss physiology with you or anybody.

11558. But you distinctly assert that unless the corneal reflex is abolished the animal feels pain?—I took that from the correspondence before me.

11559. No; all the correspondence before you said was that if the corneal reflex is abolished the animal is under deep anæsthesia.

11560. (*Chairman.*) I think everybody agrees that the corneal reflex is one of the last to disappear?—Yes, I agree to that.

11561. (*Dr. Gaskell.*) Will you not agree then that pain may disappear before the corneal reflex is abolished?—That is another thing.

11562. Just a question about Dixon and Brodie's case of light anæsthesia. I understood you to say, in answer to Question 10375, that the animal was under torture?—I say that I regard that experiment with profound distrust. I should like to have had the Inspector there.

11563. Do you not know that at these experiments a registering apparatus was being used?—I do not know in the least. I was not there.

11564. But do you not understand that if an animal is in pain it would show signs of that pain?—Not if it had curare. I do not know whether it had in this case.

11565. It was not a question of curare?—I do not know.

11566. And if the animal was in real pain do you think that the curves could have been registered by the apparatus in the way that they were?—There is a mention of curare (on page 144 of the "Journal of Physiology," Volume XXIX.): "In no instance have we obtained any effect by exciting the central end of the sciatic in a curarised animal" (I did not see that before), "but the experiments are not numerous." So that he did use curare.

11567. Not with the slight anæsthesia?—You do not know that.

11568. That is not the point. You were talking of slight anæsthesia?—Yes, and you asked me if the animal would not show signs of pain. I say, not if it was curarised.

11569. I want to know whether you do not consider when a delicate registering apparatus is going on in the animal, which moves perfectly smoothly and gets real good curves, even although the operator says the animal is under slight anæsthesia, that does not show that the animal was not feeling pain?—Not necessarily. I have an answer to that, of Sir Victor Horsley, in the case against me on that very point that an animal can be held tight when it does not move. (*Mr. Rufus Isaacs.*) Would it be possible to perform the experiment or the demonstration which we know took place, if the dog had been conscious? (*Sir Victor Horsley.*) No, absolutely impossible unless the dog had been fixed up with all manner of apparatus, to absolutely fix every bone. You can fix an animal with apparatus of *that* sort. (*Pointing to the operating board.*) That is the answer to that—you can so fix an animal on that board—and he pointed to it—they had it in Court.

11570. (*Mr. Tomkinson.*) Would your Society, if it could obtain all that it wished, go for total abolition of vivisection?—We should willingly and delightedly accept total abolition if we thought that it was within the range of practical politics; but we do not think that it is within the range of practical politics in the present state of public feeling; therefore rather than leave animals to their present fate we wish to get what we can for them.

11571. You are aware that some other societies look coldly on that attitude?—All the other societies I believe are against my own Society for taking that view.

11572. Your main point I think is that the present inspection is extremely imperfect—that it is practically non-existent?—Yes, from the animal's point of view.

11573. And that when there are cases of supposed or stated cruelty or evasions of the Act, which are brought to the notice of your Society, or complaints are made from any other quarter, the whole of the evidence of refutation has to come from the parties who would be incriminated?—That is so; either incriminated as first principals or as aiding and abetting—witnessing without protest.

11574. And you think that permission of some of the public, at all events to pay surprise visits, would be very desirable?—Yes, certainly, if it could be obtained.

11575. The use of curare you wish to prohibit altogether?—Absolutely.

11576. And morphia you do not accept as a perfect anæsthetic by itself?—No.

11577. (*Sir Mackenzie Chalmers.*) Or urethane?—No, nothing but a respirable gas. Those things take some time, at any rate, to act. If you give a poisonous dose of morphia to an animal, it cannot be said to be anæsthetised, I presume, for a considerable time.

11578. (*Mr. Gaskell.*) Do you say that morphia does not take away pain?—I did not say so. I am ready to accept expert evidence that a poisonous dose of morphia is a complete anæsthetic near the period of death.

11579. That is on the question of consciousness. I am simply on the question of pain. Is not morphia the one drug that is used for removing pain?—I never heard of anybody having a leg cut off under morphia alone; therefore I do not accept it as a general anæsthetic. If it were so, the triumph with which the discovery of anæsthesia was hailed would not have been justified, if morphia was a complete anæsthetic; because it was discovered many years before the discovery of chloroform.

11580. I should have thought it would be admitted, according to the evidence that we have had, that large doses of morphia undoubtedly remove pain entirely?—I do not accept that.

11581. (*Mr. Tomkinson.*) A great many questions have been asked, both of you and other witnesses, with regard to other forms of cruelty to animals which we know do exist, and I think that your position is perfectly consistent as a non-sportsman. We must all agree that there are such cases of cruelty, especially in the trapping of animals, which to my mind is the most cruel of all, and also in regard even to the slaughter of animals for food. Animals which are troublesome or violent or very noisy, such as oxen or pigs, are supposed to be stunned before being killed, for instance?—No doubt there are cruelties in slaughter-houses which ought to be put down.

11582. But in the case of what are called farmyard operations, castration and the like, with regard to what Sir John MacFadyean put to you, do you know

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of any case in which an animal is strapped to a board?—No, I do not.

11583. So far as my experience goes, that operation is only performed as an absolute necessity to enable animals to live?—That is my impression.

11584. Neither bulls nor horses could be allowed to continue entire, on account of the danger that it would be?—That is my belief.

11585. And with regard to pigs, are you aware that the operation is an instantaneous one, and apparently in small animals, such as lambs, so little painful that its after effects are not noticed at all by the animals?—So I should imagine. I have not seen them.

11586. Therefore it is not a question only of convenience, but a question of even allowing these animals to have their lives, certainly in the case of horses and bulls?—They would otherwise have to be destroyed, I suppose.

11587. Undue importance has been attached to that, in my opinion. Did you read a statement in an anti-vivisection pamphlet about the supposed baking of rabbits?—It did not issue from my Society.

11588. Have you read Sir Lauder Brunton's explanation of that before this Commission?—Yes, about its not being hotter than a Turkish bath.

11589. Was that explanation satisfactory to your society?—I have not gone into that.

11590. You know the statement was that so far from there being any cruelty or any baking or any excessive temperature, it was merely a warm bottle and cotton wool in order to prevent the temperature of the patient falling below a certain point?—I am not prepared to give an opinion upon that one way or the other. I have not studied it.

11591. (*Sir Mackenzie Chalmers.*) I think you said that your Society were not responsible for that pamphlet?—I do not think so. I do not think it is any publication of my Society.

11592. (*Mr. Tomkinson.*) Do you know anything of the offender against the law, I think you said his name was refused, who was deprived of his licence?—No, I do not know who he was to-day.

11593. But you said that he was reinstated afterwards?—Yes.

11594. Then as to starving animals, we have been told on a previous day that starvation does not entail pain. It is a rather novel idea, I think, when we remember all the stories we have heard of the experiences of people who have been starved after shipwreck and on other occasions?—Ugolino, for instance.

11595. (*Dr. Wilson.*) And starvation is awarded as a punishment?—I never thought of that. It is awarded as a punishment in prisons—or it was certainly at one time.

11596. (*Mr. Tomkinson.*) It is rather a startling statement to an ordinary person?—Yes.

11597. (*Dr. Wilson.*) Although you do not pretend to give an expert opinion, I take it for granted that your Society has from the outset amassed a large amount of literature on this subject of experimentation on living animals?—Yes.

11598. And your society also publishes a journal called "The Zoophilist"?—Yes.

11599. So that with regard to the usefulness of these experiments and their justifiableness, the editor of that journal is constantly scrutinising all the literature and reports which come to you?—Yes.

11600. May I ask whether the editor is coming to give evidence?—With a view of shortening the proceedings, I think I have represented pretty well all that my Society has to say. If the Commission desire to see him I have consulted with Dr. Berdoe, and he says he does not want to come unless you want him. I have also consulted my committee, and they say that if you would like to have him they will put him forward, but that he would more or less cover the same ground that I have done.

11601. (*Sir Mackenzie Chalmers.*) Is Dr. Hadwen connected with your Society?—No, we have nothing to do with him at all.

11602. You do not wish to answer for him?—Oh, dear, no.

11603. (*Dr. Wilson.*) You have already been asked many questions concerning the various clauses in your Bill which you have drafted and which has been approved by your Society. I suppose you will admit that the element of pain so long as these experiments are allowed can never be eliminated?—I suppose not in inoculation experiments altogether.

11604. Nor in experiments under Certificate B?—Not with Certificate B altogether, certainly.

11605. It has been admitted before this Commission that in a good many of those inoculation experiments pain, of course, cannot be avoided?—That is so.

11606. And also in a few experiments under Certificate B pain cannot be avoided?—No.

11607. The question of credibility has been so often raised, supposing you were assured, as several witnesses have assured this Commission, that no matter how prolonged and severe these experiments may be that are carried out, and no matter under what anæsthetic or narcotic they are carried out, they are absolutely without pain, would you accept that statement?—Certainly not.

11608. Would you accept it as a statement of opinion then?—Yes.

11609. But you would doubt it as a statement of fact?—Yes, I do not think I could accept it. It is not the opinion probably of the animal.

11610. Supposing that experiments are still permitted to be carried on before classes, and that all experiments were required to be carried out under anæsthesia, would it give greater satisfaction to your Society if every experimenter were required, in forwarding his reports of experiments, to fill up a form of certificate in respect to them to the effect that to the best of his endeavour and belief the experiments had been carried out painlessly?—Oh, no.

11611. That would afford you no satisfaction?—Certainly not.

11612. Still, after all, no matter what amount of inspection you may have, you must put some reliance surely upon the *bonâ fides* of the experimenter?—Of the Inspector, certainly.

11613. No, of the experimenter?—I quite admit, as I have already said, I am perfectly ready to admit, that there may be many licensed vivisectors as humane as myself. The object of the Act of Parliament is to enforce that humanity upon others who are not so humane; and that is the object of my Bill, to enforce that humanity upon those who have not got it.

11614. You think there is always a great risk of their becoming so keen in research that they may become callous towards the infliction of pain?—Yes, certainly.

11615. You have raised a strong objection to the Association for the Advancement of Medicine by Research having been selected by the Home Office as their advisers?—Yes.

11616. Whether that society has turned out to be biassed in favour of vivisection or not, can you not conceive that the Home Office or the Secretary of State at the time was influenced entirely by a desire to have the Act carried out more strictly and with the greatest possible care?—The evidence of Mr. Byrne, I think it was, is that the Home Office had no knowledge of the constitution of this society. It did not know who its president was, and, so far as I could make out, he knew very little about it.

11617. But when it was originally started I think it embraced many eminent men, such as Lord Lister and the late Sir William Jenner, whose names have been mentioned in connection with it. However, that is immaterial. Would you be satisfied if instead of that society, a statutory body such as the General Medical Council were made responsible?—I have already answered that question, by saying that I should not object to a body which had no licensed vivisectors upon it. I think that they should not report upon themselves.

11618. The General Medical Council consists, of course, of some of the most eminent men and teachers in the profession, and that body is entrusted with the registration of all qualified medical men, with the standard of education of medical students, and also with the power of removing from the register any medical man who misconducts himself in any way.

Would you be satisfied with a body such as that?—If there were no licensed vivisectors upon it, yes.

11619. That body is responsible for the education of the medical profession, so that the onus then would be thrown upon the profession instead of upon the Home Office for granting licences?—My desire is to separate licensed vivisectors from the medical profession. I do not include them in any way in my mind with the medical profession. In my mind the medical profession, as a rule, are not vivisectors.

11620. But, after all, is not the consensus of leading medical opinion of the day in favour of experimentation on animals?—I expect it is—the expressed opinion. What the unexpressed opinion may be, I do not know.

11621. Then it is a question between the public really and the profession, is it not?—No, I do not say that for a moment. I said the expressed opinion of the profession. There are numbers of medical men who agree with me who do not like to say so out of *esprit de corps.*

11622. I need not press that point?—But my Bill does not abolish vivisection.

11623. No, you only wish to protect animals as much as possible?—Yes.

11624. (*Chairman.*) Is there anything further you wish to say?—There is one point I should like to mention. I want to read a letter from the Duke of Portland, who is chairman of the Dogs' Home at Battersea, with regard to a suggestion which was put to Mr. Starling by, I think, Sir Mackenzie Chalmers, about using the Dogs' Home as a sort of source of supply of dogs for vivisection.

11625. (*Sir Mackenzie Chalmers.*) It was suggested by one of the witnesses?—Yes, I think it was Professor Starling. You asked the question, and he thought it would be a good thing. I accordingly wrote to the Duke of Portland as Chairman of the Home, to know whether he would approve of such a use being made of his Dogs' Home.

11626. He objects on behalf of the Home, of course?—Yes, he says: "Dear Mr. Stephen Coleridge,—Many thanks for your letter. I think Mr. Starling's suggestion is not only horrible but absurd, and it is one which, both as President of the Lost Dogs' Home and as a private individual, I should feel bound to resist as strongly as I possibly could. I say the suggestion is absurd because I am convinced it would never be accepted by the majority of the subscribers to the Home, and I have no doubt if persisted in it would mean the closing of the Institution and the loss to the public of much useful work at present undertaken and carried out by voluntary subscription. I am sending your letter and my reply to the Secretary of the Lost Dogs' Home, and, personally, I shall be glad to know that you have opposed Mr. Starling's suggestion, and I am, yours sincerely, (Signed) PORTLAND."

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11627. (*Chairman.*) Is that all you wish to say?—Before I go, my Lord, I should like to say this one word in parting. I have to thank the Commissioners very cordially for their kindness in listening to me so long, and I wish just to say this one thing. As regards the courtesy with which Sir Mackenzie Chalmers has treated me, I shall always feel very grateful, and personally nothing but pleasure, in having been able to bring accusations against the Home Office face to face with the Chief of the Department. But having said that, I wish to emphasise, if I may, that I cannot help feeling rather forcibly that after I have left this room Sir Mackenzie Chalmers remains in the position of judge upon the indictment which I have felt it my duty to bring forward; and I therefore feel that the situation is a peculiar one—it is not one of my making at all; but I do think it ought to be remembered as a fact that after I have withdrawn Sir Mackenzie Chalmers remains as the judge upon the very points which I have brought as an accusation against the Home Office of which he is the head.

11628. Whom he has to advise?—Well, to advise. But I feel sure that in the circumstances, having had the great pleasure of being cross-examined by Sir Mackenzie, he will meet that difficulty in a spirit to which nobody could have, and certainly I have, no objection.

11629. I think I should say that yours is not an extraordinary position. Upon every Commission relating to an Act of Parliament there is some member representing the administering department. It is quite usual. It is only that this happens to be a case in which feelings are moved more than they are, perhaps, in regard to motor-cars or London traffic?—It puts me somewhat at a disadvantage.

TWENTY-SIXTH DAY.

Wednesday, 10th July 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MACFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. C. J. MARTIN, M.B., D.SC., F.R.S., called in; and Examined.

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11630. (*Chairman.*) You are, I believe, Director of the Lister Institute of Preventive Medicine?—Yes.

11631. Chairman of the Anti-Typhoid Inoculation Committee, appointed by the Secretary of State for War?—Yes.

11632. A member of the Advisory Committee for the Investigation of Plague in India, appointed by the Secretary of State for India?—Yes.

11633. A member of the Tropical Diseases Committee of the Royal Society?—Yes.

11634. And you were formerly Professor of Physiology in the University of Melbourne?—Yes.

11635. At present your principal post is that of Director of the Lister Institute of Preventive Medicine?—That is the only post I hold.

11636. I think you appear before us at the request of Professor Starling's Committee of Delegates of Medical and Scientific Societies?—I was asked by that committee to attend in order that the evidence should not be repeated over and over again, their object being to arrange which persons would be most suitable to present particular pieces of evidence.

11637. I am afraid it is impossible at this stage of our inquiry to hope that everything you tell us is something that we have not heard before, but no doubt there is a great deal that we shall learn from it. You propose to give us evidence under several different heads, and the first head, I see, you propose to speak about is the relation of experiments on animals to the knowledge of the causation of infectious diseases?—Our knowledge of the causation and control of them.

11638. Would you give us your views upon that point, and we will interrupt you if we think it necessary at any point?—Our knowledge of the causation of infectious diseases has been gained by experiments on animals. By the same method has been discovered the means whereby the causal agents exert their injurious influence on the body, and the means whereby the animal body defends itself against attack. I am unable to conceive any other method by which this knowledge could have been gained. It is only by the employment of the experimental method that medicine, like other sciences has been enabled to emerge from quackery.

11639. Is not that rather a strong phrase?—I am using "the experimental method" as a philosophic term.

11640. I meant the word, "quackery," which rather infers that all knowledge not discovered by the experimental method is quackery?—By "quackery" I mean practice not based upon knowledge.

11641. All knowledge derived by clinical observation you think is quackery?—But the experimental method includes that.

11642. If you include clinical observation in the words "experimental method," I understand what you mean?—That emerges from the next sentence.

11643. When you speak of the experimental method, you do not mean the experimental method on animals?—Not exclusively. Since the function of medicine is the treatment of disease—*i.e.*, of derangement of functions in living organisms, the only possible way in which the experimental method can be applied to the study of medicine is by observation and experiment upon living beings. That, of course, includes clinical and pathological observations upon man. The fundamental conception of the nature of infectious diseases we owe to Pasteur, who established the truth of his conception by experiments upon animals. Pasteur first showed, in a series of epoch-making researches into the nature and causation of fermentations, that each fermentation (*e.g.*, alcoholic lactic acid, acetic, etc.) was due to the observation of a specific micro-organism. If the germs of one particular organism were sown in a nutrient fluid the products of one particular fermentation resulted. His experience on this subject led him to the great generalisation that infectious diseases might themselves be interpreted as particular fermentations, and as due to specific micro-organisms. By a series of masterly experiments upon animals he established the truth of his hypothesis in the case of anthrax, chicken cholera, and swine erysipelas. These results of Pasteur's may be regarded as the foundation of the whole modern study of contagious diseases both in man and in animals; and their extension by Pasteur and his pupils, and by bacteriologists and pathologists all over the civilised world, has led to the discovery of the causation of most of the infectious diseases to which man is liable. It can hardly be disputed that if we are to acquire control over any disease we must have knowledge of the agent which is responsible for its production, and that if we are to frame measures to hinder the ravages of any disease, we must study (1) the life history of the micro-organism which is the exciting cause of the disease; (2) the conditions under which it can live and flourish; (3) the conditions which are detrimental to its existence; and (4)

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the circumstances by which we may modify its virulence. An accurate knowledge of the life history of the micro-organism in question is indispensable, if we are to trace in detail how the disease is transmitted from one individual to another, whether directly from individual to individual, or indirectly by other living organisms, or by dead material. Only by the possession of such knowledge can we hope to devise the best means of hindering the extension of epidemics. In a certain number of diseases this knowledge is already at our command, and has afforded means of stamping out these diseases from places where till recently their ravages were responsible for many deaths annually—e.g., malaria, yellow fever. Moreover, knowledge of the nature of the exciting causes, their conditions of existence, and the reaction of the animal organism to their introduction, has already led in some cases to the finding of a curative agent, and has indicated a way by which we may hope to gain curative powers over other infectious disorders. Even in certain cases where the causation of the disease was not directly established by experiments on animals, because no susceptible animal was available—e.g., in typhoid fever—the discovery of the causation of the disease in man was indirectly due to such experiments, since the experience gained by Pasteur, Koch, and others on animals with regard to the characters of specific infectious disorders, has enabled us to correlate our observations on man, so as to establish the casual nexus between the typhoid bacillus and the disease. Indeed, as a result of animal experimentation a new science can be said to have grown up, the science of infection. It is by experiment that Robert Koch and his pupils alone have during the past few years succeeded in establishing the causation of the following common human ailments—namely, tuberculosis, cholera, typhoid fever, diphtheria, erysipelas, tetanus, and pneumonia. In support of the above general statement, I will give a few instance from the history of scientific discovery during the last half-century.

11644. You have referred to the success in establishing the causation of certain common human ailments, and the first you name is tuberculosis. Will you tell us now what you have to say about that?—Amongst the inhabitants of temperate climates tuberculosis is responsible for more deaths than any other cause. Although the mortality from tuberculosis has declined greatly of late years, during 1903 58,150 persons, or 11.3 per cent. of the whole mortality, died from this cause in England and Wales. Tuberculosis is also a common disease of food animals. Up to the middle of the nineteenth century phthisis and other manifestations of tuberculosis were not recognised as connected, and although some clinicians, on the basis of their experience, suggested that phthisis was a contagious disease, this was not generally recognised. The prevailing view was that phthisis and other forms of tuberculosis were the results of constitutional peculiarities capable of hereditary transmission. It was realised that by keeping the individual in the most favourable surroundings for the maintenance of robust health, the onset of such manifestations was in many cases delayed, the explanation of such delay being that the patient had "outgrown the tendency to the disease." It was not known that the essential cause of tuberculosis was introduced from without, and, consequently, it was not recognised as a preventable disease. In 1843 Klencke* by experiments on animals, showed that tuberculosis could be conveyed from one animal to another by inoculation, and in 1865 Villemin†, by carefully-conceived and conducted experiments on animals, proved that tuberculosis was an infective disease due to a specific virus of a parasitic nature, which was introduced from without. He showed that the virus was capable of mutiplying indefinitely in the bodies of animals, and of being handed on from one animal to another by inoculation. He also demonstrated by experiments on animals the casual unity of the numerous manifestations of tuberculosis, such as phthisis, caseous pneumonia, scrofula, lupus, and joint disease, etc., and also the important fact that "grapes," a common disease of cattle, was due to the same virus.

11645. What is caseous pneumonia?—Cheesy.

11646. Why is it so called?—Because it was a pneumonia with a formation of patches of degenerated tissue of a cheesy nature. At the time of Villemin's discovery the prevailing view of the medical profession was that these diseases were the products of deranged functions. Much controversy occurred for several years, until the confirmatory experiments of Cohnheim and Salomonsen (Sitzungsberiche des Schles. Gesellsch. fur vater land Kultur. 15 July, 1877) secured the adhesion of medical opinion. In 1881 Koch, following the lines laid down by Pasteur, who, by experiments on animals, had established the cause of splenic fever in stock, discovered, isolated and cultivated a bacillus in the tissues of men and animals which had succumbed to tuberculosis. By a series of experiments upon animals Koch demonstrated this bacillus to be the virus of tuberculosis. Having ascertained the casual agent in tuberculosis, Koch (Berlin Klin. Wochsch, 1832, p. 221. Mitth. a.d. Kais. Gesundheits amte, 1884, Bd. 2, p. 22) was enabled to determine the conditions under which it could live outside the body, and to discover the paths by means of which the infective agent leaves a diseased animal and obtains entrance into a healthy one. Further, this discovery placed the casual unity of the very various manifestations of tuberculosis disease upon a sound basis, and permitted a certain diagnosis to be made in early and obscure cases.

11647. You then proceed to puerperal fever?—Puerperal fever is a disease which attacks women after parturition. After a mother has borne all the troubles of pregnancy, has passed through the pains of child birth, and is filled with happiness at the birth of an infant, she is liable to be stricken down with this terrible disease, and perhaps destroyed by it within a few days. In the years 1760-1770 puerperal fever prevailed in London and Edinburgh to such an extent that in some of the lying-in institutions nearly all the patients died; and in Paris, as late as 1866, over 25 per cent. of the women confined at the Maternité died of this disease; so that lying-in hospitals were looked upon by women of the lower classes as the vestibule of death. In England, during the years 1847-1903, 93,243 mothers died of this disease. From the time of Hippocrates down to the middle of the 19th century, with the exception of a few physicians who were more capable than their brethren of analysing their experience and drawing accurate conclusions (notably Dr Alexander Gordon, of Aberdeen, in 1795), the general view as to the causation of puerperal fever was that it was due to the suppression of the serous discharge which invariably escapes from the raw surface of the uterine wall during the few days following parturition. I mention this to show how the human intellect is prone to confuse effect with cause, when the true nature of the latter is not established. About the middle of the 19th century Semelweiss, in Vienna, and Oliver Wendell Holmes, in America, drew the attention of the profession in these places to the fact that an analysis of the experience of puerperal fever in lying-in hospitals and in midwifery practice indicated in the strongest manner that this disease was of an infectious nature, and conveyed from one woman to another by the agency of the doctor or midwife. The opinions of both of these men were not, however, generally accepted. One of the principal of opponents of Holmes' view was the Professor of Midwifery at Philadelphia in 1852. In his criticism of Holmes' paper he says: "I prefer to attribute them (the cases of puerperal fever) to accident, or Providence, of which I can form a conception, rather than to a contagion of which I cannot form any clear idea." Holmes' critics were able men, and I quote the above to show the necessity of supplying direct concrete proof of the nature of a contagion in order to satisfy the minds of most men.

11648. Holmes' opinion was not formed from experimenting, I understand?—It was formed from observation of experiments that were unwittingly performed ready for him by the doctors and nurses in lying-in hospitals. In 1878 we find the Academy of Medicine of Paris formally discussing the causation of puerperal fever. It was at this meeting that Pasteur, after listening to a variety of views as to the causation of this malady, interjected "None of those things cause the epidemic; it is the nursing and medical staff who carry the microbe from the infected woman to a

* *Untersuchungen und Erfahrungen im Gebet der Anatomie, Physiologie und Mikrologie. Wissenschaftlichen Medecin. Leipzig*, 1843. *Bd.* 1.

† *Cause et Nature de la tuberculose. Bull. de l'acad. de Médécine*, 5 Dec., 1865, & 30 Oct., 1866. *Etu dessur la tuberculose, preuves rationnelles et expérimentelles de la specificité et de son inoculabilité. Paris*, 1868

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healthy one." The orator having remarked that he feared that microbe would never be found, Pasteur went to the blackboard and drew an organism growing in chains (the strepto-coccus) which he had isolated from the discharges of a number of patients suffering from puerperal fever, and from tissues of those dying of this disease. He also found the same organism in the blood obtained by a prick of the finger in similar cases, shortly before death. Pasteur believed, from analogy with his observations on anthrax and other diseases, that this organism would be found to be the cause of the disease. This was subsequently proved by experiments upon animals. Pure cultures of the organism discovered by Pasteur injected into rabbits multiplied rapidly, and spread throughout their blood, causing the death of the animal with fever and suppuration; a drop of the blood of a rabbit dying from this inoculation, if rubbed into a scratch upon another animal, occasioned its death in a similar manner.

11649. Do I correctly understand that this coccus poisons persons, and will kill persons who are not child-bearing?—Certainly. The conditions following child birth offer a very large surface for the entry of any organism; but the disease in common to all forms of wounds.

11650. You mean that the same coccus causes those diseases and others?—Yes.

11651. It is not peculiar to puerperal fever?—Not at all. The nature of the contagion in puerperal fever having been recognised, the disease almost entirely vanished from the maternity hospitals of Europe and America. The result was accomplished by avoiding every opportunity whereby the infection might be conveyed to a woman during labour, and by employing those principles of antiseptic surgery which we owe to Lister. The antiseptic system of Lister was a direct extension to wound infection of Pasteur's work on the nature of fermentation and putrefaction.

11652. Then we come to diphtheria. Will you tell the Commission what you have to say about that disease?—Diphtheria, a name introduced by Brettoneau for a series of inflammatory diseases of the throat, windpipe, and nose, associated with the development of a membranous exudation, has long been known to be an infectious disease. It is a disease more particularly of childhood, the course of which the physician found himself until recent times powerless to influence. He could only do what little was possible to diminish the sufferings of the patient, and when the larynx became obstructed, establish an entrance and exit for the respiratory air by tracheotomy, or intubation. In 1875 Klebs (*Arch. f. exper. Path. Bd. iv.*) found a bacillus hitherto unknown, in the membraneous exudation of cases of diphtheria. This bacillus did not occur alone, but associated with a variety of other micro-organisms, and it was doubtful how far this particular bacillus was responsible for the disease. In 1882 Loeffler (*Mittheillung a.d. Kais. Gesundheits amte. Bd.* 2, 1884) succeeded in isolating Kleb's organisms from the diphtheritic membrane and growing it upon solidified blood serum. Once isolated, the Klebs-Loeffler bacillus was shown by experiments upon animals (rabbits, guinea pigs, and pigeons) to produce a disease presenting a resemblance to diphtheria, including the characteristic paralysis when the animal survived the acute symptoms (Loeffler, Roux, and Yersin) (*Ann. l'Inst. Pasteur,* 1888). As had been found to be the case in man, the bacillus occurred only at the point of inoculation. This fact led Roux and Yersin (*Ann. de l'Inst. Pasteur,* 1888, 1889, *and* 1890) to the conclusion that this bacillus must produce a poison which was absorbed into the circulation. They therefore grew the bacillus in broth, and discovered that after the separation of the germs by filtration through porcelain, the fluid contained a powerful poison, which, when inoculated into animals, produced all the effects of the bacilli themselves. Thus was diphtheria toxin discovered; but, what was far more important, a method whereby a pathogenic organism exerts its deleterious effects was brought to light. Roux and Yersin's experiments also led to the discovery by Behring (*Behring and Kitasato Deutsche Med. Wochschr,* 1890. *Behring and Wernicke Feilscript f. Hygiene,* 1892), through experiments on animals, of a physiological process which is brought into play by the animal body to counteract the influence of the germs of some diseases. This process consists of the production, through the agency of its own tissues, of a substance which neutralises the poison produced by the microbes. When this substance is manufactured by the cells of the animal in sufficient quantity and with sufficient rapidity, the animal is able to successfully resist the onslaught of the diphtheria bacillus, and recovers from the disease. These observations, in their turn, have led to the development of curative means in imitation of nature's processes (serum-therapy), of great value to humanity.

11653. We now come to tetanus?—Tetanus or lock-jaw is a malady common to man and most animals. Of the latter the horses are particularly susceptible to this disease. The sufferings caused by the intense muscular spasms, which are characteristic of the disease, renders tetanus one of the most horrible maladies one can witness. The mortality from tetanus is 80 per cent. In fatal cases the spasms involve the respiratory muscles to such an extent that the patient dies of suffocation. Since the introduction of the antiseptic treatment of wounds the incidence of this disease has been greatly reduced, but formerly it was much more frequent. It has long been known that tetanus was particularly prone to follow badly lacerated wounds, especially such as became contaminated with soil or road dirt. The danger of tetanus after gunshot or other wounds in battles was formerly very considerable, and when once the infection started in an extemporised military hospital it spread so extensively as sometimes to infect every-one of the wounded. According to the German statistics in the Franco-German war, tetanus occurred in 0.35 per cent. of all wounds (*quoted by Vaillard article Tetanus maladies commes à l'homme et aux animaux, Paris,* 1906, *p.* 351). Tetanus has also been responsible for a heavy infantile mortality, especially in maternity hospitals. In the Rotunda Hospital, at Dublin, up to 1882, of 17,650 infants born, 2,944 died of this disease (*quoted from Clarke by Vaillard, loc. cit. p.* 348). This mortality could have been entirely prevented if the cause and means of spread of the infection had been known. That tetanus was a disease due to infection by a specific micro-organism was conjectured from the time that the microbial causation of anthrax and chicken cholera had been established by Pasteur. The infective nature of the disease was established by Carle and Rattone in 1884 (*Studio sperimentali sull' etiologia del tetanos. Giorn. della R. Acad. di Medicina di Torino,* 1884), who succeeded in producing tetanus in animals by injecting a small quantity of the pus from the wound of an animal suffering from this disease. The discovery of the causal agent was not, however, determined until 1889, when Kitasato (*Deutsch Med. Wochschr,* 1889, *No.* 31) succeeded in isolating and cultivating the same bacillus from the pus of wounds in cases of tetanus, and proved, by the injection of pure cultures of the organisms into animals, that it, and it alone, occasioned the disease. This bacillus had been previously seen by Nicolaier in 1884 (*Deutsch Med. Wochschr,* 1884, *No.* 52), in the local wound of animals which he had succeeded in infecting with tetanus, by the inoculation of a minute fragment of garden earth, and also by Rosenbach (*Arch f. Klin Chir,* 1885), in the pus from a wound in the case of a man suffering from tetanus. These discoveries explained how the infection of tetanus could be conveyed from one wound to another, and why soiling of wounded surfaces with earth is particularly liable to occasion the disease. Having isolated the bacilus of tetanus, Kitasato was enabled to precisely ascertain, firstly, where and under what conditions it flourished in nature; and, secondly, those conditions which destroyed it. The bacillus of tetanus remains in the situation at which it finds entrance into the tissues. Under suitable conditions it multiplies in the wound, and produces its characteristic effect upon the nervous system by means of a poison which it secretes. By growing Kitasato's bacillus in broth and filtering through porcelain, which does not admit the organisms to pass, but only the soluble products of their activity, Faber, in 1890 (*Bertin Klin Wochschr,* 1890, *p.* 717), obtained a poison, minute quantities of which, when injected into animals, produced the disease (1-100,000 of a cubic centimetre). All these investigations have afforded knowledge of the cause of tetanus, the life history of the causative bacillus, the conditions favourable or otherwise to its existence, the means whereby it may be destroyed, and the manner in which the symptoms of the disease are brought about. By taking advantage of this know-

ledge, epidemic outbreaks of the disease can be absolutely prevented, and the great majority of wounds where contamination with soil has rendered them liable to infection with tetanus bacilli can be successfully dealt with. In the recent South African War only three cases of tetanus occurred. Further, the animal experiments of Behring and Kitasato (1890) (*loc. cit.*) showed that by repeated small but increasing doses of the soluble tetanus toxin, animals can be rendered immune to this poison, and that the serum of such highly immunised animals renders inoffensive the tetanus poison with which it is mixed. The serum of horses which have been gradually accustomed to this poison until they support, without ill-effects, quantities many thousand times that required to kill horses not so treated, is a valuable therapeutic agent in human and veterinary surgery. This subject will, however, be referred to in greater detail later.

11654. You now have something to tell us about plague?—Plague is a disease of great antiquity, and previously seen by Nicolaier in 1884, in the local wound some of the earliest human documents extant record the ravages of this pest. During 1905, 1,040,429 persons succumbed to plague in India. Over 350,000 deaths occurred in the Punjab alone (*official returns*). In some of the villages of this province 75 per cent. of the population were destroyed in two years. The mortality amongst Asiatics varies between 70 and 80 per cent., and amongst Europeans at the present time it is over 35 per cent. of those attacked. As long as the causation of plague was not comprehended, and the means by which it is spread were unknown, suitable preventive measures could not be rationally prescribed. Consequently, even in recent times, wealth and human effort have been squandered in directions that have led to no useful result. The constant association of a mortality of rats and mice, with outbreaks of plague amongst human beings, has been forced upon the minds of many ancient people, and is found to exist among many primitive races of mankind at the present time. The true significance and importance of this observation could not, however, be realised until the causation of the disease was discovered by Yersin and Kitasato (*Lancet*, 1894) in Hong Kong in 1894. These observers found a bacillus constantly present in the swollen lymphatic glands (buboes) of patients suffering from the disease, and they succeeded in growing it upon artificial media. Inoculation of the cultures of this organism into animals reproduced the disease with all its characteristic symptoms. Once the cause of the disease was known, it was easy to determine that the disease from which rats and mice simultaneously suffered was also plague. All the conditions, favourable and unfavourable, to the existence of the infective agent, could now be studied, and the means whereby the infection leaves the bodies of persons or animals dead of the disease, could be determined. The relationship in time and space between human outbreaks and disease in rodents could be worked out, and the influence of epidemics amongst rats in the spread of the disease among human beings determined. The relationship of rat plague to human plague has been found to be of such a character as (taking into consideration the small extent to which bubonic plague is infectious from man to man) to indicate in the strongest manner that the spread of the disease in man is conditioned almost entirely by the occurrence of plague amongst rats in his vicinity. The Commission at present investigating the question of the spread of plague in India, have, by extensive experiments upon animals, succeeded in showing that the infection is carried from rat to rat by the agency of the fleas infesting these animals (*J. of Hygiene*, 1906 *and* 1907, *special numbers*). By the same agency they have been able to produce epidemics amongst other animals, guinea pigs and monkeys; whereas all other means failed to give rise to the epidemic spread of the disease. The Indian rat flea also feeds upon man when his natural prey is not available, so that a possible, and indeed probable, means whereby the infection is carried from rats suffering from the disease, to mankind has been established. Further experiments carried out in houses or huts, where many cases of plague occurred, have shown that animals (guinea pigs or monkeys) are attacked by the disease when placed in such infected huts, without protection from fleas; but if the simplest means is adopted to prevent the fleas from reaching the animals, by a gauze covering, or a layer of sticky paper wider than a flea can jump, or by suspending the cage at a distance from the floor greater than a flea can leap, they have in no case contracted plague.

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11655. Whose are these experiments with fleas?—The experiments of the present Commission which is investigating the plague in India, of the Advisory Committee of which I have the honour to be a member.

11656. Do these fleas which carry the plague from one to another get the plague themselves?—No, they do not suffer in health apparently. The plague microbes merely multiply inside their alimentary canal, without apparently detrimentally affecting them in the least.

It need hardly be insisted upon that the complete comprehension of the spread of the disease under natural conditions is essential to successfully devise preventive measures. Without such knowledge our efforts will, in all probability, be misdirected. A knowledge of the causation of plague has enabled a useful method of protective inoculation to be devised. Efforts towards the production of a curative serum have unfortunately, so far, been disappointing, as the means employed have not produced an anti-serum of sufficient potency.

11657. Your next head of evidence is the importance of experiments on animals in the diagnosis of infectious diseases?—The value of a means of arriving at an early diagnosis in the case of infectious disease of man and animals is considerable, because the physician or veterinary surgeon is, as a rule, unable to form a diagnosis until the disease has developed. This may mean a delay of weeks, or even months, during which precious time is lost, and precautions to prevent the spread of the disease omitted. Moreover, in mild or latent cases of a disease, it may be quite impossible to arrive at a diagnosis by ordinary clinical means. The advantages of early bacteriological diagnosis are not confined to the patient, but are of importance to the community, or the owner of stock or stable, for upon the early recognition and segregation of cases of infectious maladies, the probability of successfully stamping out the disease depend. Mild or abnormal cases of disease may be equally endowed with the power of spreading the infection. Recent researches into the epidemiology of diphtheria, typhoid, and cholera, have indeed proved, that persons convalescent from these diseases and in all respects perfectly healthy, may be innocent agents distributing broadcast these various infections. In many cases a bacteriological diagnosis of a disease can only be made by direct appeal to animal experiment; but in quite a number a simpler method by which this is avoided, but which has been arrived at in the first instance by experiments upon animals, can be employed. As instances of such methods I cite: (1) The recognition under certain conditions, of tuberculosis, anthrax, relapsing fever, diphtheria, syphilis, gonorrhœa, plague, and sleeping sickness, by observations upon the morphological staining and growth characters of the organisms contained in the material submitted to examination. (2) By taking advantage of the fact that the human and animal body when infected with certain diseases, *e.g.*, typhoid fever, cholera, and malta-fever, elaborates substances in the blood which have the property of causing the individual organisms of these diseases to aggregate together into clumps, so-called agglutinins. As this agglutinating action is within certain limits only exercised upon the particular species of microbe which is responsible for the disease, a diagnosis may be arrived at. For this purpose a single drop of blood from the patient is adequate. (3) In the case of cholera and typhoid infections a further substance is present in the blood serum, which has the power of killing and dissolving up the respective bacilli. This action is specific, so that the test applied with a few drops of blood can be used for diagnosis. (4) Nos. 2 and 3 can be applied in the reverse direction, viz., to identify microbes suspected of being typhoid or cholera, which have been separated from water or the dejecta and other excretions or discharges from the human body. In this case cultures of these organisms are submitted to the action of a small quantity of the serum, obtained from an animal immunised against infection with typhoid or cholera microbes. (5) The diagnosis of tuberculosis in cattle and of glanders in horses, in the early stages of the disease, when it cannot be recognised by clinical signs, can be made by the infection of a small quantity of tuberculin or mallein respectively. Tuberculin and mallein are the products which are formed by the growth of tubercule and glanders bacilli

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in broth. The organisms themselves are killed by boiling, and are then removed by filtration. Tuberculin and mallein in the quantities employed produce no effect when injected under the skin of healthy animals, but if the animals be suffering from the corresponding diseases, fever, and in the case of mallein, local swellings occur, thereby enabling a diagnosis to be made. They are extensively used to ascertain the existence of tuberculosis in dairy cattle, glanders among horses in large stables in the early stages of the disease. In the cases of glanders and tuberculosis the disease may exist for a long time in a latent form, the animals appearing during this stage to be in perfect health. In such animals the disease is liable to assume an acute and highly infective character. It is, therefore, of great advantage, especially to the owner of valuable stock, to be able to recognise the disease in this form, so that the infected animals may be removed from the vicinity of healthy stock.

11658. Your third head of evidence is the preventive and curative measures discovered by experiments upon animals, and requiring the inoculation of animals for their production and standardisation?—I begin with hydrophobia (rabies). Pasteur discovered that it was possible to modify the virulence of the microbes of chicken cholera and anthrax by growing them outside the animal body under particular conditions. He also found that the inoculation of such attenuated cultures gave rise to merely slight *malaise*, from which the animal speedily recovered. On subsequently inoculating those animals which had received cultures of diminished virulence with microbes of full virulence, he discovered that they had become possessed of a marked degree of resistance to the disease. On the basis of this observation, Pasteur, by carefully controlled experiments on animals, devised a method of protective inoculation against chicken cholera and anthrax. This Pasteurian vaccine has been extensively employed in many countries, and has been the means of saving wealth to stockowners. Pasteur had continuously before him the possible application of his discoveries in the sphere of animal diseases to the alleviation of human suffering. Amongst those diseases which had particularly appealed to his imagination was the tragedy of hydrophobia. In the case of an individual bitten by a dog suffering from rabies, no remedy was available apart from local cauterisation. The patient remained in suspense for months fearing the onset of this fatal disease. Reasoning from the analogy afforded by his previous discoveries, Pasteur thought it might be possible to take advantage of the interval between the infection and the development of the symptoms in hydrophobia to produce in the person bitten a sufficient resistance to the disease to prevent its development altogether. Pasteur commenced this inquiry in 1881 in conjunction with Roux, Chamberland and Thullier (*Comptes rendus*, 1882). His first efforts were directed to the isolation of the casual agent. In this he was unsuccessful, and it has not yet been accomplished. He discovered, however, that the virus existed in the nervous system of rabid animals in a pure form, and that it became attenuated by slowly drying it for two or three weeks at a temperature of 20 deg. C. He next showed that by previously inoculating his attenuated virus under the skin of dogs, these animals did not contract the disease when subsequently bitten by rabid dogs. Pasteur had shortly before shown that the inoculation into the brain of a dog of a minute portion of the nervous system of an animal which had succumbed to virulent rabies, invariably gave rise to the disease. Nevertheless, previous treatment of a dog with his attenuated virus was able to prevent the development of the malady in this case also. Having shown that it was possible to prevent the disease by treatment, previous to the inoculation with virulent virus, he tested the influence of his method when applied during the period between the infection and the development of the disease (*Pasteur, Chamberland and Roux, Comptes rendus*, 1885). The results were entirely successful, and in 1885 Pasteur was induced to try his method upon the human subject. Of Pasteur's first 350 patients, only one died. This was a little girl horribly bitten about the head and face 37 days before she arrived in Paris. Pasteur felt constrained to attempt his treatment upon this child, although the severity and situation of the bites, and the long interval before treatment began, rendered the case hopeless. Since then anti-rabic inoculation has been employed all over the civilised world, wherever the disease has existed. There are at present 51 institutes for the carrying out of the treatment, distributed over Europe, America, Asia, and Africa. Between 1886 and 1905 more than 150,000 persons have been inoculated. The result of the treatment at the Pasteur Institute, Paris, are published each year in the "Annales de l' Institut Pasteur" under three categories.

11659. You say there are 51 institutes. Can you tell us how many there are in the United Kingdom?—None. We have the good fortune to live on an island, and it was possible to get rid of hydrophobia by muzzling the dogs.

Those three categories are:—(a) Persons bitten by dogs, etc., proved to be suffering from rabies by animal experiment; (b) Persons bitten by dogs, etc., certified by veterinary surgeons to have rabies; (c) Persons bitten by dogs, etc., suspected to have rabies. The number of persons treated at Paris, and the mortality since the introduction of the treatment is given below:—

Year.	Cases Treated.	Deaths.	Mortality per cent.
1886	2,671	25	0·94
1887	1,770	14	0·79
1888	1,622	9	0·55
1889	1,830	7	0·38
1890	1,540	6	0·32
1891	1,559	4	0·25
1892	1,790	4	0·22
1893	1,648	6	0·36
1894	1,387	7	0·50
1895	1,520	5	0·33
1896	1,308	4	0·30
1897	1,521	6	0·39
1898	1,465	3	0·20
1899	1,614	4	0·25
1900	1,420	4	0·35
1901	1,321	6	0·38
1902	1,105	2	0·18
1903	628	2	0·32
1904	755	3	0·39
1905	727	3	0·41

The figures for the various other anti-rabic institutions are available, and the results are in most cases equally satisfactory. In the 150,000 persons treated in different parts of the world the mortality has been less than 1 per cent., and in most institutes less than ½ per cent.* Rabies is a fatal disease, but it is impossible to estimate exactly how far persons bitten by animals known, or suspected to have been suffering from rabies, are liable to develop the disease. The reason why this cannot be done is that the circumstances, situation, and nature of the bites vary so greatly in different cases. I have gathered together all the reliable information I could get to form an estimate. Bouley, in an official inquiry into the history of 320 cases made under the Second Empire, found that 40 per cent. of the persons bitten contracted the disease. Leblanc, 1878-1883, head of the sanitary department of the Prefecture of Police in Paris, concluded from an examination of 515 cases that it was 16 per cent.; Faber (Wurtemberg), from 145 cases, 20 per cent.; Werner (Vienna), (1879-1886), from 429 cases, 11.8 per cent.; and Hogyes (Buda-Pesth), from 470—9.3 per cent. That was the case

* *The average mortality of the 150,000 cases indiscriminately collected by myself from all the sources readily accessible to me is ·5 per cent.—C. J. M.*

when I prepared my statement; but including later details Hogyes (Buda-Pesth) concluded from 855 cases that it was 13.9 per cent. If all the cases are added up together, and the number of deaths, the average comes to 17.9 per cent. as the estimated mortality when bitten by a rabid dog. That estimate is based upon 2,264 cases.

11660. I thought these were percentages of the persons bitten who had contracted the disease. Is this last figure the average mortality?—Yes. I have added the number of cases observed by each man together, and the number of deaths returned by each man.

11661. (*Sir John McFadyean.*) Of persons untreated?—Yes, this is of persons untreated.

(*Chairman.*) It is not quite clear. You begin by saying that Bouley, in an official inquiry into the history of 320 cases made under the Second Empire, found 40 per cent. of the persons bitten contracted the disease.

(*Sir John McFadyean.*) They all died.

11662. (*Chairman.*) It does not say so. That is assumed?—In the paragraph before I stated that rabies is a fatal disease.

11663. (*Sir John McFadyean.*) You might say "invariably"?—I will put, if you think it is stronger, "invariably fatal."

11664. (*Mr. Tomkinson.*) Rabies in the dog?—No rabies in man.

11665. Do you not call it hydrophobia?—If you like I will call it hydrophobia—hydrophobia is an invariably fatal disease in man.

11666. But dogs do not have hydrophobia; the dog has rabies?—Rabies is one name for the disease, and hydrophobia is another.

11667. Is it correct to use the word hydrophobia in the case of a dog?—I think so, certainly.

11668. I thought not. I thought the dog did not have the fear of water.

11669. (*Sir William Collins.*) Do all these figures refer to the period prior to inoculation?—No; some figures are collected later; but they are all of cases which have not undergone treatment.

11670. But your figures in the Table start with the year 1886?—Yes.

11671. Is that what you regard as the commencement of the inoculation period?—Yes; but everybody does not undergo treatment for rabies even now.

11672. Would you be so good as to answer that question? Do you regard 1886 as the period from which the inoculation method was introduced?—Certainly.

11673. Do the figures which you have just given us in regard to the number of cases that were bitten and attacked relate to the pre-inoculation period?—Not entirely.

11674. Do all except those that you have just added with regard to Buda-Pesth?—No, not entirely.

11675. It seems desirable to separate those, because there may be a question whether they are all of the pre-inoculation period or whether you are adding some cases now which though not treated belong to the post-inoculation period?—In the case of Buda-Pesth that is so.

11676. Then will you be so good as to indicate in what cases you are dealing with the post-inoculation period?—I am not certain off-hand about Faber's at Wurtemberg; but I believe those were of pre-inoculation date.

11677. Then are there any except the additional cases which you have added from Buda-Pesth, which were of the post-inoculation period?—I am not sure about Faber's, without looking the thing up again.

11678. That is the only exception?—Obviously for the others I put the dates in

11679. I gathered from the dates that you were purposely obtaining cases which were pre-inoculation cases; then when you added the Buda-Pesth cases I wondered whether they related to post or to pre-inoculation. I understand now that they relate to post-inoculation?—Yes, but untreated.

11680. (*Sir John McFadyean.*) I suppose you really took all the statistics that you could find on that subject?—Yes.

11681. You did not select them because they were of the pre-inoculation period?—No.

11682. There have been no records made of those of Bouley and others?—No.

11683. You could not find any in the literature to the same extent?—No.

11684. Are you sure that the Pasteur treatment only began in 1886. It would be rather extraordinary if in the first year's treatment 2,671 persons were treated. Were not those the first published statistics merely?—Yes, you are right; I have previously stated that Pasteur commenced to treat patients in 1885. The return for 1886 includes the year before, too.

11685. (*Sir William Collins.*) But you state the mortality since the introduction of the treatment began with the year 1886?—Yes, that is the first year for which the returns were published and are available.

11686. (*Sir Mackenzie Chalmers.*) But still they are untreated cases?—Yes.

11687. Does it make any difference whether they were bitten before or after the invention, as long as they were not treated?—I imagine not.

11688. I do not know. It might?—I see no reason why it should.

16689. There is just this point, that the more severe cases might be treated, and the untreated cases would be the more doubtful cases?—That might be so, of course. But that will be in favour of my argument.

11690. I know?—I can only deal with the figures as they are available.

11691. (*Sir William Collins.*) And it might not be so?—It might not be so.

11692. (*Sir John McFadyean.*) There is just one other question I should like to ask you. I should like to be quite clear whether these statistics relate to people who were bitten merely by dogs suspected of rabies; because many returns distinguish between persons bitten by dogs suspected of rabies and those bitten by dogs experimentally proved to have been rabid or certified by a veterinary surgeon to have been rabid. The older statistics were not experimentally proved. They were on the opinion of the veterinary surgeon. The most careful statistics, so far as I can judge, are the last ones from Buda-Pesth, from Hoygés. He was the director of one of the biggest Pasteur Institutes in the world, which is situated in Buda-Pesth. And in Hungary hydrophobia is particularly important as a disease.

11693. (*Sir William Collins.*) But would the figures which appear in your statement be correct, that of the 470 case of Hogyes an attack rate of 9.3 per cent. was observed?—That was in the first 470 cases.

11694. Would you give us separately what the additional cases are which you now add which produce an attack rate of 13.9 per cent. in the total?—I cannot do it now.

11695. It will complete the statement?—Yes.

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In searching the literature in order to comply with the request of the Commissioner, I have discovered further figures relating to the mortality amongst persons bitten by dogs judged to have been suffering from rabies and not treated by the Pasteurian inoculations. The list giving date and place of observations and source of information is as follows:

Figures collected by.	Locality.	Date.	Number of Cases.	Number of Deaths.	Mortality (per cent.)	Source of Information.
Faber	Wurtemberg	1846	145	28	20	Hogyés article "Lyssa" Nothnagel's Spezielle Pathologie u. Therapie Bd. V. T. 5. p. 829.
Bouley	France	1860–70	320	128	40	Life of Pasteur. Valerie-Radot English translation, p. 267.
Leblanc	Department of Seine.	1878–1883	515	81	16	Menetrier, article "La rage" Maladies Communes a l'homme et aux animaux, Paris, 1906.
Werner, Vienna General Hospital	Vienna	1879–1886	429	44	10·3	Menetrier, article "La rage" Maladies Communes a l'homme et aux animaux, Paris, 1906.
Hogyés	Hungary (Official enquiry, Minister of the Interior)	1885–1888	470	44	9·3	Hogyés *loc. cit. supra.*
Hogyes	Hungary (Official enquiry, Minister of the Interior)	1890–1895	985	147	14·9	Hogyés *loc. cit. supra.*
Kurimoto	Nagasaki	1892	67	21	31·3	Kurimoto. Virchow's, Arch. 1899. Bd. 158. p. 148.
Kurimoto	Nagasaki	1893	145	15	10·3	Kurimoto. Virchow's, Arch. 1899. Bd. 158. p. 148.
Kurimoto	Nagasaki	1894	51	9	17·6	Kurimoto. Virchow's, Arch. 1899. Bd. 158. p. 148.

Total cases	3,127
Total deaths	517
Average mortality (per cent.)	16·5

I should like to interject that I have not selected these figures; they are simply all that are available to me for basing a judgment upon as to what the chance of death is if you are bitten by a mad dog. The average of the above is 17 per cent., and is the best estimate that I can arrive at. Assuming this as correct, and taking the mortality of patients treated as high as 1 per cent., the method of Pasteur has in 20 years saved upwards of 25,245 persons from the horrible disease of hydrophobia, and all the attendant suffering to their friends and relations, as well as relieving 148,500 persons of the awful suspense for six months, after having been bitten by a dog suspected of rabies.

11696 (*Chairman.*) You come next to serum therapeutics in connection with experiments on animals?—Many organisms, and notably those of diphtheria and tetanus, exercise their harmful influence by the secretion of a soluble poison or toxin, which is absorbed from the seat of infection and circulated in the body of the infected animal. One of the means of defence against the invasion of such organisms which is possessed by the animal body, is the capacity to manufacture and set free in the blood stream substances which combine with the toxins, and so render them inert. By habituating a large animal, such as a horse, to the action of the toxin in increasing quantities, some cells or fluids of its body can be thereby stimulated to produce antitoxins, which are then thrown into the blood stream. These antitoxins, when they have reached a sufficient strength, may be employed as a remedy for the disease in man or animals. Von Behring must be regarded as the founder of serum therapeutics. In 1890 he, with Kitasato (*loc. cit.*), showed that the blood serum of an animal, which had been immunised against tetanus, was capable, when injected into other animals, of protecting them, not only against poisoning by the tetanus toxin, but also against infection with living tetanus bacilli. He also proved that under certain conditions a curative action could be demonstrated in animals which already presented symptoms of tetanus infection. Similar though less striking results were described in the case of diphtheria. By showing that the horse reacted to repeated and increasing injections of diphtheria toxin, by manufacturing and circulating in its blood such quantities of antitoxin that a few cubic centimetres of their serum was possessed of sufficient curative power to be useful in the treatment of diphtheria in the human subject, Roux (*loc. cit.*) and Ehrtich first brought the subject from the experimental to the practical stage.

11697. How are the antitoxins prepared?—For the preparation of antitoxins on a large scale the horse is the animal now almost exclusively used. The horse can be suitably immunised against tetanus, diphtheria, and many other diseases and intoxications. In the case of tetanus, diphtheria, and dysentery the sera obtained attain to a very high antitoxic value.

11698. What precautions are taken in the selection of the horses for the preparation of antitoxin?—The horses are selected by a skilled veterinary surgeon. They must be free from all signs and symptoms of disease. Every horse is tested for latent glanders with mallein, and even a suspicion of a reaction is regarded as a sufficient basis for the rejection of the animal. If the animal has satisfactorily passed the test, he is received into the establishment, but is isolated and kept under observation for some time. Having passed through this period of probation, the animal is admitted into the stables, and the process of immunisation is commenced. During the process the animal is kept under the strictest observation—his temperature is duly registered, and the greatest possible care is taken in regard to his comfort and feeding.

11699. (*Sir William Church.*) Ought you not to qualify that statement by saying where these precautions are taken? It is rather a bold thing to say that they are taken everywhere. You have no means of knowing how strictly the precautions are carried out, except in certain establishments?—I know that these precautions are taken in our own institute; and I think I can safely say that they are taken in the other places where sera are made in this country—that is in Burroughes and Wellcome's.

11700. (*Chairman.*) Is that from what has been told you?—No; I have seen the way they work.

11701. You have visited their place?—Yes.

11702. (*Sir William Church.*) Would it not be safer to say that in this country the precautions are taken?

11703. (*Chairman.*) You mean in this country; you do not mean all over the world?—I am not aware of all over the world.

11704. At any rate, we had better confine ourselves to this country, because that is all we have to inquire into.

11705. (*Dr. Wilson.*) May I ask, do you use mallein after the horse is bled, for the production of serum? You always use it before; but do you ever use it while the horse is in the institute for experiment?—No. The mallein test is performed before the horse comes to us by the veterinary surgeon.

11706. What would be the effect of using it if used after the horse has been experimented on, after the toxin has been used for a horse that has been bled for some time?—I do not think there would be any effect.

11707. (*Chairman.*) At any rate, in your evidence you speak with confidence about your own institute?—Certainly.

11708. And as to other institutes in this country, I understand it to be your belief, from personal observation, that the process is carried out strictly there in the same way?—Certainly.

11709. Will you describe the process of immunisation?—The animal is treated with increasing doses of the particular poison. The toxins are injected subcutaneously, intramuscularly, or intravenously. At first either very minute doses of weak toxins, or toxins which have been modified by chemical agents, or in others ways, are employed. In the case of tetanus, in the early stages the toxin is usually modified by being treated with iodine. The first injection of the toxin may be followed by swelling at the site of inoculation, loss of appetite, general malaise, and rise of temperature. When these have passed off the animal receives a second rather larger injection, and in this way the quantity of toxin is increased until, within a few months, the horse is capable of tolerating many thousand multiples of what would be a lethal dose if given as a first injection. When the serum has reached the strength suitable for clinical use, blood is withdrawn from time to time by venesection. All the processes connected with immunisation are carried out under the strictest antisepsis.

11710. What precautions are employed in preparing the different sera?—The vessels containing the sterile blood are removed into a small isolated building used for this purpose only. The serum, when it has separated from the clot, is decanted into sterilised vessels, the usual bacteriological precautions being taken to prevent the admission of extraneous germs. To the serum thus obtained 0.2 to 0.3 per cent. carbolic acid is added. As a final precaution the serum is passed through a Berkefeld filter. It is then in a condition to be put into the small phials in which it is sent out for use. Before this is done, however, it is subjected to the following tests:—1. For sterility. It must be germ-free. 2. For absence of toxicity. 3. For antitoxic value. This much reach a required standard. The last two tests are carried out on small animals. The serum is also kept for a certain time, during which the horse remains under observation; if during this time the animal shows any signs of illness the serum is thrown away. It must be clearly understood that antitoxin is a substance which circulates in the blood of immunised animals, and which has been elaborated by the animal body. It has the property of rendering inert the particular poisons with which the animals were treated. It is a chemical substance which neutralises these poisons. Antitoxins are produced when a variety of poisons (all, however, of a complex nature) are introduced into an animal in suitable quantities. This property of stimulating the productions of antitoxins by the animal body is not confined to the poisons produced by the organisms of disease. Snake poisons, most ferments, and several vegetable poisons also give rise to the production of anti-bodies, which neutralise the particular poison injected. Antitoxins exert no influence upon the disease germs themselves, bnt only upon their products; the bacillus of diphtheria grows excellently well in diphtheria antitoxin. Unfortunately, the useful application of antitoxic serum-therapy is at present limited to a comparatively few diseases. The most important of these are diphtheria, tetanus, and epidemic dysentery. The value of serum-therapy in snake bite is limited, owing to the fact that the horse will not manufacture and store in its blood anti-venom beyond a certain amount. Once a certain concentration of antitoxin is reached the animal ceases to react in this way to further injections of venom. Moreover, a different antitoxin is required for nearly every species of snake, which imposes practical difficulties in the way of this treatment.

11711. You have prepared, I think, various tables showing the curative value of diphtheria antitoxin?—At the Medical Congress at Buda-Pesth in 1894 Roux communicated the results of the treatment with antitoxin of 300 cases of diphtheria in the Paris Hospital. The mortality of those treated with antitoxin was 24 per cent., as against 60 per cent. in the non-treated. In another hospital in Paris, where antitoxin was not yet used the mortality was 80 per cent. The experimental and clinical data, and favourable statistics brought forward by Roux, led to the trial of this method of treatment for diphtheria in all civilised countries. Diphtheria antitoxin came into use in England in 1895, and immediately a fall in the case mortality in this disease was noticed in those institutions where it was administered. I give some instances of the case mortality from diphtheria before and after the employment of the antitoxic treatment:—

PARIS HOSPITAL (HERICOURT, 1899).

Year.	Case mortality per cent.	
1893	45	
1894*	31	
1895	12	Average 14 per cent.
1896	15·2	
1897	13·2	
1898	14·5	

* Commencement of the Antitoxic treatment.

STATISTICS OF CASES OF DIPHTHERIA TREATED IN THE METROPOLITAN ASYLUMS BOARD HOSPITALS.

(Annual Reports of M.A.B. Medical Supplement.)

Year.	Cases treated by Antitoxin. Per cent. of all cases.	Mortality. Per cent. of all cases.
1888-93	—	28·5
1894	—	29·6
1895*	61·8	22·5
1896	71·3	20·8
1897	80·2	17·5
1898	81·4	15·5
1899	—	13·95
1900	—	12·01
1901	—	12·5
1902	—	11·0
1903	—	9·7
1904	—	10·1
1905	—	8·3

* Commencement of the Antitoxin period.

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CASE MORTALITY OF DIPHTHERIA FOR THE COUNTY OF LONDON. Calculated from the curves in Report of the Public Health Committee, L.C.C., 1904.

Year.	Per cent.
1892	21·6
1893	24·9
1894	23·2
1895 (Antitoxin introduced)	20·4
1896	19·9
1897	16·6
1898	15·0
1899	13·3
1900	12·1
1901	11·6
1902	10·8
1903	10·0
1904	10·0

CASE mortality from Diphtheria in 31 of the largest English towns from 1894 to 1904. (Armstrong, 33rd Annual Report of the M.O.H. Newcastle-upon-Tyne for 1905. Appendix E).

	Per cent.
1894	30·5
1895	33·1
1896	31·3
1897	25·1
1898	25·4
1899	25·2
1900	22·7
1901	22·0
1902	20·8
1903	17·7
1904	18·0

In the County of London a large proportion (65 per cent.) of cases of diphtheria are removed to the Metropolitan Fever Hospitals, where nearly every case is adequately treated with antitoxin.

In the provincial towns the proportion of antitoxin-treated cases to total cases is, I believe, considerably less.

CASE mortality in New York City from Diphtheria from 1891 to 1899. (Billings, New York Medical Journal, 1900).

Year.	Cases.	Deaths.	Mortality per cent.
1891	5,346	1,970	36·7
1892	5,184	2,196	40·6
1893	7,021	2,558	36·4
1894	9,641	2,870	29·7
1895 *	10,353	1,976	19·1
1896 †	11,399	1,763	15·4
1897	10,896	1,590	4·6
1898	7,593	923	12·2
1899	8,240	1,087	13·1

* Antitoxin introduced.
† Use of Antitoxin became general.

VIENNA.—Siegert Jahrbuch for Kinderheilkunde, 1902.

4,894 cases of diphtheria.

Year.	Mortality per cent.
1892–4	41
1895–7	20

BUDA-PESTH.—St. Ladislaus Hospital (Gertoczy, Wiener Klinische Rundschau, 1896).

Year.	Number of cases.	Mortality per cent.
1893	213	48·8
1894	227	44·9
1895*	401	19·9

* Commenced using Antitoxin.

RUSSIA.—St. Petersburgh Hospital, Rauchfuss. (XII. International Medical Congress, Moscow, 1897. Centralb. f. Bakt. Vol. 25.)

Year.	Cases.	Mortality per cent.
1870–4	304	59·5
1875–9	610	48·5
1880–4	1,425	58·1
1885–9	735	55·2
1890–4	792	55·2
1895–6*	801	22·7

* Commenced using Antitoxin.

THE results for 51 Russian Provinces for 1895 and 1896 (Rauchfuss *loc. cit.* were as follows:—

—	Number.	Mortality per cent.
Cases treated with Antitoxin	44,631	14·6
Cases not so treated	6,507	34·1

Rosenthal (*Medical Press and Circular*, 1900, *II.*, *p.* 293) collected the records of 132,548 cases of diphtheria treated with serum during the years 1894-9, and compared them with 183.256 collected from similar sources before the introduction of serum treatment. The results were as follows:—132,256 cases before serum treatment, mortality 38.4 per cent.; 132,548 cases treated with serum, mortality 14.6 per cent.

11712. Where did Roux collect those statistics?—From Europe and America. That is simply collecting together all available statistics from various countries.

11713. You have some general conclusions which you draw from those statistics?—In drawing conclusions from statistical data of this kind it is obvious that other factors may have to be examined and taken into account as well as serum or no serum. The incidence of a disease like diphtheria is not uniform. It occurs in waves. In certain years it is more prevalent than in others, and no doubt the severity of the disease also varies somewhat in different periods. Another factor which will undoubtedly have modified the case mortality of diphtheria is the adoption of a more accurate method of diagnosing the disease by bacteriological examination. This will most certainly have led to the wider recognition of the disease in mild and atypical cases, and therefore to the inclusion in the statistical data of a number of less severe cases of the disease. The influence of this factor in diminishing the case mortality cannot be directly measured. The universal decline in case mortality coincident

with the employment of a remedy based on rational lines, and capable of being experimentally demonstrated to certainly prevent infection or cure the disease in animals, is, however, highly significant. Moreover, in the early experience in Paris and in that in Russian provinces, quoted above, the mortality of treated and untreated cases at the same time and under similar conditions can be compared. The result is just the same, viz., a case mortality of less than half amongst cases treated with serum.

11714. Then you give particulars as to the mortality from diphtheria per 100,000 of the population living, in relation to the employment of antitoxin?—The employment of a remedy of any value will be a factor in diminishing the death-rate of a disease per 100,000 of the population living. The influence of this factor will be proportionate to the value of the remedy and to the extent to which it is employed. Other factors, however, which must be taken into account, are the prevalence of the disease, and any variation in the severity of its type. As I have already pointed out, the prevalence of diphtheria shows considerable variation from year to year. Groups of bad diphtheria years have occurred, followed by groups of years in which the prevalence of the disease has been less. It will be obvious that if the prevalence in proportion to the total population were doubled and the type of the disease remained the same, the mortality per 100,000 would also be doubled. Again, supposing that a remedy adequately administered halved the case mortality and other factors remained stationary, the mortality per 100,000 would only be halved on condition that the remedy were administered in sufficient amount in every case. If both factors were varying at the same time the result due to the one might be masked by the other. For this reason it is impossible to draw conclusions regarding the value of anti-toxin from general mortality figures alone. I think in previous evidence a question was asked about Hull, as regards the greatly increased mortality from diphtheria in Hull during the last few years, notwithstanding the introduction of the antitoxin. I have the figures for those Hull cases, and they show that the increased mortality is due to increased prevalence. The case mortality for the whole of the cases notified, according to the returns afforded me by the Medical Officer of Health for Hull, remains substantially the same or slightly lower during the last few years than what it was before the antitoxin period. There is, however, a substantial improvement in the mortality of cases treated in hospital where antitoxin was used to a large extent.

11715. (*Sir Mackenzie Chalmers.*) The case mortality of persons treated?—Yes.

11716. (*Dr. Wilson.*) The death-rate from the disease per 1,000 of the population has gone up?—The death-rate has gone up. But then they have had an increased number of cases. And I would like to insist that the antitoxin does not prevent the spread of the disease.

11717. It is used as a prophylactic?—It merely cures the patient.

11718. (*Chairman.*) The individual patient?—Yes.

11719. (*Dr. Wilson.*) But it does cure them all, if so many die?—It does not cure them all, but they do not all get it in sufficient amount.

11720. (*Sir Mackenzie Chalmers.*) A great many are not treated?—No, the average amount the cases must have had in Hull is an extremely small amount, and can be estimated, because they supplied antitoxin free of cost to the profession for their cases, and the amount employed was only sufficient, if equally distributed over those cases, to supply—I am speaking roughly—somewhere between 2,000 and 3,000 units per case. That is found inadequate for treating diphtheria, by the experience of the Metropolitan Asylums Board, who give 20,000 units per case.

11721. (*Dr. Wilson.*) But in early days was not even 2,000 units considered to be a large dose?—Yes. Of recent years the dose of anti-toxin which has been given has steadily risen, and with the increase in dose that has been given the case mortality has come down, as shown in all the figures everywhere.

11722. (*Colonel Lockwood.*) You say that 20,000 units are given now generally?—By the Metropolitan Asylums Board; that is their average.

11723. (*Dr. Wilson.*) For slight and severe cases?—At once, in every case directly it comes in within the first 48 hours.

11724. (*Sir Mackenzie Chalmers.*) Even if it is of several days duration?—Yes, unless it is moribund they always give anti-toxin at the Metropolitan Asylums Board hospitals.

11725. (*Chairman.*) Your original observation was tending to point out the unreliability, to a certain extent, of statistics?—Of the general mortality statistics. The case mortality statistics are perfectly reliable.

11726. (*Sir William Church.*) You did not mean the Commission to infer that they only used anti-toxin in doses of 2,000 units at Hull, but that the total supply of anti-toxin to Hull would only afford 2,000 units instead of 20,000?—Yes. Of course that is the most I can say.

11727. (*Sir John McFadyean.*) Which may mean that those who received adequate doses were only 10 per cent. of those treated at all?—Possibly. I can put those figures in evidence if you like to have them.

11728. (*Chairman.*) Which figures?—The Hull figures. It did not seem to me that Mr. Power was able to give you the answer to the question you asked him. That led me to communicate with the Medical Officer of Health for Hull.

11729. Perhaps you will furnish those figures, and add them to your evidence?—I will.

DIPHTHERIA.

City and County of Kingston-upon-Hull.

(From Returns supplied by the courtesy of Dr. *J. Wright Mason*, M.O.H., Hull).

Year.	Population.	Number of Cases Notified.	Total Deaths in City, including those in Hospital.	Number of Cases Admitted to Hospital.	Deaths in Hospital.	Case Mortality in Hospital.	Case Mortality of all Cases.
1892 -	204,750	95	10	13	1	7·7	10·5
1893 -	208,639	105	20	9	4	44·4	19·0
1894 -	212,679	86	19	10	3	30·0	22·0
1895 -	216,722	105	25	12	3	25·0	24·0
1896 -	220,844	156	39	2	1	50·0	25·0
1897 -	225,045	195	19	–	–	–	10·0
1898 -	229,887	119	10	–	–	–	9·0
1899 -	234,270	157	23	2	–	–	14·6
1900 -	238,736	99	15	9	3	33·3	15·0
1901 -	241,622	231	38	57	11	19·3	16·0
1902 -	245,448	480	85	202	27	13·3	17·7
1903 -	249,639	494	75	183	27	14·7	15·0
1904 -	253,865	526	61	164	23	14·0	11·0
1905 -	285,127	700	72	224	31	13·8	10·0
1906 -	262,426	1,253	135	336	44	13·0	10·7

	Total Cases.	Hopeful Cases.
Average Case Mortality before antitoxin used -	16·2	22·8
" after " " -	12·4	13·7

Use of Serum commenced in November, 1901.

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11730. You go on to distinguish between diphtheria and the laryngeal cases which, you say, are in the nature of diphtheria. Will you tell the Commission what is the effect of the serum treatment upon the mortality of laryngeal cases?—In a certain proportion of cases the local inflammation of diphtheria, which commonly commences in the throat or nose, extends to the larynx and trachea. This extension increases the gravity of the case, both owing to the larger surface involved, and to the frequent interference with the entrance of air through the narrow orifice in the larynx. To prevent suffocation laryngeal cases commonly require tracheotomy or intubation. The mortality of laryngeal cases has always been much higher than the mortality of those in which the local manifestation of the disease was confined to the nose or throat. In testing the effect of antitoxic treatment upon laryngeal diphtheria there is therefore no question of the inclusion of an unusual proportion of mild cases, so that the mortality of laryngeal cases before or after the introduction of the treatment may safely be compared, and any improvement in the death rate placed to the credit of the antitoxin. Statistics of cases of diphtheria requiring tracheotomy or intubation have, in recent years, been carefully kept by most hospitals. In 8,927 cases collected by Goodall ("British Medical Journal," 1899, volume i., page 197), from hospital records in different countries before 1894, the mortality was 71.6 per cent. With the introduction of antitoxin there was a marked fall in the mortality of cases of diphtheria requiring tracheotomy. In a series of 2,374 cases collected by the same author from similar sources, but in which antitoxin was employed, the mortality was 36.6, or approximately one-half. The same order of improvement occurred in all the principal hospitals of Europe and America in which antitoxin was administered. With the object of coming to a conclusion regarding the curative value of diphtheria antitoxin, the German Health Office (Arbeiten aus dem Kaiserlichen Gesundheitsamte, 1897, 254) obtained returns of diphtheria cases from all the hospitals in the Empire from April, 1895, to March, 1898. Amongst these were 2,041 cases in children up to five years of age, in which tracheotomy had to be resorted to. Of these 778 died; a mortality of 33.6 per cent. The German health authorities compare this mortality with that of tracheotomy cases in children's hospitals in Germany, previous to the introduction of antitoxin, which, according to the statistics of Baginsky, Hirsch, Herz, Gläser, and Feer, varied from 58.4 to 86 per cent., depending upon the age of the children.

11731. Are these cases in the report of the German Health Office cases in which antitoxin had been used?—In the first series of cases antitoxin was used with a mortality of 33 per cent.

11732. When you say returns of diphtheria cases from all the hospitals, does that imply that they were diphtheria cases which had been treated in all cases with antitoxin?—Yes, the treatment was general in hospitals in Germany within those years.

11733. Then you give some results obtained by the antitoxin treatment in tables, showing the influence of early or late administration of antitoxin upon the case mortality from diphtheria?—Data regarding this question have been collected by numerous authorities and the total figures amount to upwards of 20,000 cases.

The Results obtained by Antitoxin Treatment, according to the period after the onset of the disease at which the injection is given (*Dieudonné Schutzimpfung u. Serumtherapie*, 1900.)

Author.	Total of Cases.	Percentage of Total Mortality.	First Day.	Second Day.	Third Day.	Fourth Day.	Fifth Day.	Sixth Day.	After Sixth Day.
Welch	1,189	14·2	2·3	8·	13·5	19·0	29·3	34·1	33·7
Hilbert	2,428	18·3	2·2	7·6	17·1	23·8	33·9	34·1	38·2
Collective Investigation of the American Paediatric Society	5,794	12·3	4·9	7·4	8·8	20·7	35·3	—	—
Collective Investigation in Austrian Sanitary Department	1,103	12·6	8·0	6·6	9·8	23·5	28·8	30·7	21·0
Collective Investigation of the Imperial German Health Office	9,581	15·5	6·6	8·3	12·9	17·0	23·2	—	26·9

Mortality at the Brook Hospital Metropolitan Asylums Board) 1897 to 1902, according to the day on which Antitoxin Treatment began. (*Report Metropolitan Asylums Board for 1902. Lancet, 1903.*)

Day of the Disease on which Treatment Commenced.	Mortality per cent					
	1897.	1898.	1899.	1900.	1901.	1902.
First	·0	·0	·0	·0	·0	·0
Second	5·4	5·0	3·8	3·6	4·1	4·6
Third	11·5	14·3	12·2	6·7	11·9	10·5
Fourth	19·0	18·1	20·0	14·9	12·4	19·8
Fifth	21·0	22·5	20·4	21·2	16·6	19·4

Eight thousand and three cases of bacterially-verified Diphtheria treated with Antitoxin by the Chicago Health Department between 1895 and 1905. (Biennial Report of Department of Health, Chicago, 1904-5, p. 137.)

Day of Disease	1st.	2nd.	3rd.	4th.	Later.
Number of Cases	608	2,063	2,802	1,496	1,034
Cases of Mortality per cent.	·32	1·66	3·64	11·03	21·08

From the above records it is clear that those cases in which antitoxin was administered on the first day have the lowest mortality, and that the death rate rapidly rises as the period between the onset of the disease and administration of the serum increases. This is exactly what happens in animal experiments. If antitoxin be administered to an animal on the same day as it is injected with a fatal dose of diphtheria bacilli, 100 per cent. can be saved. If on the day after a large percentage of treated animals recover. If on the third day only a small percentage; and after the third day antitoxin is unable to prevent death—that is in animals. In other words, antitoxin is a remedy definitely known to be proportionately more effective the earlier it is administered, and the experience quoted above, derived from a wide source, shows that this is the case when the serum is administered to children, just as was to be expected from animal experiments If antitoxin were a remedy of no value, whether it were administered on the first or fifth day of the disease would be immaterial.

11734. What is the dangerous date in diphtheria? At what days would you expect in a bad case, in an average case, say, a crisis?—On which day would death take place, do you mean?

11735. Yes?—They may die any time after the second or third day, in very bad cases.

11736. What is the effect of antitoxin upon the incidence of diphtheria paralysis?—The incidence of paralysis in cases of diphtheria treated in the hospitals of the Metropolitan Asylums Board has risen about 6 per cent. since antitoxin was introduced. This was to be expected, from experimental knowledge of the action of diphtheria poison and of the influence of antitoxin upon an animal which had been either injected with this toxin or inoculated with the bacilli. If antitoxin be given to an animal in sufficient quantity and sufficiently early in the disease, both the acute symptoms and the paralysis, which may occur about three weeks later, are prevented. On the other hand, when the dose of antitoxin is not quite adequate the acute symptoms may be prevented, but the late paralysis may still supervene. Reasoning from analogy, it would seem probable that the increased percentage of paralysis is due to a considerable number of cases which would otherwise have died of acute poisoning, being saved by late or insufficient administration of antitoxin. This supposition is supported by the facts adduced in the following table, indicating the proportion of paralysis which occurred amongst patients treated with antitoxin upon the first, second, third, fourth, and fifth days of the disease respectively.

Day of Disease upon which Antitoxin was first injected.	Total number of Cases.	Total number of cases of Paralysis.	Percentage incidence of Paralysis.	Number of severe cases of Paralysis not fatal.	Number of fatal cases of Paralysis.
First	69	4	5·7	0	0
Second	277	28	10·1	1	0
Third	340	53	15·5	5	3
Fourth	323	61	18·8	8	7
Fifth, and later	571	147	25·7	28	8
Total	1,580	293	18·5	42	18

11737. Then you give a summary of the facts?—The foregoing facts relating to the influence of antitoxin in the treatment of diphtheria may be briefly summarised as follows:—(1) Diphtheria can be cured with certainty in animals by treatment with antitoxin. (2) The earlier the treatment is applied to an animal the less the quantity required and more certain the cure, and if the administration of antitoxin is delayed beyond a certain time the treatment is ineffectual. Those are the facts as regards animals. (3) A reduction of case mortality of diphtheria to one-third or one-half occurred universally coincidently with the introduction of the antitoxin treatment. (4) In many places a corresponding diminution in mortality in proportion to population occurred; but in some countries this has been masked by a simultaneous increase in the prevalence of the disease. (5) A similar fall in case mortality amongst the severest forms of diphtheria, viz., those necessitating tracheotomy, has been universally experienced, so that the apparent beneficial results of serum treatment cannot be explained by the supposition that after 1895 a milder form of the disease was suddenly and universally encountered. The beneficial results of serum treatment are greater, the earlier in the course of the disease the treatment is applied, and after the fourth or fifth day the administration of anti-toxin produces little or no influence upon the case mortality. This is absolutely in accord with what was known of antitoxin from the results of experiments upon animals.

11738. Do you attribute the discovery of the serum and the efficiency of it and the way to use it, to experiments on animals?—Entirely.

11739. Then you have something to say as regards the prophylactic action of diphtheria antitoxin?—The use of antitoxin as a remedial agent does not materially diminish the spread of the disease, for an individual remains infective for weeks, or sometimes months after convalescence. With the object of stamping out epidemics of diphtheria, it can, however, be employed as a prophylactic measure with great advantage. As previously mentioned the shorter the interval between the inoculation of an animal with diphtheria bacilli or diphtheria toxin and the treatment with anti-toxin the less is required to prevent a fatal issue. When the antitoxin is administered before the bacilli or toxin the amount of the former required is still further diminished, and a complete protection can be obtained. This prophylactic use of serum can be advantageously employed to prevent the spread of the disease in schools and institutions, and so readily suppress outbreaks of the disease. Hitherto, in England, diphtheria antitoxin has not been much used as a prophylactic, though in certain outbreaks, such as those at Cambridge and Chesterton, it was employed with considerable success (*Cobbett, J. of Hygiene, Vol. I.*). In France, Germany, and America, however, it is commonly employed with this object, and very valuable results have been obtained in this direction. The following experience occurred in Heubner's clinic (*Lohr. Jahrbuch f. Kinderheil kunde Bd.* 43, *Ht* 1, *and Slawyk, Die Therapie der Gegenwart*, 1899). From 1895 onwards prophylactic injections were given every three weeks to all the patients, and the outbreaks of diphtheria which had formerly regularly occurred in the wards, came to an end. In October, 1897, this precautionary measure was stopped in a wing not used for infectious cases, and in November cases of house infection broke out in this department. A general prophylactic inoculation was again employed, and the occurrence of cases completely ceased. Netter (Bull. de l'acad de med. Paris, March, 1902), principal health officer for Paris, reports the following instance:—In the presence of an epidemic of diphtheria in Paris among 491 children exposed to infection who did not receive protective injections, 88 contracted the disease; while

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of 502 children, who had been similarly exposed and were given protective injections, only 13 became infected. Of these 7 developed the disease within 24 hours, and 6 more than a month after the injections. Since the immunity does not begin for 24 hours and passes off in approximately three weeks, these figures strongly support the prophylactic value of the remedy. The preventative action of antitoxin is extensively taken advantage of in America. Biggs (New York Med. Journ., 1900, lxxi., p. 234) states that out of 3,109 children to whom antitoxin was administered only 9 acquired diphtheria; and Park (Journ. Amer. Med. Assoc., 1900, i., p. 902) records 6,506 cases of immunisation, among which 28 developed the disease within 24 hours, that is before the protection was effective, while only 274 were attacked after this limit of time.

11740. You now come to tetanus antitoxin as a curative agent?—It is possible to obtain tetanus antitoxin of extraordinary preventive power, as tested experimentally by injecting the serum previously to the toxin. The experimental results are satisfactory when the two are mixed together at the time of injection; but as a curative agent, in practice its value is far less than that of the corresponding antitoxin for diphtheria, because the disease is usually only discovered after serious lesions of the nervous system have already been produced, upon which, unfortunately, the antitoxin can exert little influence. The injection of antitoxin can neutralise toxin circulating in the blood, and combine with any toxin which may be formed after its injection, but it cannot restore damage done to the nervous elements, although it can prevent nervous elements from being attacked. Koehler (*Münchner Med* 12. *Wochschr.*, 1893), dealing with the statistics of ninety-six cases treated by antitoxin, found the death-rate to be 30.4 per cent., which is certainly much below the normal death-rate, viz., 86 per cent. On the other hand, Steuer (*Centralbl. f. d. Grenzgebiete d. Chir. u. Med.*, 1900), from his statistics, could demonstrate no improvement over cases treated without antitoxin.

11741. (*Sir William Collins.*) One observer showed a reduction of 50 per cent., and the other showed none?—That is so.

11742. (*Chairman.*) Have you any suggestion as to how that could arise?—Different dosage possibly. Unfortunately the damage is done before you know that the person has got tetanus.

11743. You mean it has got too late?—Yes.

11744. Then it may be from that or it may be from the proper dose not being administered?—Yes; it is not very hopeful under any circumstances.

11745. What is not very hopeful?—The antitoxin treatment of a case of tetanus which has developed.

11746. What time after the injury would you say was the time you expect it to develop?—It may be anything from a few days to months; it is not of immediate onset. The most acute cases develop most rapidly.

11747. Do you mean that if a person has a bad fall and scrapes all the skin off his hand in a dirty place it would take weeks to develop?—Yes. I am not sure of the shortest time that has ever been recorded, but certainly it is unusual for the symptoms to occur in less than a week.

11748. (*Sir John McFadyean.*) The usual period after inoculation is about a week, is it not, in animals?—Yes.

11749. (*Chairman.*) Then what have you to tell us with regard to the prophylactic action of the tetanus antitoxin?—Whatever doubts may be entertained of the value of this antitoxin as a curative agent—as a prophylactic its power is fully established. In veterinary practice, where it has been used under test conditions, it has proved infallible. The bacillus of tetanus is common in dung, and hence on the skin of horses. A considerable mortality from tetanus occurs in veterinary surgery, owing to the entrance of the bacillus into wounds, especially in castrations, which from the situation of the wound are particularly liable to be contaminated. Nocard has collected the results of the experience of prophylactic injections gained by 63 veterinary surgeons during a period of 22 months.

11750. Does that mean that the tetanus bacillus may be swallowed with impunity? You say it is common in dung. I suppose it has been swallowed by the horse?—Yes, swallowed in the hay and food, and it usually lives quite comfortably in its intestine.

11751. And it is not injurious in passing through the human body?—No, not unless it gets in through any abrasion. Certain cases apparently arise in that way from abrasions in the alimentary canal of horses. 2,700 cases, both operative and traumatic, were selected for preventive inoculation from those in which by experience they knew that tetanus was most likely to occur. Not one of these 2,700 animals contracted tetanus; whereas, in the practice of the same veterinary surgeons, during the same period, but not so treated, 259 cases of tetanus occurred (*Nocard, Comptes rendus du Congrès de Moscou*, 1897, *t. vi.*, *p.* 244).

11752. (*Sir William Collins.*) Perhaps you will add the reference in cases where you do not give the authority. In some cases you do, and in some you do not?—I will, where I can do so. I can nearly always give the reference to the original, but sometimes I have to depend upon the collected statistics. The originals would be very troublesome.

11753. Then in that case perhaps you will give the reference to the collected?—Yes.

11754. (*Sir Mackenzie Chalmers.*) And the collectors?—Yes. In case of severe wounds in man, such as compound fractures, where there has been much soiling of the wound, and especially if earth has found its way into the depths of the wound, a prophylactic injection of tetanus antitoxin is advantageously employed. Herhold ("Deutsch Med. Woch," 1901, page 479), who was surgeon to the German contingent on the expedition to Pekin, at first lost several patients by tetanus. Afterwards he used prophylactic injections in all cases of crushed wounds, contaminated with dirt, and no more cases of the disease occurred.

11755. (*Chairman.*) Then you mention dysentery antitoxin. I do not think we have heard of dysentery antitoxin before?—Epidemic dysentery is an infectious disease, which is more particularly liable to assume serious proportions when individuals are crowded together in gaols, asylums, or camps. It is in war especially that dysentery becomes a formidable disease, both on account of the high death rate and of the number of men thrown out of action, often for prolonged periods. Knaak (Die Krankheiten, in Kriege, Leipzig, 1900) states that in the American War of Secession, 1861-1864, there occurred 259,071 cases of acute dysentery, with 5,576 deaths; and of chronic dysentery 28,451 cases, with 3,855 deaths. In the Franco-Prussian War of 1870-1871 there were 38,652 cases (49 per thousand) and of these 2,380 died—62 per cent. of those affected. In addition there were 223 cases among officers, etc., with 25 deaths. The bacillus of bacterial dysentery was first isolated by Shiga, in Japan (centralb f. Bakt, 1898). Kruse found the same bacillus in an outbreak of dysentery in South Germany (Deutsch Med. Woch, 1900-1901), and Flexner in Manilla (Centralb f. Bakt, 1901). The bacillus is pathogenic for the guinea-pig and rabbit. In these animals the bacillus itself, or the poison obtained by growing it in broth, produces dysenteric diarrhœa, severe nervous symptoms, paralysis, and death within a few hours or days, according to dose. In the intestines of these animals morbid changes, similar to those occurring in man, have been observed.

11756. Then as to serum therapy in this connection?—Shiga (*Deutsch Med. Wochschr.*, 1901) first immunised horses against the dysentery bacillus, and employed the serum as a therapeutic agent in man. Kruse (*Deutsch Med. Wochschr.*, 1903, *p.* 49) obtained a similar serum by using the bacillus isolated by him in Germany, and Rosenthal (*Centralbl. f. Bakt*, 1904, *p.* 463), Todd (*Brit. Med. Journ.*, 1903, *J. of Hygiene*, 1904, *p.* 480), and others have added to the knowledge of the subject.

11757. What are the properties of the serum?—Anti-dysentery serum possesses preventive and curative properties when employed on animals, inoculated either with the living bacillus or its toxins. All the pathogenic effects referred to as occurring in experimental animals can be prevented or cured.

11758. Have any results been obtained in the case of man?—No extensive data as to the value of anti-dysentery serum as a therapeutic agent in man are as yet available, but so far as it has been used the results recorded have been of a very promising character. Shiga (*loc. cit. supra*) used his serum on 298

cases, and obtained a reduction in the death rate to one-third of the rate in cases not treated by serum, the death rate in cases receiving only serum treatment was 10.8 per cent., and in 2,599 cases, treated by other means, 35.4 per cent. Kruse treated 100 cases with good results. Of 19 patients under 10 years one died, which is equal to a death rate of 5 per cent., the usual mortality in cases of this age being from 13 per cent. to 20 per cent. The injection of the serum was followed by a rapid reduction in the number of stools, and by the improvement in the general condition. Similar results were obtained at Moscow by Rosenthal (Centralb f. Bakt, 1904), in 157 cases treated with serum. The death rate was 4.5 per cent., compared with 10.11 per cent. in other hospitals during the same epidemic. When the serum was used in the first three days recovery took place in one or two days. Rosenthal's serum was employed in the Russo-Japanese War. Koreutchewsky (Bull. l' Inst. Pasteur, 1905) treated 70 cases, and found the treatment very efficacious. The painful symptoms were controlled as by a narcotic. Barikan (quoted by Vaillard and Dopter) treated 59 cases in Manchuria, with one death. Vaillard and Dopler (*Ann. l'Inst. Pasteur*, 1906) treated 96 cases, all of a severe type, with one death.

Cases of moderate severity, 50; deaths, 0.
Severe cases, 18; deaths 0.
Grave cases, 24; deaths, 0.
Extremely grave, 4; deaths, 1.

These authors found, like other observers, that after the use of the serum there occurred a diminution in the frequency of the stools. The painful symptoms, such as spasms and colic, were ameliorated.

11759. I gather that this antitoxin has not been tested nearly as much as the previous ones for diphtheria and tetanus?—That is so.

11760. I understand that you consider the antitoxin tetanus is a perfect remedy if applied in the right dose at the right time?—Perhaps I might explain a little more fully. If you injected a person with tetanus, it is my belief that in man, as is the case in animals, if you gave him a dose of tetanus serum the next day he would certainly not get tetanus.

11761. That is what I meant. You look upon it as a certain remedy if applied very promptly?—Yes, if it is applied as a precaution. You cannot tell that a person is going to have tetanus every time he gets a wound and soils his fingers. In most cases he will not.

11762. I quite understand the difficulty. It may not be a case of negligence in not giving it to him. But at the same time, if it were given him you say it would save him?—Certainly, I think so.

11763. With regard to diphtheria, do you say the same thing?—My own belief is that if a child could be treated with the diphtheria antitoxin in an adequate amount and sufficiently early in the disease the mortality would be nil. The moment you knew that the child had diphtheria, if you could treat him with antitoxin the mortality would be nil. That opinion has also been stated by the Medical Officer of Health of Chicago from his experience of the disease in that city, where they distribute diphtheria antitoxin free to every doctor, provided that he afterwards fills up a form to say that his patient cannot afford to purchase it.

11764. As to dysentery, you cannot speak with the same confidence, I understand—No, but from the experiments on animals I regard the dysentery antitoxin as for practical purposes coming after the diphtheria antitoxin. In practical value it will, I think, make a good second to the diphtheria antitoxin.

11765. And you have warning that there is dysentery much more promptly than you have in the seeds of tetanus?—Yes, before you know that a person has got tetanus the damage is unfortunately done.

11766. (*Sir William Collins.*) Did you say that the mortality in Chicago had been reduced to nil?—No, I said that the opinion which I have just expressed that if you could take a child with diphtheria sufficiently early and adequately treat it with antitoxin, the mortality would be nil, coincides with that of the Medical Officer of Health for Chicago from his experience in that city.

11767. Can you tell us how far that result had been achieved in Chicago?—I can give you the statistics of the general mortality in Chicago. Unfortunately, I have not available the case mortality statistics in Chicago.*

11768. (*Sir Mackenzie Chalmers.*) May I put it in this way to see if I understand it: According to your belief, if every contact, if every person who comes in contact, and is therefore liable to infection, received a proper dose, you would have no development of diphtheria in contacts?—That is also my belief, but that it not what I was expressing before.

11769. It comes to the same thing, does it not?—Quite so.

11770. (*Sir John McFadyean.*) What you mean really is, that if, at the moment the first lesions are visible in the throat, the child got 20,000 units, there would be no mortality from diphtheria?—That is so.

11771. (*Dr. Wilson.*) In spite of the severity of the type?—Yes.

11772. (*Sir Mackenzie Chalmers.*) What is the incubation period of diphtheria?—About ten days.

11773. (*Sir John McFadyean.*) I suppose that has been experimentally proved to be not true for the tetanus antitoxin?—Yes.

11774. You quote Nocard's observations here that he collected, but I do not know whether you are acquainted with another set of experiments in which he infected animals deliberately, and then gave them anti-tetanus serum at intervals afterwards, and found practically that no recoveries took place when the first dose of the serum was given on the onset of symptoms?—Yes, I am familiar with those observations.

11775. Is it the fact that all those who were treated before the onset of the symptoms recovered?—I cannot say the whole of them. So far as regards some of those that were treated just before the onset of symptoms, there was I believe a small mortality amongst them.

11776. (*Dr. Gaskell.*) What do you mean by being treated on the first day. What is the first day of diphtheria?—The first day is the first day that the disease is recognised.

11777. (*Sir Mackenzie Chalmers.*) By the appearance of the false membrane?—By a combination. I imagine that in most of these cases the first day in the Metropolitan Asylums Board Reports refers to the first day that the disease is recognised by the presence of the bacilli in the throat and by some symptoms of illness, such as sore throat or slight malaise.

11778. (*Sir William Collins.*) Does not the statement which you have just made to the Commission amount to this: That in your opinion every death from diphtheria is preventible?—Only under ideal circumstances.

11779. (*Dr. Wilson.*) And by this serum?—By the serum.

11780. (*Sir Mackenzie Chalmers.*) If every contact was treated you would have no development of the disease in any one?—I believe not; not if it was adequately treated.

11781. (*Sir John McFadyean.*) I suppose part of the ideal circumstances would be at least a daily inspection of the throat of every young child. I mean in the family?—Yes.

11782. (*Dr. Wilson.*) But you admit that in a great many healthy persons you find the bacillus?—Yes, occasionally.

11783. How about those?—I do not quite understand your question.

11784. You have admitted that in the throats of apparently healthy persons (they may be in a diphtheria house or not) you do find the bacillus?—Yes.

11785. Would you inject these?—If they are not ill?

11786. If they are not ill?—Certainly—if you want to stop the outbreak.

11787. What about the patients who have had the disease in whose throats the bacillus may be detected

* *The data asked for were already given in answer to Q. 11733 in the table showing the mortality from diphtheria in 8,000 cases treated in Chicago on different dates of the disease, where it may be seen that of 608 cases treated on the first day of the disease the mortality was ·32 per cent.—C. J. M.*

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months afterwards as you say?—It is no good injecting them.

11788. But you hold that they are in an infectious state?—They are in an infective state; they can infect other people.

11789. And can be kept away from work?—Unfortunately they have to be.

11790. No, I think you would put a stop to the employment of a great many men if that could be done. I think you must go by the physical symptoms and not the bacteriological. If you go in for the bacteriological test out and out you would have to confuse a great many people?—That is so. If you test convalescents and contacts bacteriologically you will find a great many people in apparent health who are carrying about the diphtheria bacilli. That is how the disease is spread.

11791. That is an assumption?—It is my opinion, of course.

11792. (*Chairman.*) We will go on to the next question. You were going on to speak about vaccines against plague and typhoid fever. What have you to say with regard to them?—The discovery by Pasteur, that animals could be protected against chicken cholera and splenic fever by inoculation with attenuated cultures of the organisms which cause the diseases, and the practical value of this process in counteracting the ravages of these diseases has already been mentioned. The inoculation of mankind with living attenuated cultures, according to the Pasteurian method has been made use of in the case of cholera, but the apprehension that at any moment the virulence of attenuated organisms might possibly become enhanced has prevented the general adoption of this method of protective inoculation in the case of human beings. In 1888 Gamaleia (*Ann. l'Inst. Pasteur*, 1888, 1889) found that guinea pigs and pigeons could be protected against infection with an organism (vibrio metschnikovi) by the previous inoculation of killed cultures of this vibrio. Gamaleia had found this microbe to be responsible for a disease of poultry in Odessa. It also kills guinea pigs and pigeons. This was an important discovery, as it showed that to produce protection it was not essential to infect with the living microbe, but that some degree of immunity could be induced by the inoculation of the chemical products contained in the substance of the dead organisms. Subsequently the principle discovered by Gamaleia was by experiments on animals, extended to other diseases, *e.g.*, cholera, typhoid (Brieger and Wasserman, *Zeitschr. f. Hyg. Bd.* 12, *and Deutche Med. Wch.*, 1892; Pfeiffer, *Zeitschr. f. Hygiene, Bd.* 21, 1896) and plague (Yersin). In 1895 plague broke out in Bombay and spread throughout the city, and extended to neighbouring places. The general sanitary measures employed proved powerless to prevent the spread of the disease. Haffkine therefore turned his attention to the problem of preventive inoculation against plague in the hope of arriving at a useful method which could be applied upon a large scale to man. After numerous experiments with cultures of the plague bacillus which had been killed by a variety of means, Haffkine decided upon a broth culture of the organism sterilised by heating to 65 deg. C. Having tested the vaccine by innoculating first himself and then the personnel of his laboratory, the first extensive trial of the efficacy of this vaccine was made in January, 1897, upon the inmates of the Byculla Jail, where plague had broken out (*Statistics of inoculations with Haffkine's antiplague vaccine*, 1897-1900. *Bannerman—Gov. Central Press Bombay*, 1900). The submission to inoculation was voluntary. The jail contained 337 adults of both sexes, who were not obviously infected with plague. 154 offered themselves for inoculation. Three of these were, however, in the incubation stage of plague, for they developed plague the same evening. For the purpose of arrivng at an estimate of the value of the preventive inoculation these persons in the incubating stage of the disease must be eliminated from the calculation, as also many cases amongst the uninoculated, who also developed plague upon the day the injections were made, leaving 177 uninoculated and 151 inoculated. The result of this experiment was as follows:—

	Number of Persons.	Cases of Plague.	Deaths.
Uninoculated -	177	12	6
Inoculated - -	151	2	0

In December, 1897, an outbreak of plague occurred in another jail in Bombay (Umkardi) thus affording a further opportunity of testing the efficacy of Haffkine's vaccine, under conditions as favourable for the formation of a judgment as it is possible to obtain in connection with human diseases.

	Number of Persons.	Cases of Plague.	Deaths.
Uninoculated -	127	10	6
Inoculated - -	147	3	0

Another opportunity for testing the efficiency of Haffkine's vaccine occurred in the village of Undhera. The whole population of this village was assembled in family groups, and the intention was to inoculate every alternate person. The total number of inoculated persons was 513, and uninoculated 437. The larger number of inoculated was owing to the insistence of some individuals to participate in the possible advantage of inoculation. The epidemic lasted 42 days, and the result was as follows:—

	Number of Persons.	Deaths.
Uninoculated - - - - -	437	26
Inoculated - - - - -	513	3

On the strength of the favourable results of the above experiments Haffkine's method of inoculation was officially adopted by the Government of India, and the arrangements made for the manufacture of vaccine on a large scale. During the plague season 1902-3, 505,398 persons were inoculated in the Punjab alone. (Report on plague and inoculation in the Punjab from October 1st, 1902, to September 30th, 1903.) The official report gives details concerning the villages of 12 districts in which the proportion of inoculated persons was greatest. The number of inoculated in these 12 districts was 224,428, and the uninoculated, as estimated from the total population, 639,600. The number of cases and deaths from plague were as follows:—

	Number of Persons.	Cases.	Deaths.	Cases per cent.	Case Mortality per cent.
Inoculated - - - - - -	224,428	3,393	814	1·5	23·9
Uninoculated - - - - -	639,600	49,430	29,733	7·7	60·1

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In the case of the district of Amritsar (Browning-Smith. Report on plague and inoculations in the Amritsar district, 1902-3, "Indian Medical Gazette," 1904) the inoculations and subsequent observations were all made by one individul, and as these show evidence of having been conducted with particular care, they are worth quoting.

	Number of Persons.	Cases.	Deaths.	Cases per cent.	Case Mortality per cent.
Inoculated	30,839	572	150	1·21	26·2
Uninoculated	130,927	11,233	7,883	8·58	70·2

The value of inoculation against plague is undoubted. According to the less favourable of the above figures the chances of a subsequent infection are reduced to one-fifth by this means, and the chances of recovery are two and a half times as great as in the case of the uninoculated. In other words, the mortality has been reduced to one-twelfth by inoculation.

11793. You now come to anti-typhoid vaccination?—Typhoid fever is a disease to which our soldiers are particularly liable, especially those stationed in warm climates. In India the loss to the British Army from deaths alone amounts to half a battalion a year, apart from the pecuniary loss to the State through sickness and invaliding. In war the loss is much more serious; more men being often placed *hors de combat* through this cause than by the bullets of the enemy. Consequently any practicable means whereby the incidence of typhoid fever amongst troops can be reduced is of the utmost importance to the British Army. The subcutaneous injection of minute quantities of killed typhoid bacilli in animals—guinea-pigs or rabbits—protects them against a subsequent fatal dose of the living bacilli. The immunity is greater when three or four such injections, separated by an interval of 10 to 14 days, are made, than with a single injection. A method of preparing a vaccine against typhoid fever was devised by Wright and Semple in 1896. They satisfied themselves of the innocuousness of their vaccine by experiments—first upon themselves, and subsequently upon their friends. The first trial of protective inoculation against typhoid fever was made in October, 1897, at the Kent County Asylum, where there was an outbreak of the disease. (The figures quoted in par. 11793 to 11799 are taken from "The Practitioner," March, 1904.) The treatment was offered to the nurses and attendants of the Asylum, and 84 out of 200 accepted the offer. Four cases of typhoid occurred in the uninoculated, but none among the inoculated. The figures were inadequate to enable one to form any opinion regarding the value of inoculation, but were sufficient to show the innocence of the method, and to justify more extended trials. The first anti-typhoid inoculations on a large scale were made amongst British troops in India (Bangalore, Rawul Pindi, Lucknow), when the Plague Commission, of which Professor Wright was a member, was in India, November, 1898, to March, 1899. These inoculations were voluntary, at private cost, and without official sanction; though the original proposal for them, in 1897, had come from the Indian Government. The results of these early Indian inoculations were as follows:—

Number under Observation.	Cases.	Deaths.	Percentage of Cases.	Percentage of Deaths.
Inoculated 2,835	27	5	0·95	0·2
Uninoculated 8,460	213	23	2·5	0·34

On 25th May, 1899, the Indian Government made application to the Secretary of State for India that the inoculations should be sanctioned, and should be made at the public cost. The application contained the following:—"The anti-typhoid inoculations have been, we believe, on a sufficiently large scale to show the actual value of the treatment, while the results appear to afford satisfactory proof that the inoculations, when properly carried out, afford an immunity equal to or greater than that obtained by a person who has undergone an attack of the disease; further, the operation is one which does not cause any risk to health."

11794. (*Sir William Collins.*) You do not give the case mortality in those Indian cases. I see in the inoculated there were 27 cases with 5 deaths, that is about 1 in 5. Then in the uninoculated 213 cases and 23 deaths; that is about 1 in 9, is it not?—Yes; I have not put the case mortality down.

11795. You have given it in so many figures, that I thought you might have added it there?—I have not done the arithmetic, you mean. It shall be done. Sanction for anti-typhoid vaccination having been given, a number of men from several regiments were inoculated. The regiment in which the largest number of men were inoculated was the 15th Hussars, which remained in India. The incidence of typhoid in this regiment from the 22nd October, 1899, to 22nd October, 1900, is given below:—

	Strength.	Inoculated.	Cases.	Deaths.	Not Inoculated.	Cases.	Deaths.
Officers	22	19	0	0	3	0	0
N.C.O. and men	481	317	2	1	164	11	6
Women	36	24	0	0	12	0	0

From which it is seen that the incidence of typhoid amongst the uninoculated was 5.8 per cent., and amongst the inoculated 0.55 per cent., or less than one-tenth.

11796. Where is the reference to that published? Is it in "The Health of the Army" Annual Reports?—Those statistics were published in the medical papers, "Lancet" or "British Medical Journal," and were collected together in "The Practitioner," March, 1904.

11797. Would they be in some official publications?—I do not know whether they were in an official publication. The Army Medical Department supplied Wright, while typhoid inoculation was going on with the figures, which he published from time to time in the "Lancet," I think.

11798. I do not think they appear in the annual Blue Book, entitled "The Health of the Army," do they?—I do not think so.

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11799. (*Chairman.*) You proceed to give the results of anti-typhoid inoculation amongst British troops during the South African War?—The statistics concerning the campaign in South Africa, which have been collected together by the Army Medical Department, indicate a substantial diminution both of the incidence and deaths from typhoid fever amongst the inoculated. The following instances are taken from these reports:—

GARRISON OF LADYSMITH.

——	Number.	Cases.	Deaths.	Per cent. Incidence.	Case Mortality per cent.
Uninoculated - - - - -	10,529	1,489	329	14·14	22·1
Inoculated - - - - -	1,705	35	8	2·05	20·3

MANCHESTER REGIMENT, WINBURG.

——	Number.	Cases.	Deaths.	Per cent. Incidence.	Case Mortality per cent.
Uninoculated - - - - -	547	23	7	4·2	30·4
Inoculated - - - - -	200	3	0	1·5	0·0

7TH HUSSARS, SOUTH AFRICA.

——	Number.	Cases.	Deaths.	Per cent. Incidence.	Case Mortality per cent.
Uninoculated - - - -	244	20	3	8·2	15·0
Inoculated - - - - -	307	7	0	2·9	0·0

Those are the extracted ones. There are a number of other figures collected. I chose those which seemed to give the clearest idea and were best capable of comparison between inoculated and uninoculated.

11800. (*Sir William Collins.*) Where are they published?—They are all published in "The Lancet."

11801. Is there no official publication of them?—I think not at present.

11802. (*Chairman.*) Is not the Army Medical Department an official body; do they not officially publish it?—I am sure that these figures have not been officially published; because when that Committee of Inquiry into Anti-Typhoid Inoculation was called together, we were supplied with a copy of "The Practitioner," March 1904, in which Wright had collected all these cases, and which were certified to by the statistical officer to the Army Medical Department as being correct. If they had had an official publication containing these figures they would obviously have placed it before us.

11803. At any rate, you have described what the source of them is?—Yes. More recently a carefully conducted test has been carried out under the auspices of the Anti-Typhoid Inoculation Committee, appointed by the Secretary of State for War. (An account of this outbreak was published in the Journal of the R.A.M.C., May, 1907.) In August and September, 1905, a proportion of the 17th Lancers, whose total strength was 490 men and 24 officers and wives, were inoculated with anti-typhoid vaccine. This regiment embarked for India on September 6th, and proceeded to Meerut, where they went into the camp. The number of inoculated was 150, 127 of whom were inoculated on two occasions; 14 days elapsed between the two inoculations. In October, whilst the regiment was still in camp, enteric fever broke out, and 46 cases occurred. Of these 45 were amongst the uninoculated and 1 amongst the inoculated. The men, both inoculated and uninoculated, were all mixed up together in the camp and exposed to the same chances of infection. The value of anti-typhoid inoculation can no longer be regarded as doubtful, and considerable effort is being made by the Commander-in-Chief in India to secure the inoculation of as large a proportion as possible of the British Army in India.

11804. (*Dr. Wilson.*) That is not Wright's antitoxin?—It is essentially the same. The dose is different; it is much smaller. On experience it was found advisable to diminish the dose; it was as effective, and gave rise to less symptoms of soreness.

11805. (*Sir William Collins.*) Has that Committee reported?—No, it has made an interim report only.

11806. (*Chairman.*) Are these statistics taken from the interim report?—These figures of the 17th Lancers have been published in the Royal Army Medical Corps Journal.

11807. (*Sir William Collins.*) Have they been published in any Parliamentary Paper?—I do not think so.

11808. Not the interim report?—What became of the interim report I do not know. It went to the Army Council. Whether they presented it as a Parliamentary Paper, I do not know.

11809. Is the final report ready?—No; the investigation is still going on. This was an attempt to get an experiment that should be carefully controlled; about half the persons were inoculated, and about half uninoculated, and a special medical officer was sent out to watch them and to diagnose every case, so as to make sure that it was really typhoid. We had done that in the previous years, but we cannot ensure that the draft will go to a neighbourhood where there is any typhoid or any opportunities of contracting typhoid; so that, having made preparation for a test of this kind, one is never sure whether there will be any opportunity for its being carried out. In the case of the 17th Lancers they went straight away to Meerut, and encountered an epidemic of typhoid.

11810. (*Sir John McFadyean.*) I should like to ask whether, in view of the clearness of that result, you think it will be a moral proceeding to repeat that—that is to say, for the whole of the men to be sent out to have the advantage to be derived from this vaccination?—The morality question rests with the War Office. Our reference is to obtain information.

11811. (*Dr. Wilson.*) Vaccination is voluntary, of course?—Yes.

11812. Is there any inducement given to the men?—No.

11813. (*Chairman.*) You said as regards this anti-typhoid inoculation that, so far as the experiments go, you think they show very valuable results?—Yes, I think so.

11814. (*Sir William Collins.*) Has any order been issued by the Commander-in-Chief in India with reference to this?—He issued instructions that he would like as many men vaccinated as possible. There has been no order to say that men must be vaccinated. I doubt whether that is in his power, or whether it would be discreet for him to do so, even if he wanted to.

11815. But an order has been issued recently?—I can only tell you that a telegram came home from the Commander-in-Chief, saying that he would wish as many men coming out to India to be inoculated as possible; but that order did not come to the Committee.

11816. That is what you mean by a considerable effort being made by the Commander-in-Chief in India to secure the inoculation of as large a proportion as possible of the British Army in India?—Yes.

11817. (*Chairman.*) Now, you come to the importance of animal experiments in providing a pure food and water supply for the community. Those experiments for that purpose are conducted by various public bodies, are they not?—Yes.

11818. A certain number of public bodies all over the country?—Yes, public bodies are the great vivisectors in this country.

11819. I do not know that that would be accepted exactly as a doctrine by everybody—that they are the great vivisectors, but they are the great persons for making experiments on animals?—Yes.

11820. And you consider, I understand, that these experiments are necessary and of the highest value to the community?—Yes. Bacteriological methods are now largely employed as an aid in securing for the community a pure food and water supply. Animal experiments are necessary in many of these examinations, and many others would be impossible, but for knowledge previously acquired from experiments on animals. These methods are used not only to trace the origin and spread of outbreaks of disease due to contaminated food and water which may occur, but are constantly employed by municipal and other health authorities to test the purity of meat, milk, and water which is supplied to the public. To begin with milk, cheese and butter. In the examination of these products for tubercle bacilli, diphtheria bacilli, the Malta fever coccus, typhoid bacilli and allied organisms, animal experiments are had recourse to. The testing of dairy cattle with tuberculin may also be cited as a method utilised to secure a milk supply free from the infection of tuberculosis. In the case of meat, animal experiments are employed to discover the presence of anthrax bacilli and tubercle bacilli. In tinned meats and sausage poisoning the recognition that outbreaks of food poisoning (so-called ptomaine poisoning) are due to the bacillus enteritidis of Gaertner, and the bacillus botulismus, Ermengen, has been of use in indicating preventible dangers in connection with the consumption of certain foodstuffs, particularly those arising from the utilisation of infected meat for the manufacture of pies and sausages and for canning purposes. This knowledge depends entirely upon animal experiments. Since the causation of these outbreaks of food poisoning was understood sanitary inspectors have been alert to obviate this danger, and suspected carcases or preserved foodstuffs are submitted to bacteriological examination, and, if necessary, condemned. In the case of trichinosis, the relationship of this disease to the presence of the parasite trichina spiralis in pork and hams, and the consequent necessity for the examination of the carcases of pigs for the presence of the parasite was directly due to animal experiment. Water is examined for typhoid, anthrax and cholera bacilli and other organisms indicating pollution, such as bacillus coli communis, the recognition of all of which is either directly due to or based on animal experiments.

11821. Then you state the reasons why the varieties of animals employed in scientific experiments should not be restricted?—The following are, in my opinion, very strong reasons why the variety of animals employed for scientific experiments should not be restricted, and particularly why dogs should not be excluded from use for this purpose. (I.) Diseases peculiar to these animals, such as distemper and epidemic jaundice (piroplasmosis) require investigation both in the interest of possessors of these animals and of dogs themselves. (II.) One case at least is known, and this is a very important one, in which dog and man harbour two stages of the same parasite ecchinoccus or hydatids. This parasite is not responsible for any great amount of damage in Great Britain, but in some pastoral countries, notably Australia and Iceland, the proportion of the rural population which becomes infected with hydatid cysts is very considerable. In Iceland it has been estimated by the chief medical officer that one-seventh of the entire population were affected with echinococcus. It is also responsible for a similar cystic disease in sheep. In Australia the disease is very prevalent amongst the pastoral population, and I do not think I am exaggerating when I say that in at least 1-20th of the cases operated upon for tumours a hydatid cyst is discovered. The life history of the echinococcus occurs in two stages. One stage is a cyst which occurs in man, sheep or ox, and occupies almost any situation in the body, but is more commonly found in the liver, lungs, brain or bones. This cyst often reaches the size of a man's head. The other stage is a small tapeworm a quarter of an inch in length, which inhabits the intestine of the dog. This small worm is the sexual stage, and produces quantities of microspic eggs. These are swallowed by man or beast in drinking water which has been contaminated with the excreta of infected dogs. The eggs hatch in the alimentary canal of man, ox or sheep, and the embryo emerges and finds its way to various situations where it forms a cyst. The causal relationship of a minute worm in the intestine of the dog to a gelatinous cyst containing a gallon of fluid in the chest of a man, could, in this case, only have been ascertained by experiments upon the dog. (III.) Diseases common to man and dog have been fruitfully studied upon the latter, *e.g.*, rabies and ankylostoma. (a.) Pasteur's investigations upon rabies which led to the discovery of a means of preventing the occurrence of this disease in persons infected by mad dogs, were entirely conducted upon these animals, and could not have been arrived at otherwise, for the dog was the only other available animal in which the symptons and clinical course of the disease were at that time recognised. (b.) Ankylostoma or miner's worm is a disease seriously affecting the health of miners in many parts of Europe and Great Britain. It is due to multitudes of small parasites which attach themselves to the intestinal wall and suck the blood of their host. Until recently the way in which man became infected with these parasites was not understood. As the worms are found in the small intestine it was naturally presumed that the eggs were in some way introduced by the mouth, either in drinking water or food. All attempts to stay the extension of this disease by attention to the food and water were, however, without avail. In 1904 Loos (*Brit. Med. Assoc.*, 1904, *Mining Jl.*, *Aug.* 6, 1904), of Cairo, succeeded in demonstrating the precise channel through which these parasites affect an entrance into the human body, and succeed in establishing themselves in the alimentary canal. These experiments were made upon dogs. The ova of ankylostoma pass out in the dejecta of infected individuals. The ova hatch in the fæces and active small embryonic worms emerge. These, when placed on the skin of a dog were found to burrow through the epidermis, and were tracked into the small veins and lymphatics of the neighbourhood. Through either course they were carried by the blood stream to the venous side of the heart, and thence into the capillaries of the lungs, where they became impacted. Then they forced their way into the air cells of the lungs, and upwards into the small air-tubes, giving rise at the same time to a certain amount of catarrhal exudation. This exudation being swept upwards by the ciliary movement of the cells lining the air-tubes, the embryos arrived, after this complicated journey in the mouth of the animal and were swallowed. This demonstration of Loos (*Centralbl. f. Bakt.*, *Vol.* 33, 1903, *p.* 339) of infection through the skin was subsequently confirmed upon man by Calmotte (*l'Ankylostomiase* (*Anémie des Mineurs*) *Biologie, clinique, traitement, &c., Paris*, 1905) in France, and Boycott (*J. of Hygiene*, 1905) in this country.

11822. In the course of that journey through the man, or through the animal, do they do no moral harm to them?—No, no appreciable harm.

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11823. Then they got into the mouth, and, I suppose, passed out in the fæces ultimately?—Then they go into the intestine when swallowed, and then they develop into full-grown worms, which suck blood.

11824. And then they become mischievous?—And then they become mischievous. The dog and certain species of monkey are the only animals known to be capable of occupying the position of host to the human ankylostoma. The monkeys not being available, these facts could not have been ascertained without experiments on dogs. (IV.) A much more weighty objection is that physiology in Great Britain would be very seriously handicapped if, for any reason, dogs were not available for animal experiments. This would be a calamity, for pathology and medicine are founded upon experimental physiology, and any hindrance to the development of physiological knowledge must produce a corresponding diminution in the rate of advance of these medical sciences. I cannot better express my opinion on this subject than by quoting from the evidence given by Charles Darwin before the last Royal Commission on Vivisection: "I am convinced that to hinder the progress of physiology would be to commit a crime against humanity." I realise that physiological knowledge would not cease altogether to progress because an impediment were placed upon its development in this country, but am sufficiently patriotic to wish that my countrymen should maintain that honourable place as workers in this field of science which they at present possess. The physiologist must have access to animals of varying size. For some problems experiments upon small animals are more suitable, but many important functions of the body can only be investigated upon an animal large enough to permit of the accomplishment of the necessary experimental manipulations—e.g., the physiology of the circulation, respiration, and nervous control of the same. Physiological facts derived from experiments upon animals can only be applied to man when the mechanisms in the two cases present close similarity. In the case of investigatons upon the digestive system it is necessary that experiments be performed upon an omnivorous animal. The dog, as domesticated, is omnivorous, and can be obtained of sizes varying within a considerable range, and is, therefore, well adapted for such investigations. As well as possessing the advantages above indicated, an experimental animal must be accessible to the physiologist from the point of view of price. Monkeys and apes can, on this account, only be employed for special researches, such as those upon the nervous system, where their much greater similarity to man in regard to nervous organisation renders them preferable. Owing to the fact that dogs are not utilised as food in this part of the world there is, in our large towns, a continual supply of valueless and homeless dogs, which can be procured at a moderate price. Lastly, it is desirable that an animal be of such a character that it can be kept in health and looked after in the adjuncts of a physiological laboratory. Physiological laboratories are, as a rule, in cities and attached to universities, and not usually supplied with any stable accommodation.

11825. (*Chairman.*) Then the last head of your evidence is as to demonstrations upon living animals in teaching. That we have heard a great deal of, and, of course, it is a very important question?—The study of the structure and arrangement of dead organs and tissues, even when coupled with the best of descriptions of the functions of such organs and tissues, is inadequate to supply the student with a true conception of physiological processes. The only method with which I am acquainted whereby one is enabled to explain a fact or process is by reference to other facts or processes which are already part of the experience of the pupil. Physiological phenomena are so peculiar, and often differ so essentially from those which have already become part of the experience of the student during his study of chemistry and physics, that it is impossible to convey more than a distorted conception of most processes occurring in the animal body without some direct appeal to experience. I am confident that no one ever realised many of the most elementary physiological facts without witnessing experiments upon living animals. It is not possible, nor is it necessary, that every experiment which has contributed to present knowledge of physiology should be reproduced for the benefit of the student, for when he has seen and appreciated some few fundamental experiments it is possible to assist him to an understanding of further physiological phenomena by appealing to his previous experience. The opinion expressed above was forced upon me by my own experience as a student. I first studied physiology at a medical school, where I had no opportunity of witnessing physiological experiments. My conceptions of physiological phenomena were drawn from examination of dead animals and descriptions conveyed by word of mouth and printed books. This means of study energetically pursued enabled me to reproduce descriptions of physiolgical processes to the satisfaction of my examiners and myself. Subsequently I had the good fortune to study in the Physiological School of the University of Leipzig, and to see experiments, when I found that the conceptions which I had formed of what took place in physiological phenomena were distorted, and bore little resemblance to reality. The class of physiological experiments which it is necessary to show to students involves no suffering, as they are all conducted under anæsthesia. A mongrel which nobody will provide for, and which would otherwise be destroyed by the agency of the police, is drugged to unconsciousness, and his exit from this life is made an opportunity for the conveying of cardinal knowledge of function to the student, which knowledge cannot be attained in any other way. It has been suggested that the witnessing of physiological experiments would have a brutalising effect upon the student. I have never discovered this. When professor at Melbourne I performed from thirty to forty experiments upon living animals each year during my course of lectures. This class included six or eight young ladies. What I did notice, however, was that when confronted with experiments on animals the class became interested, earnest observers, and assumed the attitude of scientific searchers after truth.

11826. (*Dr. Wilson.*) May I take it that as Director of the Lister Institute you superintend generally the bacteriological methods of diagnosing infectious disease and the preparation of the various sera, and so on?—I have merely a general administrative supervision. I have skilled bacteriologists and experts in other departments whose special duties are of a particular nature.

11827. May I assume that the experiments on animals entailed in these methods of research and for the preparation of these preventive or curative substances are almost exclusively confined to inoculations or injections?—That is so in the Lister Institute—almost exclusively.

11828. Whenever diseased conditions are produced in animals by these experiments, do you concede that the element of pain can never be eliminated altogether?—I imagine that it is not entirely eliminated, either the element of pain or the want of feeling of well being.

11829. Discomfort?—I do not know whether it is. I certainly think that in some cases there is discomfort.

11830. And in some cases where death ensues after injections there might be a good deal of pain, of course?—In some cases there may be.

11831. Would you kindly tell me the names of the substances which you prepare as toxins and sera in the Lister Institute?—Do you mean for commercial purposes?

11832. For commercial purposes?—There is diphtheria antitoxin, tetanus antitoxin, dysentery antitoxin, and Yersin's anti-plague serum.

11833. Do they prepare any vaccine lymph at the Institute?—Yes, and vaccine lymph.

11834. All these are, as you say, prepared for sale?—They are prepared for sale.

11835. Could you give me any idea of the approximate number of doses of these preparations which are sold during the course of a year. I suppose that would not come within your knowledge?—I can ascertain it for you. I could not tell you off-hand.

11836. May I inquire how much one of these large doses of anti-diphtheritic serum costs—how much is charged for it?—These are sold to the public through our wholesale agents at the rate of 2s. 6d. for 2,000 units.

11837. So that for 20,000 units?—It would be ten half-crowns.

11838. It would be 25s. for a single dose of the anti-diphtheritic serum?—It would be ten half-crowns; that would be 25s. for the amount that is the average dose administered by the Metropolitan Asylums Board people, who have the very greatest experience in this matter.

11839. And in a case of illness of average severity, how many of these doses do you think would be given?—Not more than 20,000 units altogether.

11840. During the whole course of treatment?—Yes, certainly.

11841. What would be the initial dose then?—It might be given in three or four doses within the first 24 hours; that is the usual practice, I believe.

11842. But is it not the fact that serum is given for several days in some cases?—It may be; but that is not the best way to give serum. The best way to give serum is to get a much in as possible as soon as possible.

11843. So that it may be taken as an estimate that for an average case of diphtheria about 30s. would have to be expended in treatment with this serum?—I think so.

11844. I hope you will not consider this an invidious question, but may I inquire whether you yourself and the whole of the expert staff are paid by fixed salaries, or whether the salaries depend in any way upon the amount of these products which are sold?—No, the salaries are fixed, fortunately; because the results from the sales of these products are not very remunerative, and under any circumstances it would in no way affect the salaries of the staff. As a matter of fact they do not quite pay expenses.

11845. Although I have no doubt that the horses on the Institute farm, which are used for the production of serum, are carefully vetted as to soundness, is it not the fact that these animals do not come under the Vivisection Act at all?—They do not come under the Vivisection Act as long as we are not trying to find out anything new.

11846. Nor, of course, do any of the horses used by the large manufacturing chemists for the production of sera?—Provided they are not attempting anything in the nature of an experiment. If they are only manufacturing serum for commercial purposes they are, I understand, without the scope of the Act.

11847. But as they have to be frequently injected and repeatedly bled, and the greatest care, of course, must be taken as regards their condition of health, do not you think that they should come under Government supervision?—I see no objection at all. It seems rather an anomaly that you should be able to bleed a horse for commercial purposes without supervision and not for experimental purposes.

11848. And that would apply to the calves you use for calf lymph?—Yes, for vaccine. I gather that it was merely a question of expediency in framing the Act—the difficulty to draw the line.

11849. In the production of sera in the animals themselves do not you think that, as much depends upon the purity and standardisation of all these various sera and vaccines, all establishments of the kind should be more or less under Government control?—There is no control of it in this country. In most foreign countries, there is a Government control and an establishment for standardising such preparations. But that is apparently not the way we proceed in this country.

11850. Before dealing with the results gained from all these newer methods of prevention and cure, may I ask whether you conceive that in this country, at all events, many endemic or epidemic diseases have either been banished altogether or kept under control without having recourse to experiments on animals?—I cannot remember any just now. I think it is possible.

11851. Plague, for example—we have heard so much about that?—Yes, plague disappeared from this country for reasons we do not understand in the least.

11852. We got rid of it without any recourse to experiments on animals?—Yes.

11853. And was not leprosy also common in this country?—Yes.

11854. And coming to more recent times, has not cholera been practically banished?—Yes. I do not know that cholera was ever very common in this country.

11855. There have been several epidemics about half a century or more ago. And typhus and relapsing fever have been practically banished from this country, with the exception of a few outbreaks in poor and dirty courts?—Yes.

11856. And even typhoid fever has been vastly reduced, has it not, both as regards the mortality and prevalence?—Yes. I am not prepared to say how far the knowledge obtained by experiments on animals has not contributed to that reduction in the case of typhoid fever.

11857. But I am talking of prevalence before that time?—Yes, the prevalence had diminished before. I admit that.

11858. And even with regard to phthisis, was not the mortality from that disease reduced by half, at least, before Koch discovered his bacillus?—Yes, that is so.

11859. (*Dr. Wilson.*) And now I come to a disease to which I do not find any reference in your admirable and compendious *précis*—I mean small-pox. I presume you do not question the value of vaccination as a protective against small-pox?—No.

11860. But Jenner's great discovery that cow-pox was a modified form of small-pox was made through induction, was it not, and not through any experiments upon animals?—I think you will find that he took advantage of the fact that cow-pox existed in milch cows, and was associated with cow-pox in the milkers.

11861. (*Sir William Collins.*) Can you give the reference to that?—No, I cannot now

11862. (*Chairman.*) I thought it was only that they got pimples on their hands?—Anyhow, he took advantage of observations of conditions that were ready made. There were no experiments on animals in that case.

11863. (*Dr. Wilson.*) Though calves are now almost exclusively used for the production of lymph, vaccination until very recent years was largely carried out from arm to arm, was it not?—I believe so.

11864. And could be so carried out still if all children were believed to be healthy?—Yes.

11865. But did not Pasteur found his methods of protective inoculation on Jenner's discovery, and are not all these newer protective methods to which you refer more or less based on that discovery?—I presume that Jenner's work was quite familiar to Pasteur when he discovered that an old culture of chicken cholera was capable of conferring a certain amount of immunity; but I believe it was not due to Jenner's work that Pasteur discovered that fact. I believe it was purely accidental.

11866. But do not you think he was influenced at all by Jenner's discovery?—I should imagine that he would probably have been influenced in drawing his conclusions by his knowledge of Jenner's discovery.

11867. Is not vaccine lymph, whether it is obtained from the calf by vaccination, or from arm to arm, as in human lymph, believed to be a modified or attenuated form of the virus of small-pox?—Yes.

11868. And is not that the reason why it is used as a protection against small-pox, because it is believed to be a modified or attenuated form of the small-pox virus?—I imagine that is why Jenner first used it.

11869. And is not vaccinia, or the conditions following vaccination, whether in the cow or calf or human being, also believed to be a modified form of small-pox?—Yes.

11870. So that as one attack of small-pox renders a person immune against a subsequent attack, so vaccinia, or the conditions induced by vaccination, more or less effectually protects against small-pox, is not that so?—That is so.

11871. But is not this protective vaccine manufactured, so to speak, entirely within the tissues of the living body, and cannot be cultivated outside the body, whether it be of the calf or the child?—I am afraid I do not quite understand that question.

11872. That the virus of vaccinia, the lymph, cannot be cultivated, as you do other toxins?—That is so; it has not been cultivated.

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11873. It cannot be cultivated outside the living body. Is it not entirely produced in the tissues as in the arm under the skin?—It has not so far been cultivated outside the living body.

11874. In this respect, then, does it not differ vastly from any other vaccines you name, such as tuberculin, mallein, and Haffkine's plague vaccine or Wright's typhoid vaccine?—I will not admit that it differs vastly; it differs to the extent you mentioned—that the one has not been cultivated and the other has.

11875. And all these newer vaccines are prepared, of course by cultivating the so-called specific microbe of the disease on various organic media or substances outside the living body?—Yes.

11876. Is it not the fact that while a calf which has been successfully vaccinated is rendered completely immune against a second vaccination, the serum of successfully-vaccinated calves absolutely fails to confer any immunity at all on other calves?—I think that that is to a certain extent true, but not absolutely true. Experiments have been made showing, first, that the serum of a vaccinated animal will destroy vaccine lymph when mixed with it; and, secondly, that in some cases it has been found difficult or impossible, I believe, to successfully vaccinate an animal that has had several injections of the serum of a calf that has been vaccinated.

11877. But my question was whether it has not been proved by experiments that the serum of a successfully vaccinated calf will not protect another calf against vaccination?—Yes.

11878. Does not this fact alone, which you admit has been established, cast very logical doubts upon the protective value of the various sera you have mentioned?—I do not think so, in the least.

11879. Although it must be admitted that there is a specific virus, as I call it, for all these infectious diseases, it does not follow, does it, that the micro-organism which is characteristic of any particular infectious disease, is the actual *causa causans* of that disease?—I am afraid I do not understand the distinction.

11880. You believe that the microbe is the cause, wherever you find it?—I believe in the case of a large number of infectious diseases, many of which I have mentioned in my evidence, it is undoubted that the microbe is the essential cause of the disease.

11881. Take phthisis, for example. It is now admitted, is it not, that the tubercle bacillus is not found in the early stage of the great majority of cases of the disease?—Is not found where?

11882. In the sputum?—I do not admit it.

11883. Then is not the typhoid bacillus now found in the fæces of healthy persons?—It has been found in the fæces of healthy persons, especially in those who have suffered from typhoid and are known to have suffered from typhoid.

11884. Has it not been recently found in the jejunum, even in cases of typhoid further up the intestine?—I am not aware whether it is ever found in the jejunum or not. You can get it from the small intestine in cases *post-mortem*; but it is very difficult to isolate it.

11885. Has the typhoid bacillus ever been found in polluted air?—I should imagine not.

11886. Or even in polluted water?—It has been found in polluted water, certainly.

11887. But very rarely?—Very rarely, yes.

11888. Then as you admit that typhoid fever cannot be produced by inoculation in animals, how can it be said that the typhoid bacillus is the exciting cause of the disease? You cannot by inoculation produce typhoid fever in animals?—It is true that you cannot completely reproduce the disease by feeding with the bacillus of typhoid. The evidence so far of the typhoid bacillus being the cause of typhoid fever is not absolutely complete; but its association with the disease is so constant, and the fact that it can always be recovered from the organ of patients who have died of the disease (and, indeed, it was in the first instance discovered in the organs and not in the alimentary canal) renders it so extremely probable that it is the cause of the disease as to leave little room for doubt. And, in addition to that, you have now the evidence from agglutination and the bactericidal action of the sera of patients convalescent from typhoid fever, which is entirely specific for that organism.

11889. But is not this reaction sometimes found in respect to other diseases?—It has never been found in respect to other disease to anything like the same extent or the same dilution as for typhoid.

11890. But it has been found; so it is not an absolute test?—A slight specific reaction has been found in the serum of patients which are not known to have suffered from typhoid fever.

11891. What about para-typhoid?—The serum of para-typhoid cases reacts in a high degree to the particular para-typhoid bacillus.

11892. But there is at present a great deal of confusion between the two, is there not?—I think that confusion is being rapidly cleared up by bacteriological investigation.

11893. Now, take the bacillus of diphtheria; you have admitted that that is often found in healthy throats?—Yes.

11894. And is it not often confounded with what are called pseudo bacilli, like Hoffman's bacillus?—I do not think it is often confounded nowadays.

11895. Do you think mistakes in bacteriological examination cannot be so rarely made?—I think they are rarely made nowadays.

11896. If you inject the toxin manufactured or produced from the diphtheria bacillus into a horse, are any of the prominent symptoms of the disease, as seen in the human patient, developed in the horse?—Do you mean in the course of immunisation?

11897. Yes. You inject the cause of the disease, and you believe that you produce the disease in a modified form, do you not? You never get diphtheria; you never get the throat manifestations in the horse?—No, not when you inject it under the skin.

11898. Of course, if you are to inject any stuff into the throat it will produce irritation?—Not any stuff.

11899. I will not say any stuff; I do not mean it in any sense disrespectful to the serum—but any injurious material?—If you inject any irritant into the throat it will produce inflammatory changes.

11900. Can it be said that any of the diseased conditions produced in animals by injections of toxins derived from so-called specific microbes, bear any close resemblance as regards the incubation period, for example, and train of symptoms, as in diseases propagated by infection on the usual natural lines. Do you get the real disease with it?—You get the real disease in a modified form, owing to the fact that you inject the poison all at once instead of having to wait for it gradually to be developed and be absorbed day by day.

11901. But you are never certain that some of these induced diseases have periods of incubation as in human beings. You do not get these illustrated in animals?—If you inject a toxin directly into an animal you cannot expect to have the same period of incubation as when you infect it with a disease.

11902. But if you inject vaccine lymph into the calf you get always the period of development; that is to say, a vaccine lymph vesicle is there on a particular day after?—But I would point out that the two conditions are not analogous. In one case you are injecting a poison, in the other case you are injecting a living organism which has to grow.

11903. But in injecting animals with toxins you do not, of course, inject the organisms?—No.

11904. No bacillus has been discovered yet in small-pox?—No.

11905. Has any so-called specific microbe or micro-organism been discovered, apart from small-pox, for scarlet fever?—No.

11906. Or measles?—Measles is doubtful.

11907. In whooping cough?—There are good bacteriologists who claim to have discovered the organism responsible for whooping cough within recent years.

11908. But there is not general agreement upon it?—It is not established at present.

11909. Nor, of course, in hydrophobia has any specific microbe been discovered?—No.

11910. With regard to hydrophobia, compared with other prophylactics, does it not seem very incongruous

that Pasteur should have selected the spinal cord of an assumed infected rabbit as his vaccine against hydrophobia? That is a very singular instance, is it not, of the substance that is used?—I think I am right in saying that in the first instance Pasteur used the saliva of dogs, and subsequently used the spinal cord; and I think the train of reasoning was quite clear and logical which led him to expect that he would find the virus of hydrophobia within the nervous system.

11911. But as the virus is found in the saliva of dogs, would it not have been just as logical to try to discover it in the salivary glands, for example?—He did.

11912. And failed?—He found it unsatisfactory.

11913. Although the saliva itself contained the virus?—Yes.

11914. I see in your *précis* that you have given statistics to prove the success of the Pasteur treatment. But is it not a fact that all deaths are excluded which either occur during the treatment or within 15 days after it has been completed?—Yes, they are all excluded. May I say why?

11915. Yes; I know why?—I might put why in evidence, perhaps.

11916. Certainly. I was going to ask you has it not also been made clear by statistics that a large number of deaths have occurred after Pasteur's treatment? I cannot say how many just now, but I know that in 1898—that is nearly 10 years ago—there were over 800 fatal cases which have been collected after treatment?—I am afraid I do not quite know where I am. Am I to answer that first question?

11917. Yes, certainly. Give your reason?—The reason why they excluded all cases which occur within 15 days of the termination of the treatment was that Pasteur found that the incubation period after the direct access of virus into the central nervous system of a dog was 15 days, and he did not imagine that his treatment would be of avail when the virus had once reached the central nervous system; consequently, he never expected or claimed that it would be of any value when once that had taken place.

11918. He did claim it in the first instance, did he not? He was surprised that these deaths occurred?—I do not think he ever claimed it. Anyhow, that is why they were excluded. That was what he considered to be the limits of his treatment—that if the virus had once got into the central nervous system he did not expect that he would be able to counteract it.

11919. But was not that explanation given after deaths had occurred during treatment, and within a short period after it?—I think very likely.

11920. But this differs, then, vastly from, for example, the serum treatment in diphtheria, does it not?—It has nothing in common. The principle of that is entirely different. In the serum treatment of diphtheria the serum contains a body which neutralises the toxin of diphtheria—it combines with it chemically, and neutralises it. In the case of hydrophobia it is a vaccination. You are endeavouring to accustom the tissues of the animal to a resistance against the virus of hydrophobia by educating it first of all with a mitigated virus, and gradually increasing the severity of it. It is a process of education of the tissues of the individual. The other is a mere chemical reaction.

11921. There is no certain incubation period for hydrophobia in the human being, is there?—No. The onset of hydrophobia occurs, in the larger proportion of cases, in the second month after the infection; but it may occur in the first month, and it may occur as long as one year afterwards, and I believe cases have been recorded of even longer incubation.

11922. And when injected into animals, what is the incubation period?—It varies with the virulence of the virus injected.

11923. So that you admit, I suppose, that statistics of this description have to be very carefully scrutinised before any such Commission as this?—Satistics of every description have to be very carefully scrutinised, I find.

11924. Of course, as you have said, the disease has been effectively stamped out in this country without having recourse to Pasteur's treatment?—Certainly.

11925. Now, with regard to Haffkine's prophylactic against plague, can it be said that it has had any marked effect in stemming the spread of the disease in India?—I think the figures which I have given show that it has had a marked effect.

11926. But in a very limited number of cases compared with the millions of India?—It has not been attempted in the majority of instances.

11927. Is it, or is it not, within your knowledge that within the last few years an epidemic of plague was stamped out in Egypt without having recourse to Haffkine's vaccine or any other prophylactic?—There have been several small epidemics in Egypt, and whether Haffkine's vaccine was used or not I cannot tell. That is not the only way in which you can operate.

11928. Is not the progressive improvement of sanitary conditions the only effective basis on which prevention of the disease can be carried out?—Provided we agree as to what sanitary conditions mean, yes.

11929. Is it not the fact that before Haffkine's treatment became the vogue the mortality from the disease among the European population in India was comparatively slight?—Do you mean the case mortality or the general mortality?

11930. The general mortality?—The general mortality among the European population in India has always been slight.

11931. So that any statistics applied to Europeans with regard to the effect of Haffkine's vaccine on natives would be rather misleading, would it not? You cannot compare them exactly in the same line?—I do not know that they would be misleading. They would be a small number of figures I take it.

11932. But a vast number of Europeans did go in for it voluntarily; I think Lord Curzon himself, if I remember aright, was vaccinated?—Yes, I think so.

11933. But before treatment was introduced cases of plague were comparatively few amongst Europeans?—Yes.

11934. Now there is another point. Although the isolated groups of statistics that you give appear to show that the vaccine may possess some value, inasmuch as vaccination is voluntary, and not compulsory, would not those vaccinated, believing in the vaccine, possess, as it were, more physical resistance to the disease on account of that belief?—I see no reason to suppose that that would be the case.

11935. You do not believe in faith cure—in faith being an agent?—Yes, I do. I believe in faith being an important agent. But this is not cure; this is a question of incidence.

11936. Would they not be naturally inclined to take other reasonable precautions if they went in for the treatment?—I see no reason to suppose so.

11937. But they volunteer for the vaccination, do they not?—Yes.

11938. Would you not think that they would also be prepared to take other careful precautions?—No. On the contrary, one might imagine that those who had been vaccinated would be the most careless.

11939. But they know that a good many do not escape. That element would not come in. Have not somewhat disappointing results from the use of Haffkine's vaccine induced Dr. Klein to prepare a vaccine of his own?—Dr. Klein has, I believe, prepared a vaccine.

11940. I see that you admit that attempts to produce a serum for the cure of the disease have been disappointing; but is not Yersin's serum largely relied on in the eastern dependencies of France?—It may be so; I believe it is much more used in French territory than it is in other territory.

11941. And has not Lustig's serum also found staunch advocates?—Lustig's serum has been advocated.

11942. Now with regard to Professor Wright's anti-typhoid vaccine, was not its success in affording protection to our soldiers during the South African war seriously called in question?—It has been called in question.

11943. Were not the statistics also scrutinised by the well-known statistician, Dr. Karl Pearson?—Yes.

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11944. And characterised by him as somewhat misleading?—I think he disagreed as to the extent to which they justified the conclusions that Wright had drawn from them. It was a question of the extent.

11945. But they appeared to him not to support the contention that it had acted as a protective?—No, if I remember rightly they appeared to him to be less conclusive. He was dissatisfied with the numbers and the discrepancies in certain cases. But I think Karl Pearson was taking a very high standard as to what would be required of a vaccine.

11946. He took vaccination into account in making comparisons, as I have been doing?—Yes.

11947. Is it not the fact that during the Russo-Japanese war there was an entire absence of any prevalence of typhoid fever among the Japanese troops, owing to the careful sanitary and other precautions which were taken, without the aid of any such anti-typhoid inoculation?—I have not seen any account of the amount of typhoid fever in the Russo-Japanese war amongst the Japanese troops; I cannot say.

11948. But it was very trifling, was it not?—I do not know; I have not seen any account of it.

11949. You do not know whether they used any prophylactic or not?—No.

11950. Because you have just said that they do use the anti-dysenteric serum—Shiga's serum?—Yes.

11951. I do not know whether this is within your knowledge. May I ask whether any of this anti-typhoid vaccine was used to stem the spread of typhoid during the Lincoln epidemic some two or three years ago?—Not that I am aware of.

11952. Did not Wright try it in the Maidstone epidemic?—That was the first instance in which it was tried.

11953. Are not these inoculations both with Haffkin's vaccine and with Wright's followed by somewhat painful symptoms?—I think the symptoms are quite trivial.

11954. But at the outset, from the original vaccine?—In the early stage the dosage was considerably greater than is necessary now.

11955. But how do you know about its being greater than necessary? Was not the dose lessened because of the unpleasant symptoms?—No; the dose was lessened on account of the investigations undertaken under the auspices of the Anti-typhoid Inoculation Committee.

11956. By experiments?—By experiments.

11957. Then the original experiments could not have been relied on, and were not so reliable?—It is always possible to improve things as you learn more and more about them. And, in the first instance, as I have pointed out in my evidence, Wright was suddenly confronted with the vaccination of a mass of troops in South Africa, owing to the great prevalence of typhoid fever there, and had to put a process into use which no doubt he would gladly have proceeded with more slowly and improved his methods.

11958. Would not a great deal depend upon the particular strain of bacillus that might be cultivated?—I think not.

11959. Would that apply to any serum that might be produced as regards the strain of bacillus that must be used either for producing the vaccine or the serum. Is it immaterial?—It is generally held that it is more advantageous to use a bacillus of considerable virulence. But those are points which are not definitely determined.

11960. Has not Professor Wright recently made rather a damaging pronouncement concerning the serum treatment?—I should think it is very likely.

11961. Does he not now strongly advocate treatment by vaccine and not serum, as indicated by what he calls the opsonic index?—I believe that is so.

11962. Do you think that this new departure in bacteriological treatment can ever become possible in general medical practice, even if it should be proved to be successful? Could it be possible for medical practitioners, I mean, to take this opsonic index into practice?—I think it would be difficult for busy medical practitioners. But, if it were found essential, it will always be possible to find people capable of doing it.

11963. I come now to tetanus. Is it not the fact that the treatment of tetanus by anti-tetanic serum has fallen into disrepute?—I would hardly agree with you that it has fallen into disrepute. It has not justified the expectations.

11964. But it was first tried by injecting it under the skin, subcutaneously, was it not?—If I may put it this way: although I consider that you are treating tetanus at a very late stage of the disease by the time you have got symptoms of spasms, it would still be my duty to administer the anti-toxic serum.

11965. Then would you administer it by injecting it under the skin, or under the dura mater after trephining the patient, as has been done in some cases?—I should inject it intravenously. I have done so on more than one occasion.

11966. And not under the skin or under the dura mater?—No.

11967. You referred to puerperal fever. Has not the anti-streptococcus serum treatment for puerperal fever also been more or less a failure?—It has been unsatisfactory, owing to the fact that it is highly specific, and puerperal fever is caused by a great variety of different strains of streptococci. You are never sure that the serum will be made from an animal immunised with an homologous strain.

11968. But were not great expectations held out with regard to it at first?—I cannot help that.

11969. Can it be said that though pneumonia is one of the most prevalent of diseases, the anti-pneumonic serum has in any sense become popular as a remedy?—No, I think not.

11970. Does it not appear somewhat singular that anti-tetanic serum, for example, which was first used as a remedy should recently be almost exclusively used as a prophylactic?—No; it seems to me to be perfectly clear why it should be so.

11971. Why was it not so used in the first instance? They started it as a cure, did they not, and not as a prophylactic?—They did not find out the limitations until they had used it.

11972. That is to say, they did not use it as a prophylactic until they found that it failed as a cure?—No, that is not quite so; because it was used as a prophylactic in animal experiments in the discovery of it, the working out of the process.

11973. Even as a preventive of tetanus in animals, it was not used for some time?—I think it was; I think that was one of its first virtues that was discovered—its prophylactic value.

11974. It was not used as a prophylactic in tetanus at first?—I am not aware whether that was the case in practice or not.

11975. But in spite of the statistics supporting its claims as a preventive, it does not follow, does it, that the great majority of men or animals, in which it is used, would have developed tetanus if it had not been used. You cannot say that?—If you will leave out the first part, I could understand it.

11976. Very well, I will put it in this way: that we will suppose horses or people have abrasions, and that you use this serum for all of them to prevent the disease; it does not follow, does it, that any or many of them would have had the disease if they had not been injected?—It does not follow that every horse that has abrasions will become infected with tetanus. I prefer to put it in that way, if I may.

11977. May I ask whether anti-tetanic serum was used to prevent the disease occurring among the wounded during the South African War?—Yes.

11978. To a large extent?—There were only three cases of tetanus.

11979. But was that due to the use of the serum or to clean surgery?—It was due to clean surgery.

11980. And you cannot say whether the serum was largely used or not?—Whether the serum was used in those cases or not I am unable to say.

11981. But I mean generally amongst the wounded?—I do not think so. There was a large amount of serum taken to the South African War, but I do not suppose it was used. As a matter of fact they had no tetanus except three cases, and it would not have been generally used.

11982. You point out that in the Franco-Prussian War there were ·35 per cent. cases amongst the wounded?—Yes.

11983. And that was before the days of clean surgery?—It was before they knew how tetanus was transmitted, or what its causation was.

11984. So that after all is not the chief preventive against tetanus, to ensure that wounds are washed clean and kept as clean as possible?—Yes.

11985. Now I come to the much vexed question of the serum treatment of diphtheria. Of course, the antidiphtheric serum is believed to be the most successful of all sera in treatment, is it not?—Certainly.

11986. And that is not quite a harmless product, is it?—It is not quite harmless.

11987. It produces rashes in a great many cases?—Yes.

11988. And other disagreeable symptoms?—The most disagreeable symptoms are the rashes. They keep the child awake at night.

11989. Are not medical men encouraged to use it in all suspected cases of throat illness, and not to wait for the results of the bacteriological testing for the bacillus?—I think that is quite a wise procedure.

11990. But if the bacteriological testing fails to confirm the suspicions of the medical attendant and the case does not turn out to be one of diphtheria, would you not call this somewhat haphazard treatment and scarcely fair to the patient?—No. I should consider it had been done in the best interests of the patient, for the reasons given in my evidence as regards the necessity of the earliest possible treatment by all antitoxins.

11991. If it does not turn out to be diphtheria?—No harm is done.

11992. But you admit that it causes rashes?—The patient does not die of the rashes.

11993. How long does it generally take before the medical man receives the bacteriological report, after forwarding a swab from the throat of a suspected case?—Fourteen or fifteen hours.

11994. But supposing you have to send to the country by post?—You can telegraph the result.

11995. But the swab has to be sent?—Say from 14 to 24 hours, in this country.

11996. But as there are several other bacilli closely allied to Loeffler's bacillus, can the bacteriologcial testing be regarded as absolutely reliable without having recourse to the guinea pig?—Not absolutely; but it is a near approximation to the truth, which is of great value for practical purposes.

11997. How long would it take to complete a test for injecting a guinea pig?—About four days.

11998. In relying upon the bacteriological testing, is not the medical attendant placed in a somewhat invidious position; does not the delay in waiting for the report rather militate against the chances of cure?—I have just previously stated that I thought the injection of the serum in the case of a patient that was suspected to have diphtheria was the best treatment in his interest.

11999. But then that would be dispensing with the testing altogether?—No, because you can inject one dose. The earlier you can treat with antitoxin the less is necessary. You can give a moderate dose of antitoxin and then if the diagnosis is confirmed on the morrow by bacteriological examination, you can give more. If it is not confirmed you would give no more.

12000. But then you must admit that this waiting for the report of the bacteriological testing does militate somewhat against the chances of cure?—I advise not to wait.

12001. Then the testing could be dispensed with?

12002. (*Sir Mackenzie Chalmers.*) Are there not in a great many cases well-known marks which no medical man can mistake?—Yes.

12003. When you see the false membrane and the other symptoms?—Yes.

12004. (*Dr. Wilson.*) But the false membrane does not appear within a day or two, does it?—Not as a rule.

12005. Now the success of the diphtheritic serum treatment in this country is based, is it not, mainly on the cases admitted into the London isolation hospitals before and since the introduction of the serum treatment?—No; I have given a large number of tables from many other places.

12006. I am going to refer to some of those. You have admitted, I think, that if, for example, the type of the disease had become much milder in London since the serum treatment was commenced, that would influence the statistics somewhat, would it not?—I have stated that in my evidence about it.

12007. It would lessen their value. I think you have also stated that the bacteriological testing rather tends to make the cases appear more numerous, because slight cases are notified now which would not have been notified in old days?—I have suggested that that may very possibly happen.

12008. But if the death-rate per thousand of population has gone up in spite of the antitoxin treatment during the past ten years as it has, that surely could not be regarded as a statistical result in favour of the serum treatment, could it?—It could not be regarded as a statistical result in favour or against.

12009. Granting that the serum does assist in freeing or loosening the diphtheritic membrane owing to the reaction which it produces, and its effect on the circulation and minute blood vessels under the membrane, can it be said with certainty that its after effects are so beneficial as they are believed to be? I admit that it may have an effect?—I do not know how beneficial they are believed to be.

12010. You believe that the results of the treatment are beneficial, and you gauge them by figures?—Yes.

12011. For example, in your *précis* you state that paralytic symptoms are more common now than they were before the serum treatment?—Yes.

12012. And you hold that that is an argument that severe cases survive which would have proved fatal had not the serum been used?—I suggest that that is a possible explanation.

12013. Might I not also suggest that a possible explanation may be that, admitting that the serum may possess some action in assisting in loosening the membrane, its after effects may increase the number of paralytic cases?—You may hold that opinion, Sir.

12014. But is it not as logical as the one you have mentioned?—I have adduced some reason for my opinion; you have not done so yet for yours.

12015. I will not labour this point further. Though the general death-rate from the disease has gone up, the case mortality rate in the county of London, according to the statistics which you furnish, has gone down from over 20 per cent. in the pre-antitoxin days to about 10 per cent. in the last few years. That is correct, is it not?—That is correct.

12016. I see also that you refer to the statistics collected by Dr. Armstrong, Medical Officer of Health for Newcastle, in various towns in the north, centre, and south of England. You give that table?—Yes.

12017. These statistics show, do they not, that although there has been a reduction in the case mortality rate, it is not nearly so marked as it is in London?—That is so.

12018. And you attribute this, do you not, to the fact, or to the assumption, rather, that the antitoxin was not so freely used in these other towns as in London?—I think that is a probable interpretation.

12019. But is it not the fact that almost all medical men now use the antitoxin?—I think the majority of medical men use the antitoxin. But of that I have no way of judging except by the amount of antitoxin which is sold.

12020. Now I have here a very short extract from a paper recently published, which was read at a sessional meeting of the Sanitary Institute by Dr Hibbert, who is Dr. Armstrong's deputy in Newcastle, and is also physician to the isolation hospital. In that paper he deals with the statistics which you have collected, but groups them in a different form. For example, during the five years from 1900 to 1904, in eighteen northern towns the average case mortality rate was 24·7 per cent., in five midland towns it was 15·7 per cent., and in eight southern towns 13·1 per cent.; and in London you say it is now 10 per cent.?—Yes.

12021. That table shows, does it not, that even during the years 1900 to 1904 the case mortality rate of diphtheria was even higher in the eighteen northern towns than it was in London before the serum treatment was introduced. For example, before the serum

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treatment was commenced in London it was about 24 per cent.?—Yes, 20 to 24 per cent.

12022. And in the eighteen northern towns during these recent years, 1900 to 1904, it was as high as 24·7?—No, not quite; 18 in 1904 and 22·7 in 1900.

12023. Now he also makes these remarks in his paper:—"I may add that at the City hospital here" (of which he is physician), "where antitoxin in doses of from 6,000 to 12,000 units is given as a routine treatment, Dr. Harris and myself have not had evidence that antitoxin is the definite curative agent which it has apparently proved itself to be in the hospitals of the Metropolitan Asylums Board. There is, however, this fallacy as regards diphtheria statistics, that at least one southern town" (he does not name the town) "includes amongst its cases of diphtheria those 'contacts' in whose throats the bacillus is found, but who do not show any clinical symptoms of the disease." Does not this statement somewhat qualify your inference that the much higher case mortality in the northern towns is probably due to the fact that antitoxin is less used there. You see that they use it freely?—I am unable to say unless I have more data.

12024. I am quoting from this paper?—I am not complaining of the data that you have given me. I do not think the statement of this gentleman necessitates a modification of opinion on my part.

12025. But no medical practitioner, so far as I know, dare neglect the use of serum nowadays?—I am very glad to hear it.

12026. In country districts the parson would be down upon them, and the country squire would be asking why the serum was not used. As you quote several diphtheria statistical tables from Continental records, as well as from American records, I will now ask your opinion about some records which are given by the Medical Officer of Health of the City of Toronto, in his report for 1904, based on the diphtheria cases which were treated in the city hospital for ten years. It appears that there have all along been doubts among medical men there concerning the efficacy of serum—that is, in Toronto, and groups of unselected cases were treated in the hospital, some with antitoxin and some without, during those ten years; and this is a summary of the results for ten years. He says: "Since 1894, when the first dose of antitoxin was given at the hospital, the mortality results have almost always been unfavourable. In every year, save one, the percentage of deaths in antitoxin cases has even been higher than the ordinary hospital rate, which is of course influenced by the increased antitoxin mortality. In 1894 the figures were respectively 21·4, against 14·07, and this state of things continued without much improvement until 1902, when the antitoxin was about 1 per cent. under the gross hospital rate." Then he gives the statistics for 1903:—

	Cases	Deaths	per cent.
Total hospital admissions -	565	65	11·5
Antitoxin treatment - -	228	37	16·2
Ordinary treatment - -	337	28	8·3

But then he goes on to observe, "The statistics of the last decade, which can now be presented, embrace a sufficient number of cases on which to base a fair opinion of the value of antitoxin in hospital practice.

	Cases	Deaths	per cent.
Total hospital admissions	5,100	667	13·0
Antitoxin treatment -	1,132	181	16·0
Ordinary treatment -	3,968	486	12·2."

And then he says, "In a report of this kind any speculations as to the cause of the pronounced failure of a remedy from which so much was expected would probably be deemed out of place. A bare statement of fact is therefore presented, and this can, if necessary, be supplemented or substantiated by a large accumulation of original and classified evidence of unquestionable reliability." Do you accept that report?—Not without examination.

12027. (*Dr. Gaskell.*) Have you seen that report before?—I have not seen that report before.

12028. (*Dr. Wilson.*) You also believe that the serum is effective as a prophylactic?—Certainly.

12029. Would you recommend, then, that it should be used in every household, as they use vaccination, when a case of diphtheria crops up?—I should always use it as a prophylactic injection to any of my own children if one of them took diphtheria.

12030. But doctors do not inject themselves who attend cases of diphtheria, do they?—I imagine not.

12031. Now with regard to the anti-dysenteric serum; is that used in India, do you know; has it been taken up in India?—No, it has not been used in India to any extent.

12032. (*Mr. Tomkinson.*) How long were you in Melbourne?—Seven years.

12033. And with the exception of your experience there you have performed no experiments of a physiological kind upon animals?—Yes, I held a licence before I went out to Australia, in 1889 and 1890 in this country.

12034. I thought you said that that was the only case; perhaps I was mistaken. You there performed 30 or 40 experiments on living animals?—That was in connection with my lectures each year.

12035. And more besides?—A great many more besides.

12036. But, with the exception of that experience at Melbourne, have all your researches in the pursuit of science and discovery been confined to inoculation?—Oh, no.

12037. With regard to tetanus and hydrophobia, there is no claim that any cure of either has been discovered, once the disease has been established?—No, I make no claim.

12038. I think you include tetanus among the diseases which are occasionally cured. You say, "Unfortunately, the useful application of antitoxin serum therapy is at present limited to a comparatively few diseases; the most important of these are diphtheria, tetanus, and epidemic dysentery"?—It is usefully applied to prevent tetanus during the period prior to the development of symptoms.

12039. As a preventive, of course, I quite understand it. I thought you meant that it was usefully employed in the cure?—No; in the case of tetanus it is a prophylactic.

12040. And you claim that the use of this antitoxin or serum, or whatever is applied to either man or horses, is a prophylactic against tetanus?—Yes.

12041. I suppose you admit that that to a certain extent is guesswork?—No, I do not admit for one moment that it is guesswork. I submit that it is capable of scientific demonstration.

12042. It is a negative proof that tetanus does not follow in certain cases in which it is applied?—You are referring, I imagine, to the statistical evidence that it occurs in a certain number of wounds of a particular kind, and if you give tetanus antitoxin as a prophylactic it does not occur at all. I do not consider that that is guesswork. But, moreover, it is founded upon absolute demonstration. You can take a hundred animals and infect them with tetanus, and if you apply the prophylactic immediately not one in that 100 will develop the disease. Whereas if you take 100 similar animals and infect them with tetanus, and do not apply a prophylactic, they will all die of the disease. I call that the simplest form of scientific demonstration, apart from guesswork.

12043. (*Dr. Gaskell.*) In that case you speak of its curing disease?—It is really curing disease, because you apply the remedy after the infection, and before the manifestation of the symptoms.

12044. (*Mr. Tomkinson.*) In your *précis* you describe the treatment of a horse for antitoxin, and the effect upon the horse. You say, first of all, "It is carefully tested as being a healthy horse"; and then, "The first injection of the toxin may be followed by swelling at the site of inoculation, loss of appetite, general malaise, and rise of temperature." Have you often studied horses under this treatment?—I have seen a large number of horses under this treatment.

12045. Does the horse seem to suffer very much?—No, I think not.

12046. Is there great wasting?—Not at all. The horses compare favourably with those in anybody's stable. They are all fat and shiny.

12047. Have you seen the picture of the Khartoum dog?—I have.

12048. Do you consider that that would be the average result of certain inoculations?—It does not represent the condition of our horses.

12049. I suppose not, not from the description you have given of the horse, I am glad to hear. I wanted to know whether there was any suffering in the horse approaching the suffering that that dog had obviously had to go through?—No, nothing of the kind. There is no evidence that the horse during the process of immunisation suffers more than the man does who drinks too much the night before, say, and has a headache and general unsteadiness in the morning, and is off his feed. I should think that would put it in a simple, practical way.

12050. I see that you are strongly opposed to a limitation of the choice of animals for purposes of experiment?—Yes

12051. Because you say that dogs and monkeys, being in many respects nearest to man, are the most useful subjects for experiments?—Yes. I place particular emphasis on the accessibility of the animal for the physiologist.

12052. I suppose in all the experiments that you have performed of the minor kind, the animals have not been subject to anæsthetics?—Not in the inoculations.

12053. May I ask, are the laws regulating the practice of vivisection in the Colonies at all akin to ours?—They differ in different countries. In New South Wales, where I was for some years, there is no law.

12054. There is a law, I suppose, for the prevention of cruelty to animals?—Yes, but there is no vivisection clause in it.

12055. Then there is no immunity or permission given to vivisect?—No; it was not supposed that anybody would want to vivisect when they framed the law.

12056. And it would be a breach of the law prohibiting cruelty to animals?—No; there was nothing to prevent us performing any experiments we liked for scientific purposes in the University in New South Wales. There was no Vivisection Act.

12057. (*Sir Mackenzie Chalmers.*) There was no regulation?—There was no regulation of vivisection.

12058. (*Mr. Tomkinson.*) But there was a law for the prevention of cruelty to animals in New South Wales?—Yes. If anybody had supposed we were cruel they might have prosecuted us under that Act.

12059. (*Sir Mackenzie Chalmers.*) Nobody thought of that?—Nobody did.

12060. (*Mr. Tomkinson.*) Was there a Vivisection Act in Melbourne?—In Melbourne there was a clause in the Cruelty to Animals Act allowing the Governor in Council to give a licence to such persons as he thought advisable to perform experiments on animals; and that licence in my case included myself and every thing done in my laboratory for which I was responsible.

12061. Were you the only person to whom it was granted?—No; there were some few other persons. The pathologist and the bacteriologist and others in connection with the scientific Departments of Agriculture and Stock also held licences.

12062. Was the use of anæsthetics imposed by that licence?—Not necessarily. The licence was a comprehensive one, and in returns one had to fill in what one did.

12063. And, as a general rule, did you use anæsthetics?—Not for inoculations; always for physiological experiments.

12064. Did you find it easy, or at all events, possible, to keep the animals perfectly anæsthetised for a considerable time—as long as you wanted?—All day if one wished. There is no difficulty.

12065. (*Dr. Gaskell.*) Tetanus is a very painful disease, is it not?—I imagine so.

12066. With regard to the production of antitoxin for tetanus, is any pain connected with it?—No, not the proper production of antitoxin.

12067. (*Sir Mackenzie Chalmers.*) In the commercial production?—No.

12068. (*Dr. Gaskell.*) Would you tell us why there is no pain connected with it, because it has been put before us that there is pain connected with the production of antitoxin?—Because you never produce tetanus when you are immunising a horse.

12069. And short of producing tetanus, what is the case?—Nothing more than a slight rise in temperature and general *malaise* following each injection, which I have referred to in my evidence.

12070. I wanted to bring out simply that that applies to tetanus as well as to other things?—That is so.

12071. There is another question that I want to know about, too, and that is, what precautions are taken nowadays with respect to plague antitoxin—firstly, as to being sure that it is free from other deleterious products; and, secondly, as to standardising it. It has been said that the plague antitoxin has been out of favour in India, and would not be recommended on account of the want of precautions being taken in preparing it, and also the inefficiency of standardisation. Can you give me any information upon that subject?—The same precautions exactly are taken as with every other form of antitoxin to render it sterile and free from any contamination whatever. The reason why the plague serum is out of favour, as you phrase it, is, that it is not of very great value, unfortunately. Although it is the only rational remedy we have, it is impossible at present by injecting plague bacilli into the horse to make it respond by the manufacture of a sufficient amount of immunising bodies to be of great practical value. It is very weak compared to such sera as diphtheria and tetanus, and to that extent it is a disappointment.

12072. (*Sir Mackenzie Chalmers.*) You are referring to Yersin's serum?—Yes.

12073. (*Dr. Gaskell.*) In reality when bottles are sent out you cannot be quite sure that the standard dose put upon them is accurate. Is not that so?—I do not think there is any difficulty about that, because when you have a remedy that is too weak you give as much as you can of it.

12074. (*Sir Mackenzie Chalmers.*) You have told us that the work of the Lister Institute consists in preparing commercial sera and antitoxins. Is there any scientific work done there as well?—Yes, certainly. The preparation of the antitoxins that are of established value is only a very small part of the functions of the Institute. Practically the whole energies of the Institute are devoted to research work.

12075. How many people are working there?—I think there are about 25.

12076. Are any special lines of research going on now?—Yes, everybody is doing something or other.

12077. Merely inoculation work dealing with infective disease?—No; there is a chemical side to the Institute, where various chemical problems connected with biology are investigated.

12078. Does that involve animal experiments?—That may or may not be concerned with animal experiments. Often it is not.

12079. May I take it generally from your evidence that in your opinion research on the lines of the Institute cannot be carried on without animal experiments?—Most certainly. The Institute would have to go elsewhere if animal experiments were not permitted by law. It would have to go to one of the Colonies or America.

12080. You yourself have been on this Plague Commission, I think?—Yes.

12081. And you have been in India?—Yes.

12082. And were working there. What is the number of persons on the Plague Commission?—I think there are eight doctors.

12083. How many assistants?—I think, taking the minor assistants, it must amount to one hundred.

12084. Are all those people continually exposed to the virus of plague?—Yes, more or less.

12085. What precautions have been taken to secure them, if any?—They were inoculated at regular intervals.

12086. With Haffkine's prophylactic?—With Haffkine's prophylactic.

12087. Were they inoculated once a year?—They were inoculated more frequently than that, I think.

12088. Among your eight doctors who are continually handling this virulent material, how many cases have you had of accident?—None amongst the doctors.

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12089. Yet from the nature of your experiments I suppose you are continually in contact with the poison?—Yes. They take reasonable precautions so as not to get it inside themselves.

12090. One of them, at any rate, has had some abortive attacks of plague, has he not?—One of them suffered from what is called pestis minor. I did not mention that.

12091. However, no effects followed. The prophylactic was effective?—Yes.

12092. How far was it effective among the native assistants?—To my knowledge there have been three cases of plague amongst them.

12093. Fatal or non-fatal?—They were non-fatal.

12094. Therefore amongst several hundred people in continual contact with this disease, the Haffkine vaccine has been a successful prophylactic?—I can only say that they did not die of plague.

12095. And they had every opportunity?—How far that is to be attributed to Haffkine's prophylactic can only be found out by dealing with a larger number of figures.

12096. The statistics are too small?—They are too small for me to attach importance to.

12097. Still, from your own experience, do you think it is right or superfluous to inoculate with Haffkine's vaccine these people specially exposed to the plague?—I can best answer that by saying that I have been always careful to inoculate myself.

12098. More than once?—Yes.

12099. It produces, I suppose, for two or three days a great discomfort?—Not at all. In my case it produced no discomfort, because I went about it in a gradual way, taking a very small dose in the first instance, and the next week a little more, and so on, which is the best way to immunise anybody.

12100. Just as you would a horse?—Yes, just as you would a horse; the dose was small to begin with, and gradually multiplied. I was never in the least incapacitated either in the way of appetite or my ordinary occupation. I had a slight soreness.

12101. When a strong prophylactic dose is given there is generally for two days discomfort, is there not?—Yes, but it is very seldom that it would incapacitate a European from going about his ordinary business. Occasionally it happens that a man for some reason or other is distinctly invalided, say, for 30 hours; but that is unusual.

12102. It does not take place if the inoculation is carried out gradually?—No, it does not take place then.

12103. Your experiments in India with plague have been animal experiments, of course?—The experiments have been entirely on animals.

12104. And you have as the result of those experiments discovered, at any rate, the common mode of transmission of plague?—Yes, I think so.

12105. Namely, by the rat flea, as you say?—Yes.

12106. You would hardly say that that is the only mode?—It is not the only possible mode; but the evidence adduced by the Commission is such as to lead one to regard it as the only serious mode of epidemic spread.

12107. Therefore, at any rate, you have accomplished this by your experiments—that you know the evil to be dealt with?—Yes.

12108. Though you have not yet succeeded in discovering anything like a remedy after the outset of the disease?—No, not yet. Whether it is possible to satisfactorily interfere is another question.

12109. Still is it or is it not an important step to know the means by which a disease is communicated from one person to another?—I think it is an essential step.

12110. And that you say cannot be obtained without the use of animals for experiment?—No.

12111. Incidentally, if the rat flea is the ordinary way of communicating the disease, how is it that doctors continually handling these animals in transferring the fleas from one to another, were not bitten?—I imagine they were occasionally bitten. For one thing, they wore top boots.

12112. But in combing out the fleas, to take them from one animal to another, I mean?—The animal was chloroformed and the fleas were chloroformed. Otherwise it would have been impossible to have dealt with them; the Commission would have been exterminated.

12113. But you think there must have been occasional bites, and do you attribute the immunity to the prophylactic or not?—I attribute some of the immunity to the prophylactic.

12114. (*Chairman.*) Why did you say the flea was chloroformed?—The animal and the flea. If they were getting fleas off the animal they would put the animal in a receptacle with chloroform vapour, and that quiescent sent the animal to sleep and the fleas to sleep, and one could then pick them off at one's leisure.

12115. (*Sir Mackenzie Chalmers.*) And then apply them to another animal?—The fleas recovered from the anæsthetic in a few minutes, and they could then be dealt with as wished.

12116. Is it known at all how a flea biting either an animal or a human being can deposit the plague bacillus?—It is known that the flea whilst biting commonly discharges from behind.

12117. In the process of biting?—Nearly all suctorial animals whilst sucking blood at one end discharge the contents of the intestine at the other, and in that way they can discharge infection.

12118. Then the discharge of the intestine gets on to the puncture?—Yes.

12119. And that is the mode of entry?—That is the probable mode. It itches and you rub it.

12120. It is exactly the same with an animal?—Yes, the same with an animal.

12121. (*Chairman.*) Is it the discharge that gets into the wound?—I can only say that that seems the probable way in which infection actually takes place. The fact of the soiled proboscis is not excluded, but as infection may take place as much as fifteen days after the flea has fed upon the infected animal, during which fifteen days it has been fed upon the blood of a healthy animal every day, that practically excludes the soiling of the proboscis as the means of inoculation.

12122. (*Dr. Wilson.*) Does that apply also to the mode of infection conveyed by the mosquito in malaria?—No, it is more elaborate and more perfect in the case of the mosquito; because in the malaria parasite you have two stages, one which lives in the mosquito's body, and one which lives in the blood of man. The stage that lives in the mosquito's stomach ultimately breaks up into a large number of eggs, so to speak, which get into the salivary glands of the mosquito and are directly discharged with the saliva when biting.

12123. (*Chairman.*) But that discharge which you speak of from behind is common both to the flea and the gnat?—Yes.

21124. Only it is deposited further off the wound in the one case than in the other, I suppose. The gnat's body is very perceptibly longer than the flea?—Yes.

12125. (*Sir Mackenzie Chalmers.*) The gnat actually injects the poison?—Yes.

12126. The flea merely deposits it close by?—That is what we imagine is the usual case.

12127. There is no reason for thinking that the mandible of the flea is charged with infection?—There is the reason for not thinking it that I have just mentioned—namely, that the infection will remain for fifteen days, and the flea, being fed upon a healthy animal each time, would wash its proboscis, which practically excludes this mode of infection from practical politics.

12128. By means of experiments on animals have you discovered at all the length of the prophylaxis conferred by Haffkine's vaccine?—I do not think that there are very accurate data. The Commission has not concerned itself with that problem. There is certainly some immunity left in animals, and also apparently in man after twelve months from some of the statistics which were collected by Bannerman.

12129. Can you draw any general inference from the length of prophylaxis in an animal to the length of prophylaxis in man?—Only in a rough way.

12130. Is the incubation period the same in man as in animals?—Roughly speaking, everything takes place more rapidly the smaller the animal.

12131. The guinea-pig displays plague sooner after inoculation than the monkey, for instance?—Yes, all processes take place much more rapidly in small animals, so that everything goes a little quicker.

12132. (*Chairman.*) How about the flea. What is the period of prophylaxis?—The flea does not suffer.

12133. (*Sir Mackenzie Chalmers.*) The flea takes its plague as a food?—The flea is a mere passive transmitter.

12134. Is the Commission still at work, or has it completed its work?—It has completed what it undertook to do in the first instance, and it is now resting during the hot weather. The Committee have suggested to the Government of India certain other matters which it considers it would be advisable to investigate, and has expressed its willingness to do so if it is wished.

12135. It has issued one report?—It has issued an interim report: A second will be ready to-morrow, and there will be about three others, which will be more bulky, which deal largely with epidemiological matters and the analysis of a very large amount of evidence, which takes a long time.

12136. But, in your opinion, is it still important that further experiments should be made; or do you think that experiment has done its work, and it is no longer necessary to use animals for plague experiments?—I would not say that for one moment. I do not know what may be necessary to-morrow or next month. I do not know how knowledge is going to grow.

12137. There is only one other question that I think I need trouble you with. As regards the Khartoum dog which was referred to, I imagine that people who die of trypanosomiasis would be attacked exactly the same as that dog, and look as miserable in a photograph?—Yes, the people who die of sleeping sickness very closely resemble that dog.

12138. It is not a very suffering disease in the later stages, is it?—No; but that particular trypanosomiasis was a disease of mules, which was being investigated.

12139. For the purpose of finding a cure and preventing these animals from suffering from it?—First of all for ascertaining what the disease was, and then if possible how one could best prevent it by sanitary means, or cure it if possible; and dogs were used rather than mules, as being convenient and cheap in Khartoum.

12140. The only means of eventually combating the disease being to get exact knowledge as to its nature and its mode of transmission as your first starting point?—That is the case.

TWENTY-SEVENTH DAY.

Wednesday, 17th July 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. C. J. MARTIN, M.B., D.SC., F.R.S., recalled; and further Examined.

Mr. C. J. Martin, 17 July 1907.

12141. (*Sir Mackenzie Chalmers.*) There was one question that I forgot to ask you last week, arising out of your plague experiments in India. If I understood you aright, your experiments established that the ordinary bubonic form, excluding the pneumonic form, is not infectious?—Our experiments did not establish that. It was established by the clinical experience of the plague in India.

12142. It is quite consistent with your experiments with respect to infecting animals, when you excluded the fleas?—Yes.

12143. It had been established before, by clinical experience, that plague is not infectious and barely contagious?—In order that there should be no misapprehension as to what is meant by the words infectious and contagious, I might say that the evidence from the experience of plague in hospitals in India and in houses in India is that it does not spread from one patient to another, provided that it is confined to the bubonic form.

12144. Your experiments with animals confirmed that?—Our conclusions, from our experiments, are consonant with that experience.

12145. You would not put it higher than that?—No.

12146. You have been a professor of physiology?—Yes.

12147. But you are yourself a medical man, holding medical degrees?—Yes.

12148. A question has been raised as to whether licences and certificates under the Act ought to be granted to people who have no medical qualifications. What is your opinion on that?—I think there is no reason whatever why those persons should be excluded, provided that they are suitable investigators.

12149. Have you the same guarantees for character and humanity in their case?—I think so.

12150. You think that persons who are what I may call pure physiologists ought to have licences?—I think the pure physiologists are quite as humane as the doctors.

12151. But have the certifying authorities the same guarantee in that case? That is rather a difficulty. Of course, in the case of a medical man, he belongs to a great profession, and you have a hold on him that you have not in the case of men belonging to no recognised profession?—I think that the certifying authorities can easily acquire the necessary information.

12152. There are, I believe, distinguished physiologists who are not medical men?—The Professor of Physiology at Cambridge is not a medical man—one of the most distinguished physiologists in this country.

12153. Generally have you any alterations that you would suggest, from your experience, in the existing law and practice?—On the whole I am content with the working of the present Act, but I would take exception to the recent action of the Home Office in not extending a licence to foreign scientists wishing to work in our laboratories.

12154. Would you kindly give us your grounds for that; it is an important question?—First of all, we were all of us indebted years ago to foreign laboratories for the hospitality they afforded us; and there is a general brotherhood amongst scientific men in Europe and America, so that if one wishes to learn something in particular one goes to the man who has devoted his life to that particular subject, and one can go to his laboratory and learn his methods and whatever he is able to teach one. That recent action of the Home Office has prevented any foreign scientist from coming to England to learn from us.

12155. (*Chairman.*) Do you think that it might lead to retaliation on the part of foreign laboratories?—No, I do not think it would lead to retaliation. I think there would be too much sympathy with our disabilities for that.

12156. (*Sir Mackenzie Chalmers.*) May I put this to you as a difficulty which the Home Office might feel? In the case of an Englishman he is well aware of the prevalent English feeling on the subject of experiments on animals, and there is certainly an idea that foreigners are less careful of animal suffering than Englishmen?—I think that difficulty could be got over by placing greater responsibilities upon the heads of laboratories.

12157. That is what I wanted to know. Can you develop that at all; can you suggest what form it should take?—The form of licence that I had in Victoria grants that in principle. That licence included the head of a laboratory and such persons as he made himself responsible for; and he returned to the Government everything.

12158. He reports everything?—He reports everything.

12159. He is responsible for the whole of the work done in his laboratory?—He takes that responsibility; the head of the laboratory must take that responsibility.

12160. You think that under those conditions there

would be no objection to granting licences to foreigners who had proved character and qualifications?—None whatever.

12161. You would make the English head of the laboratory responsible for all the work done in his laboratory, and would only allow the foreigner to work under that particular specified licensee?—I think that would get over the difficulty. In the case of my own laboratory I certainly regard myself as responsible for the carrying out of the Act.

12162. That the law is honestly and carefully observed?—Yes, in my own laboratory.

12163. Whether you are experimenting yourself or anyone is experimenting under your supervision?—Certainly.

12164. (*Sir John McFadyean.*) I want to direct your attention to Question 11924 of your evidence, given on the last day. It was put to you in the form of an affirmation, but I take it that it was intended as a question: "Of course, as you have said, the disease has been effectively stamped out in this country, without having recourse to Pasteur's treatment?"; and you in answer to that question said, "Certainly." On reading it over it seemed to me that there must have been a misunderstanding, either on the part of Dr. Wilson, in putting the question, or upon your part in assenting to it; because it appears to imply either that it had been claimed by somebody that the application of the Pasteurian treatment to human beings might be expected to have the effect of exterminating rabies in dogs; or alternatively it appears to imply that the Pasteurian treatment was never applied to people in this country who were bitten by rabid dogs. I want to know from you now whether you are prepared to answer the question "Certainly," taking it in either of those senses?—When I said "Certainly," I meant that the Pasteur treatment had nothing to do with the stamping out of rabies in dogs.

12165. But it is qualified by the expression "in this country"?—But rabies is essentially a disease of the dog, and the treatment of the men who are the victims of the dogs' bites by Pasteur's treatment will not affect the disease amongst the dogs. That is why I said "Certainly."

12166. But have you ever heard it suggested that the Pasteurian treatment, whether it is good or bad, can have any influence upon the spread of rabies or the continuance of it among dogs?—No.

12167. You would not assent to the statement that in this country people have not derived any benefit from the Pasteurian treatment?—No.

12168. Do you believe, on the contrary, that some of them have derived benefit?—Certainly.

12169. Then I want to direct your attention to a question or two on the preceding page, beginning at Question No. 11914. These questions were with regard to the reason why, in the statistics purporting to set forth the failures of the Pasteurian treatment, those cases were excluded in which the disease developed within fifteen days after the treatment had been completed; and you have given the reason which to you appears to justify the exclusion of these cases in answer to question 11917. But the question I want to ask you is this: Are the available statistics greatly affected, supposing these cases are included among the failures; or could you give us any evidence showing what the percentage is in which the disease develops in persons subjected to the Pasteurian treatment, excluding no cases at all?—If these cases were not excluded the mortality would be about doubled, or a little more than doubled, so far as I can judge. I can only remember the figures in round numbers for one Institute, but that is a large institute, and included about 30,000 cases. The mortality given in the usual way, excluding persons who have died within fifteen days of the treatment, was about 0.45 per cent., and, taking the total mortality, it was about 1 per cent. (See note after Question 12173 below.)

12170. As compared with the mortality among non-treated persons?—In the figures that I collected and gave in my evidence that mortality would appear to be about 17 per cent. I can put in the exact figures, of course, if it is wished, but that is as nearly as I can remember.

12171. I think it would be as well to calculate out the figures showing what the calculated mortality for bitten persons was?—Not for every institute, I trust.

12172. The larger the number of statistics you put in, the more convincing it will be. Probably 30,000 cases would suffice for the one institute?—Yes, I can put my hand on 30,000 cases from one institute; but to put in the total statistics for 150,000 cases would be a great labour.

12173. I think we would be content with 30,000?—From 1890 to 1905 28,033 persons underwent Pasteur anti-rabies inoculation at the Buda Pesth Pasteur Institute. (Report of the Institute, 1905, by Dr. August v. Szekeley, director Centraebl. f. Bakt. Part 1. Referate vol. 40, p. 34, 1907.) Of these 159 died during or within 15 days after the treatment and 129 subsequently. The total mortality was 1.02 per cent. and the mortality, excluding the 159 cases from the statistics, 0.46 per cent.

12174. (*Sir Mackenzie Chalmers.*) Have you statistics of the Kasauli Institute?—Not with me; but they are given in the report of the Sanitary Commissioner for India year by year.

12175. They show the number of people treated, and the number of failures, but they do not show at what date the treatment was commenced?—No, they are not at all full in that Report.

12176. (*Sir William Collins.*) The evidence which you have been so good as to give the Commission has dealt mainly with the causation and control of infectious and infective diseases, has it not? So far as our knowledge and our power in regard to them have, in your opinion, been the result of experiments upon living animals?—Yes.

12177. Do you think that in the evidence which you have given to the Commission you have had due regard to what might be called the failures as well as to the successes of bacteriology?—I have not been concerned with the failures of bacteriology. There are failures in every form of scientific inquiry.

12178. For instance, I notice on page 1 of your *précis* you tell us that Pasteur's methods have led to the discovery of the causation of most infectious diseases to which man is liable?—I think that is so.

12179. Would you tell me, for instance, in the case of scarlet fever, have we identified the cause of it?—No.

12180. Or of measles?—No.

12181. Typhus?—No.

12182. Small-pox?—No.

12183. Chicken-pox?—No.

12184. Whooping cough?—Yes, that is probably correct.

12185. Would you name the organism?—No. I will name the authors—Bordet and Gengou.

12186. Are they the only experimenters who claim to have identified an organism as the cause of whooping cough?—I should think not.

12187. Do you regard their organism as established?—No, not as yet, but as very probable.

12188. Influenza?—Yes.

12189. Do you think that is established?—I think that is established.

12190. Would you name the author who has identified the true cause of influenza?—Pfeiffer.

12191. Have other authors claimed to have established other organisms as the cause of influenza?—No doubt.

12192. Mumps?—No, I think not.

12193. You would, I suppose, hardly have the Commission believe that sanitary science did not exist before the dawn of bacteriology?—No.

12194. You would hardly put aside as worthless the work of Chadwick, and Farr, and Simpson, and Radcliffe, and Dr. Kay, and Southwood Smith, and others?—No.

12195. Have there been means of stamping out or reducing mortality from diseases whose cause has not been identified as a particular organism?—Yes.

12196. Such as typhus?—Rabies.

12197. Typhus?—Small-pox.

12198. Do you agree that typhus is one of them?—Yes.

12199. And cattle plague, for instance; was that successfully dealt with before the identification of a par-

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ticular organism as its cause?—No organism has yet been identified as the cause of rinderpest, I presume you mean?

12200. You would dispute any contention to the contrary?—Yes.

12201. And I think you intimated, if you did not actually state, that puerperal fever, by the reforms introduced by Semelweiss, was dealt with satisfactorily as regards its prevention before the identification of any particular organism as its cause?—I endeavored to point out that it was not dealt with satisfactorily, because persons would not believe in its infectivity until they had evidence of a specific cause.

12202. Was it not claimed in Vienna so long ago as the sixties of last century that by the adoption of Semelweiss's views the mortality from puerperal fever had been reduced, if not abolished, in lying-in institutions?—Semelweiss was practically persecuted in Vienna, and driven from his post, and died in a lunatic asylum, from suicide, I believe.

12203. Quite true; and a monument in recent years has been erected to his memory in Buda Pesth?—I think so.

12204. But if you consulted a letter written by himself to the "Medical Times Gazette" in 1861, you will find that there were those who recognised the truth of his doctrine even in Austria during his lifetime?—No doubt.

12205. And before the identification of an organism as the cause of puerperal fever?—Certainly.

12206. Now as regards tubercle, was it not at one time contended by the Medical Department of the Privy Council that Dr. Burdon Sanderson had established the cause of tubercle? You quoted several authors, such as Klencke and Cohnheim, but I do not see a reference to Sir John Burdon Sanderson?—I think, if I remember rightly, that Dr. Sanderson's (as he was then) observations were in the direction of showing that tubercle was not a specific disease.

12207. That it was due to what he called the common septic ferment?—Yes. But Sir John Burdon Sanderson was mistaken, and, of course, he fully realised that later.

12208. And did not Sir John Simon, in reporting his researches to the Privy Council, state in regard to these matters that he thought they might "now be taken as settled"?—That I do not know; but if so, Sir John Simon was also mistaken.

12209. You know Koch's work on the cure of consumption?—Do you mean on the introduction of tuberculin inoculations about 15 years ago?

12210. It was a work entitled "The Cure of Consumption," translated and published in this country in 1890?—I am not familiar with that pamphlet, but I was in London at the time when Koch's treatment was being extensively applied in some of the London hospitals.

12211. It was claimed in that work that "Phthisis in the early stages can be cured with certainty by this remedy"—that is, tuberculin. Is that established?—No, that is wrong, I imagine.

12212. Is tuberculin recognised as a cure for consumption?—The revival of tuberculin as a means of treatment has occurred within recent years, and I believe is giving beneficial results under modified conditions with regard to dosage.

12213. Do you remember Koch's last statement when in this country in regard to the prevention of consumption?—Koch has made a good many statements.

12214. Did he not rather advocate what might be called the sanitary mode of prevention rather than any method of inoculation?—I think prevention is always better than cure.

12215. But was the prevention to be by the wholesale inoculation of tuberculin or by the rooting out of insanitary dwellings, in Koch's opinion?—By preventing infection by all means.

12216. The passage to which I was referring is "That above all other considerations he placed 'the domestic conditions under which the poor everywhere, but especially in great cities, have to live. It is,' he said, 'the overcrowded dwellings of the poor that we have to regard as the real breeding places of tuberculosis; it is out of them that the disease always crops up anew, and it is to the abolition of these conditions that we must first and foremost direct our attention if we wish to attack the evil at its root, and to wage war against it with effective weapons'"?—I think that is quite sound.

12217. That was in 1901; I think it was his last visit to London. I notice also in this work of Koch's on the cure of consumption that he makes this rather remarkable statement: "As regards the effect of the remedy" (tuberculin) "on human beings, it was evident at the very beginning of the experiments, that in one very important point the effect of the remedy on man is entirely different from that on the guinea pig, which is the animal usually experimented upon," and he goes on to say: "Here, again, is a fresh and conclusive proof of that most important rule for all experimentalists, though an experiment on an animal gives no certain indication of the result of the same experiment upon a human being"?—I think that is true—"no certain indication."

12218. You would agree with that opinion?—I agree with "no certain indication."

12219. Do you agree with the opinion of Koch in regard to the non-communicability of the tubercle of animals to man?—No.

12220. Did he claim to have established that as the result of experiments upon animals?—I do not see how he could establish the non-communicability of bovine tubercle to man by experiments on animals.

12221. Did he claim that the tubercle of animals could not be conveyed to man?—It was his opinion that it was anyhow an insignificant way by which human beings become infected with tubercle.

12222. Has that been disproved?—I think it has been disproved by the Royal Commissions on Tuberculosis both in England and in Germany, and by other individual workers.

12223. Has that been the result of experiments on animals?—Yes.

12224. Then the disproof has been the result of experiments on animals, in your opinion?—Experiments upon animals have been necessary to obtain data for coming to that conclusion.

12225. Did not Koch claim that it was from experiments on animals that he got the data necessary to come to his, in your opinion, erroneous conclusion?—I do not think so.

12226. Did he not use experiments upon animals in the course of his investigations?—Certainly.

12227. Have you looked into the statistics of tetanus as to whether the death rate is going up or down?—No.

12228. Do you remember Kanthack's paper with regard to tetanus; did he not collect some cases?—I do not remember it.

12229. I think you spoke in one place of the cause of syphilis having been established by experiments on animals?—Did I?

12230. Yes, on page 13 of your *précis*?—I do not think it amounts quite to the statement that you have put in my mouth.

12231. I am very anxious that you should make your own statement, and if I have put one into your mouth I apologise for having done so. Will you make your own statement? You speak of "many cases in which bacteriological diagnosis of a disease" has been arrived at by experiments on animals, and in that category you mentioned syphilis?—Yes.

12232. I was under the impression that you implied that syphilis could be diagnosed by bacteriological investigation upon animals. If I am wrong in that contention, will you be so good as to correct me?—You are quite wrong.

12233. Then, perhaps, you will state what is your contention?—The contention in that statement is that syphilis can be diagnosed by the recognition of the spirochœta pallida.

12234. Has not the spirochœta pallida been claimed as the cause of syphilis?—Yes.

12235. By Schaudin?—Schaudin himself did not claim it as the cause. He was the discoverer of it in syphilitic material. His partner, Professor Hoffman, suggested it as the cause. It has been extensively investigated by hundreds of persons in every country during the last two or three years, and has been found

to be almost invariably associated with syphilitic lesions of different kinds.

12236. Then am I right in thinking that, if not by you and possibly not by Schaudin, at any rate, by some observers, the spirochœta pallida has been claimed as the cause of syphilis?—By hundreds. And strength has been given to that claim by the fact that material containing the spirochœta pallida can be inoculated into apes and reproduce the disease.

12237. Up to a few years ago was not Lustgarten's bacillus claimed as the cause of syphilis?—Lustgarten claimed the bacillus as being the cause of syphilis, but he was mistaken and was speedily shown to be mistaken.

12238. For many years Lustgarten's bacillus was mentioned in text books as the cause of syphilis, was it not?—That may be so.

12239. You remember Mr. Watson Cheyne's remark in regard to that, in which he said, "But lately Alvarez and Favel state that they have found bacilli in the smegma taken from the healthy prepuce which stain almost in precisely the same manner as Lustgarten's bacillus and as the tubercle bacillus"?—That was one of the observations which showed that Lustgarten's contentions were not justified.

12240. Do you agree with Mr. Watson Cheyne in that statement?—Yes.

12241. You have given the figures for deaths from hydrophobia in Paris in your *précis*. Have you looked into the statistics for this country?—No, I have not looked into the statistics for this country. So far as I know, anti-rabic inoculation has never been performed in this country.

12242. But the Registrar-General for England and Wales publishes the figures of the deaths from hydrophobia?—I believe so.

12243. I have them before me from 1886 to 1905 (the same years that you give for Paris). I find (page 48), beginning with 1886, there were 2, 2, 4, 8, 4, 1, 1, 1, 2, 3, 1, 0, 1; and then since 1899, none—that is, of females—and of males for the same years (page 34), 24, 27, 10, 22, 4, 6, 5, 3, 11, 17, 7, 6, 1, 0, 0, 0, 2, 0, 0, 0. That makes 175 in the same years that you have given the figures for Paris as 124, I think. One is comparing England and Wales, of course, with Paris. Has it occurred to you to make any remark upon those figures?—I cannot see that there is any connection between those two sets of figures.

12244. Are you able to give any suggestion as to the reason why there has been hardly any fatal hydrophobia or rabies in man in this country since 1899?—I believe there has been no rabies in this country since about that time.

12245. Will you give us your opinion as to why there has been no rabies since 1899?—Because of Mr. Long's Bill to muzzle the dogs.

12246. The result of muzzling the dogs has been the abolition of rabies in dogs and the absence of hydrophobia in human beings?—Yes.

12247. I thought that would be the explanation that you would give me.

12248. (*Colonel Lockwood.*) And the restrictions on importation?—Yes, the quarantine restrictions.

12249. (*Sir William Collins.*) Are similar methods not practised in France?—No, people interested in rabies in France have urged upon the French Government to employ some such means, and have pointed to England as the great example. In Germany muzzling orders and the destruction of stray dogs is carefully carried out, and one way in which they have diminished the number of dogs of doubtful ownership was by doubling the dog tax.

12250. At any rate, in this respect this country seems to have the advantage of France?—It has the advantage of France in this respect.

12251. (*Sir John McFadyean.*) Has not Great Britain in this connection got an immense advantage through being an island? Is it possible to establish a quarantine for dogs all round the French frontier, which would be absolutely essential to securing the result we have in this country, would it not?—Yes, it would.

12252. (*Sir William Collins.*) Now, as regards the curative value of diphtheria antitoxin, I see you have given various percentages of case mortality of diphtheria apart from the use of antitoxin?—The case mortality from antitoxin varies much in different years and in different epidemics.

12253. On page 20 of your *précis* I see you give 80 per cent. in one hospital in Paris, 60 per cent. in another, and then on page 21 you give 21.6 per cent. in 1892 in London, and, as you say, there are great variations in the case mortality apart from antitoxin?—Yes, quite so.

12254. On page 23 of your *précis* you give some Russian statistics—cases treated by antitoxin, mortality 14.6 per cent; cases not so treated, 34.1 per cent. Have you looked into the figures of cases treated with and without antitoxin in London?—I do not know where to get any reliable figures that coincide in time for London. I doubt whether there are any available.

12255. The statistical report of the Metropolitan Asylums Board purports to supply them?—Coincident in time?

12256. Year by year. On page 309 of their Medical Supplement for 1906 I see cases treated with antitoxin, mortality 10.4 per cent.; cases not so treated, mortality 1.5 per cent. I thought you would desire to offer some explanation of that?—I am not aware of those figures. I have no doubt that there is a satisfactory explanation of them consistent with what I have put forth.

12257. You have given a statement on pages 23 and 24 of your *précis* as to the caution that is necessary in dealing with mortality statistics of diphtheria. Has the mode of diagnosing diphtheria changed largely since the introduction of the antitoxin treatment?—I imagine that a considerable number of cases are now diagnosed with greater certainty by bacteriological diagnosis of swabs from the throat.

12258. Prior to 1895 were any cases diagnosed in that way?—Yes.

12259. Will you give me the year prior to which no such case was diagnosed in that way?—Say 1883.

12260. When was the treatment by antitoxin and the diagnosis of it by finding the bacillus commenced?—The discovery of the bacillus was in 1882; it was established in 1882 and Roux's cases were treated in 1894.

12261. When would you say that the bacteriological mode of diagnosis was largely adopted?—As a routine practice in England about 1890—somewhere between 1890 and 1892, I imagine.

12262. And the antitoxin treatment commenced and became general in what year?—It commenced in this country about the middle of 1895.

12263. Has the introduction and the large use of bacteriological diagnosis been the means of bringing, as it were, under review a large number of cases which, prior to the use of the bacteriological diagnosis, were not called diphtheria?—I have indicated in my evidence that that is extremely probable.

12264. And that has to be taken into account in estimating the mortality?—Yes.

12265. Both case mortality and mortality per population, or not?—In estimating the case mortality.

12266. The case mortality only. Now, according to the figures published by the Metropolitan Asylums Board, the case mortality in 1888 was 59.3 per cent. for diphtheria, and then in the following years it fell to 40.7, 33.5, 30.6, 29.3, 30.4, 29.3. Those years would be prior to the antitoxin treatment, would they not?—Which was the last year?

12267. 1894—29.3?—Yes.

12268. There was a fall in the case mortality prior to the antitoxin treatment?—According to those figures, yes.

12269. And since 1894 the mortality has further declined from 22.8 down to 8.8 in 1906. I understand the point made by you, on page 23 of your *précis*, is, that if the cases of diphtheria are largely added to by the superaddition of a number of mild cases recognised by bacteriological diagnosis, that would have the effect of reducing the case mortality?—It would have that effect.

12270. Are you able to estimate the extent to which that cause is operative?—No.

12271. You yourself have written upon snake venom, have you not, in Clifford Allbut's "System of Medicine"?—Yes.

12272. I see you state there, "The mortality in persons bitten by the larger snakes of India and South America would not, from the scanty records available, appear to be more than 30 per cent."?—I may have said so

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12273. The mortality in persons bitten?—May I have the date; there are two editions.

12274. 1907 is the one this has been extracted from?—That is the last edition. I may have made that statement.

12275. You also, I think, in this paper, discuss the method advocated by Sir Lauder Brunton and Major Rogers?—I have no doubt.

12276. I find this passage, "Washing the punctures with any kind of solution is of course futile. Wall also considers that the application of any disinfecting agent to an incision through the wounds is almost useless; he found that such a method sometimes succeeded after the injection of the poison with a syringe, but was without avail after the natural bite of a snake. The same difficulty will be met with in the method lately advocated by Sir L. Brunton, Sir J. Fayrer, and Major Rogers, in which it is advised that an incision be made over the punctures with a lancet-shaped blade and crystals of permanganate of potash then be rubbed in. Major Rogers found this method successful in the case of cats, but in these experiments he knew accurately the direction taken by the point of the needle. In practice we find that it will only be by chance that the permanganate reaches the poison, which may be placed deeply in the areolar tissue and at some distance from the punctures." That was your opinion in 1907, I gather?—It is my opinion now.

12277. I thought you would probably give me that answer.

12278. (*Chairman.*) When you speak of the larger snakes in India and South America and the percentage of deaths, what do you include in the larger snakes, because some of the large snakes do not kill by biting?—No, the very large snakes are not poisonous.

12279. Do you include the cobra?—Yes.

12280. The rattlesnake?—Yes, and the fer de lance.

12281. The most deadly?—Yes.

12282. (*Sir William Collins.*) You think, then, that all this washing of punctures with any kind of solution is futile?—Yes.

12283. You attach some importance to the elastic ligature and to excision of the infected part?—Yes.

12284. Do you regard that as the only useful treatment that at present can be advocated?—No, I think you can get a certain amount of usefulness by the injection of antivenin—that is a serum.

12285. Do you agree that the difficulty with regard to that is to get antivenin, which is polyvalent?—Yes.

12286. Then as regards plague, you are a member of the Committee on Plague that is now engaged on its researches, are you not?—There is a Committee and a Commission.

12287. I am afraid I cannot discriminate; will you be so good as to discriminate between the Committee and the Commission?—The Committee is an Advisory Committee appointed by the Secretary of State for India, and the work has been done by a Commission.

12288. Is it a Royal Commission?—No, a Commission.

12289. Who appointed the Commission?—The Committee.

12290. The Commission has emanated from the Committee?—Yes, the Committee have placed their work in Commission. I was in the first instance, at the request of my colleagues on the Advisory Committee, in India for eight months organising things and starting the work, so that to that extent I was a member of the Commission as well as of the Committee.

12291. There was a previous Plague Commission, was there not, in 1901, which, I think, reported?—That was a Commission to take evidence. Ours is essentially a working Commission.

12292. Have you read the report of the previous Commission?—I have read most of it at some time or other. It is several large volumes.

12293. I notice that amongst their conclusions they state: "Plague has attacked persons who have undergone inoculation as many as four times in the course of two years previous to their attack," and, again, "as many as 8 per cent. of the inoculated population may suffer from plague, as was the case in Bulsar. Many varying influences have been at work in determining the rate of attack in different places, and it is impossible to give a numerical expression for the measures of protection against attack which inoculation confers"?—That was the opinion of the Indian Plague Commission from the early records of inoculation, I imagine.

12294. Six years ago?—Yes, it is a long time ago.

12295. I think you investigated, or had something to do with the investigation, with regard to certain untoward results which followed the use of Haffkine's vaccine at Malkowal?—There were certain questions of scientific fact and opinion concerning that accident, which were referred to the Lister Institute.

12296. As we have had the matter mentioned, and you yourself, I think, reported upon it for the Lister Institute, perhaps you will be so good as to state the result of your inquiries as to the source of contamination of tetanus, from which, I think, 19 persons died who were vaccinated with Haffkine's material?—The expression of opinion of the Institute was, as briefly as possible, in the following words: "That the probabilities are in favour of the view that the tetanus impurity was primarily in the fluid, but that they did not feel justified in asserting this as a proved fact in view of the possibility of contamination at Malkowal."

12297. Did the conclusions of the Institute coincide with those of the Commission?—I am not quite sure what the conclusions of the Commission were. I think they substantially coincided.

12298. I have before me a report entitled "East India (Punjab) Inquiry into Deaths from Tetanus" (Cd. No. 106), which states: "With reference to the findings of the Malkowal Commission, the Institute" (that is, your Institute, the Lister Institute) "were asked to report, among other matters (1) on the comparative efficacy of the standard and the new fluids as a protection against plague; (2) on the comparative liability of each fluid to contamination; (3) on the probable origin of the tetanus virus in the Malkowal cases. Their Report on these questions reached India early in December, 1904. They summarised their conclusions as follows:—(1) The Institute sees no reason to differ from the conclusions of the Commission that the new prophylactic is not less efficacious than the old. (2) The Institute is of opinion that in the hands of more or less unskilled workers it is easier to ensure freedom from contamination by Mr. Haffkine's 'standard method' of manufacturing plague vaccine than with the 'water-agar proces' as employed by him. (3) The Institute is in entire agreement with the Commission as to the value of 0.5 per cent. carbolic acid in restraining tetanus growth when added to plague prophylactic, and its experiments emphasise still further the importance of this addition in preventing the growth and toxin formation in a vaccine which might be liable to the possibility of contamination with the spores of tetanus. (4) The conclusions of the Institute coincide with those of the Commission that in all probability the tetanus was, at the time of the inoculation, in the fluid contained in the bottle, but that it is impossible to determine at what stage in its history or in what way bottle 53N became contaminated.'" Were those the conclusions that appeared over your signature?—Yes.

12299. Then the conclusions of the Institute apparently did coincide with those of the Commission?—I think, in drafting that report, I used the word "coincide" when I should not have done. I think the Commission went a little further. The conclusions of the Institute coincided with my view of the findings of the Commission, but, as was pointed out to me afterwards, the Commission had gone a little further than we had in their views.

12300. Do you think that the inoculation with Haffkine's vaccine or any other vaccine in India is having a valuable influence in checking the spread of plague?—Very valuable.

12301. Do the figures indicate that?—I think so.

12302. I have before me a memorandum of the Royal College of Physicians, in July, 1905, on Plague, in which they say: "The total number of recorded deaths from plague in India during 1896, the first year of the present epidemic, was 30,000. In many weeks of last year, 1904, the mortality amounted to over 30,000 a week, and the total mortality from the disease for that year was 1,040,000. This year 687,705 deaths from plague were recorded for the first four months. The

mortality from plague in India has become greater in each successive year for nine years, the year 1900 excepted; and since 1896 nearly four million persons have died of the disease." From recent replies in Parliament I gather that from September, 1906, to April, 1907, 5,326,000 deaths from plague occurred, and in April, 1907, there were 314,000. Has plague ever been more prevalent in India in the memory of man than it has been during the past year?—Not that I am aware of. In reply to a question by Sir Mackenzie Chalmers, last week, I was unable to agree that the evidence that some hundred or so inoculated persons under our control in India had not suffered from plague to any extent was of any value, on account of the small number; but I have since remembered that there was a very good observation made amongst all the sweepers of the whole City of Bombay, who were being utilised in connection with our Commission to gather in dead rats every morning, and the amount came to upwards of 5,000 a week, all of which were examined for plague by the Commission.

12303. (*Sir Mackenzie Chalmers.*) 5,000 dead rats, or sweepers?—5,000 dead rats. These men were under the control of the health authorities—the municipal officer of health—they were working to assist us. The figures there, I think, are large enough to be significant. There were 7,182 inoculated sweepers, and there were 418 uninoculated sweepers. The attacks of plague were as follows:—Amongst the 7,000 inoculated, 14; amongst the 4,000 uninoculated, 28. The deaths among the 7,000 inoculated were 13, and amongst the 4,000 uninoculated 26. In other words, the percentage of attacks among the uninoculated was 6.7, and among the inoculated 1.9.

12304. (*Sir William Collins.*) What paper are you quoting?—This is from the Report of the Plague Research Laboratory for the official year ending the 31st March, 1906.

12305. Were they all housed in the same way; because I saw some reference to sweepers being housed in certain quarters, in one of the official reports?—The sweepers are a low caste people; they always live by themselves. But not altogether for the whole of Bombay. In each quarter of the town there are certain streets where the sweepers live.

12306. Are there any living in mud huts?—Not in Bombay, I imagine.

12307. (*Chairman.*) The mortality among those attacked seems to have been about the same in each case?—Yes.

12308. 26 out of 28, and 13 out of 14?—Yes.

12309. (*Mr. Tomkinson.*) Are those percentages the percentages of those attacked inoculated or not inoculated?—Yes.

12310. Not the deaths of those attacked?—No.

12311. (*Sir William Church.*) To what should you attribute the fact that Semelweiss's teaching did not spread in Europe or Holmes' in America with regard to puerperal fever?—To the natural disability of man to assimilate a new idea.

12312. That is to say, they were unable to demonstrate the cause of the spread of the disease. It was a mere theory?—Yes.

12313. Until bacteriology enabled a definite cause to be demonstrated?—That is my opinion.

12314. Sir William Collins mentioned a number of diseases to which we are liable, and on which no light has yet been thrown by bacteriology as to their cause—mumps, typhoid, measles, chicken-pox, and various others; but are any of those diseases diseases which we share in common with other animals?—That has been one of the difficulties in ascertaining their cause. In most of those cases it is impossible to communicate them to animals and make an experiment.

12315. Therefore we have been unable to study them on animals?—Yes; I think that is largely the reason why we have proceeded such a short distance.

12316. I want to ask you one or two questions about diphtheria. First of all I want to know what is your definition of paralysis? You have been good enough to furnish us with a table showing the total number of cases of paralysis, some severe, and the number of fatal cases, in answer to Question 11736?—For the purposes of that table I imagine that the slightest interference with motive power in any part of the body is regarded as a case of diphtheritic paralysis.

12317. It would include then all cases of paralysis of the palate or of the eyes?—Yes.

12318. And also of the limbs?—Certainly, the slightest difficulty in swallowing will be called a case of diphtheritic paralysis.

12319. This includes everything?—Yes.

12320. You have no means of telling us whether the number of severe cases of paralysis has increased—cases in which the limbs are paralysed?—No; the figures are very small, you see, for severe paralysis; they would not be very significant.

12321. Then how was it, as you have been taking immense labour in collecting statistics with regard to the antitoxin treatment, that you were not acquainted with the information that Dr. Wilson gave us from Toronto?—I have no doubt that had I spent more time I could have made out a much stronger case. I could, anyhow, have given much larger numbers of figures. But Dr. Wilson quoted from a report in a provincial town in Canada, and I did not look in those places for it.

12322. You were not acquainted with it?—No, I was not acquainted with it.

12323. You are not any more acquainted than the Commission are with it; therefore you do not know whether it is exactly comparable?—No.

12324. Quite apart from the statistics, which I do not wish to go into, have you ever met with any doctor who has had a large experience of diphtheria before the introduction of antitoxin, and subsequently to the introduction of antitoxin, who has not thought it of value?—No, I have never met such an one.

12325. (*Colonel Lockwood.*) We were talking about muzzling being a preventive of the disease of rabies in England. Are you aware that in France, especially in the south, muzzling is largely used?—It may be used at the present time.

12326. It has been for ten years?—Has it?

12327. And is very strongly enforced, but rabies still exists very largely, does it not, in France?—I am unable to say offhand to what extent; it varies so much from year to year. It always occurs in epidemics. If you get one rabid dog you may get a hundred; they bite one another. And then it disappears almost entirely from the neighbourhood.

12328. But it does exist in France at the present time?—Yes, it certainly exists.

12329. In reference to the answers that you gave on the question of diphtheria, you are probably aware that the diagnosis of diphtheria in country districts is extremely loose?—I imagine it is not so efficient as it is in the Metropolitan Asylums Board.

12330. Are not the medical officers of health paid so much for every case of diphtheria that they detect? Perhaps you do not know?—I do not know whether they have a notification fee now or not. If so, it would be such a very small amount that I do not imagine that it would tempt even the most impecunious medical man to increase the rate. (*Note.*—Colonel Lockwood was presumably referring to the Medical practitioner and not to the Medical Officer of Health, and I answered the question in that sense.)

12331. You are aware of the very strong feeling existing in a large section of the public against the employment of experiments on living animals?—I am.

12332. To what would you attribute that feeling?—I think it is the natural instinct of a decent-minded man; and it has to be reasoned out of him.

12333. Do you attribute it entirely to sentiment?—I attribute it to the natural disinclination that any right-minded person has to inconvenience any animal unnecessarily.

12334. But do not the medical profession, for whom I have the greatest respect, equally share in that opinion?—I think the medical profession share equally in the sentiment which I have just expressed; but they understand the usefulness and the necessity for inconveniencing animals and utilising them for our own ends.

12335. You think that the reason why there is no repugnance, if that is the right expression to use, among the medical profession to experiment upon living animals is due to a wish to benefit human nature, and that their natural sentiment is not deadened by continual experimentation?—I think not at all, any more than the sympathy of a medical man for children

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in general is any less fine because he has to cause pain to children in the ordinary course of his occupation.

12336. You have told us in your evidence how valuable you think these experiments have been to science. Do you consider that science has suffered at all since the regulations existing at the present time have been imposed?—I do not think materially. I think a certain number of people may have wasted some of their time in correspondence; but I think no material hindrance has been imposed.

12337. Or do you think that our scientific men are less advanced than their brethren on the continent in consequence of those regulations?—No.

12338. When you answered the question with regard to the kindness of scientific men abroad in welcoming their *confrères* in their laboratories, and that we did not allow them in ours, there are some cases, are there not, of foreigners practising in England, what we may call roughly vivisection?—I believe not at the present time.

12339. There have been?—Yes, there have been. I was referring to the recent action of the Home Office, within the last few months, in refusing a licence to foreigners who come to work in our laboratories.

12340. But that only refers to the last few months?—Only to the last few months.

12341. Do you think that there are any extra measures of protection to animals that would tend to allay the state of public opinion, which, I am sure, you dislike as much as anybody? Could you not suggest to us any measure that would tend to allay that alarm that some of the public feel, and which, to the medical profession would not be repugnant?—I am unable to suggest any offhand, but I will think over the matter, and if I have any fertile suggestion to make, I will make it when I have studied Mr. Coleridge's proposed Bill.

12342. Thank you. That would be very valuable to us all. You have witnessed a good many experiments yourself?—Yes, a very large number.

12343. And in all those experiments have you never witnessed any, what we should call cruelty?—No.

12344. Much suffering?—Not much suffering.

12345. And in England, where the experiments you have witnessed are under licence, has the anæsthesia always been complete?—Yes, always.

12346. And the law strictly carried out?—And the law strictly carried out.

12347. And the carrying out of that law assisted and aided by all present?—Yes.

12348. Have you ever witnessed any levity on the part of the students?—I have had no experience with experiments before students in this country.

12349. But I will take your general answer, that you have never seen any cruelty exhibited, and that under the Act anæsthesia has always been complete?—Yes.

12350. Now, in answer to Question 12056, you stated that you had performed experiments in New South Wales, where there was no vivisection Act, and at 12060, in Melbourne, where there was a certain amount of restriction on experiments on animals?—That is so.

12351. As far as regards the progress of science in those two places, do you think there was any difference?—None whatever. I can take no exception to the form of the licence which I held in Melbourne; I think it was only right and proper.

12352. You see my reason, naturally, for asking. You do not think that the regulations imposed in Melbourne have any deterrent effect upon the advance of science?—None whatever; they were not in the least irksome.

12353. They existed?—They existed.

12354. (*Dr. Wilson.*) Have you yourself had any experience in the treatment of diphtheria?—Not a great experience.

12355. But you have had some?—Yes.

12356. And do you still adhere to that view which you expressed so strongly at the last sitting of the Commission, that if a patient sickening for diphtheria receives an adequate dose of antitoxin the case will not prove fatal, no matter of what intensity the infection may be?—Most emphatically. And I would point out that when asked by Sir William Collins whether I could supply any data from the figures at Chicago to substantiate that view, I said that I was unable to do so; but I find on looking through my evidence that those figures had already been supplied. 8,000 cases of bacteriologically verified diphtheria treated with antitoxin by the Chicago Health Department between 1895 and 1905 were cited, in which the mortality of 608 cases treated on the first day was reduced to 0.32 per cent; and just above that table was given the experience of the Brook Hospital of the Metropolitan Asylums Board for the years 1897 to 1902, in which the mortality of all cases treated on the first day was during all those years nil.

12357. (*Sir William Collins.*) Does the first day mean that the bacillus has been found and the invasion of symptoms, but no membrane in the throat?—It means that the patient is taken into hospital within 24 hours of the discovery of the disease.

12358. What does the discovery of the disease consist of?—It may mean a sore throat, which is certified to by a doctor, but in the majority of cases it would be a bacteriological examination nowadays.

12359. But would there be a false membrane in any or many of the cases on the first day of the disease?—No.

12360. Scarcely any?—Very few.

12361. (*Dr. Wilson.*) You admit that there are great errors of diagnosis without a false membrane?—No, I do not admit that. I admit that there are great errors in the clinical diagnosis of diphtheria, but I do not think that the errors of bacteriological diagnosis amount to more than 10 per cent. By "bacteriological diagnosis" in this sense I mean the examination of the growth obtained from the throat on one occasion.

12362. (*Sir Mackenzie Chalmers.*) May I call your attention to the paragraph in Sir James Thornton's pamphlet, "The Principal Claims on behalf of Vivisection," about Pasteur's anti-rabic inoculations, and ask you whether you have any observations that you wish to make upon it?—(*After referring.*) No, I have really no observations to make upon it. I have no doubt that Professor Billroth and Professor von Frisch may have expressed that opinion in early days. The Pasteurian treatment was not very popular in Germany when it was first introduced, owing to a good many reasons.

12363. There is no date given to Professor Billroth's opinion?—No, but I do not think Professor Billroth would have expressed that opinion recently.

12364. Is there any known or authenticated case in which it has been shown that the anti-rabic inoculation has itself created hydrophobia?—I am not aware of any.

12365. In the case of experiments on animals is rabies ever produced by inoculation in an unbitten animal?—Do you mean that should one take the cord straight out of a rabic animal and inject it under the skin of another animal would it give him rabies?

12366. No; what I meant was this: If you take the actual inoculations used for human beings, do the inoculations used for human beings, if inoculated into an animal, produce rabies?—If the animal is treated as human beings are treated it does not get rabies.

12367. That has been tried and tested?—It was, of course, tried a large number of times before Pasteur ever attempted it upon mankind.

12368. Then with the intensity of the treatment gradually increasing, does the same conclusion apply?—Yes.

12369. I was asking the question in reference to the fact that there certainly has been a fear that by undergoing the Pasteur treatment people may contract rabies?—I think that fear is unfounded. I gather that you wish to know whether there is a possibility, however remote, that by undergoing the Pasteur treatment, especially in its intensified form, a person might get hydrophobia who had not been infected by a dog?

12370. Yes?—There is a possibility, but it is extremely remote.

12371. (*Dr. Gaskell.*) Is it not the fact that the intensified form of treatment has been practically given up?—No; on the contrary, the intensified form of treatment is much more generally used now than it used to be, especially in Russia; they have had more confidence, and have realised more and more that the danger which Sir Mackenzie Chalmers has referred to is less and less.

12372. (*Sir Mackenzie Chalmers.*) Should you have any hesitation yourself in recommending anyone who

had been bitten to undergo the treatment?—None whatever.

12373. (*Mr. Ram.*) May I ask you one question with regard to what you said about the use of tuberculin? You said, as I gathered, that it was used, that doubts were cast upon it as to whether it was effective, and now there has been an increase in its use, with a modification of the dose?—That is so.

12374. What led to the re-introduction of it, or the re-use of it?—It was due to methods of ascertaining the action of tuberculin which were discovered by Wright, owing to which he found that the effect of a dose of 1-1000th of what used to be given in the early days was equally efficacious, and that the large doses were rather detrimental.

12375. Is Professor Wright now using it largely?—Extensively; nearly all the hospitals in London seem to me to be using it.

12376. Have you had any experience yourself of the results from its use?—My opinion on that would not be valuable, because I have not followed the cases. I have seen a series of cases one day and another series of cases another day. That is a question for the clinician.

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12377. Is it now claimed by Professor Wright that an absolute cure can be effected after there is undoubted tubercle by this treatment?—In the case of some forms of tubercle I believe that Professor Wright holds that opinion strongly.

12378. I think one of the witnesses told us it was being used in the case of tubercle in the bladder, which cannot, of course, be got at. Do you know any such case as that?—Yes, I have seen several cases of tuberculosis of the bladder and urinary apparatus.

12379. Treated in this method?—Treated in this method.

12380. Have you ever known of any case which has been believed to be an absolute cure?—Yes, but whether it was entirely due to tuberculin I am unable to say. I am not in a position to say. It can only be judged by large figures.

12381. (*Sir William Collins.*) Do you assert of your own knowledge that tuberculin cures consumption?—I have not, yet.

12382. I ask: Do you assert of your own knowledge that tuberculin cures consumption?—I am unable to assert that. I think, like all things in medicine, there are limitations to almost all our endeavours.

Mr. Dudley Wilmot Buxton, M.D., B.S., M.R.C.P., called in; and Examined.

12383. (*Chairman.*) You are a Doctor of Medicine and Past President of the Society of Anæsthetists?—I am.

12384. And you are Consulting Anæsthetist to the National Hospital for Paralysis and Epilepsy in Queen Square?—Yes.

12385. And you are Anæsthetist and Lecturer on Anæsthetics in University College Hospital?—I am.

12386. You have had a very large experience in the study and administration of anæsthetics?—Yes, very large.

12387. Anæsthetics, I suppose, were already used when you began your studies?—Yes.

12388. But since then it has been necessary to study them very carefully, and in the course of that study to make use of experiments upon animals?—That is so.

12389. We have had a good deal of controversial evidence as to the amount of sensation to pain which there is in animals under certain circumstances, under certain classes of anæsthetics, and under certain strengths of dose in application, and we should be very glad of your assistance in giving us information on those points, if you will. I think in your *précis*, which you have kindly sent us, you go very shortly and clearly into these matters. You say that anæsthesia is marked by progressive effects divided into degrees of narcosis upon the nervous system?—Yes.

12390. Will you explain that to us in your own way, especially with reference to the stages at which sensation of pain is lost, and more particularly with regard to the symptoms which occur during narcosis, and explain how far those may be ascertained to be symptomatic of pain or not?—Sensation in the first degree is *ab initio* deadened; in the second degree it is materially lessened; and in the third degree there is absolutely no sensation of pain. In the first degree I should say the sensation was very much lessened, in the second degree still more lessened, and in the third degree and after absolutely lost.

12391. How do you define the three degrees?—The degrees are roughly defined in this way. The first degree is generally from the commencement of the inhalation until the loss of conscious, voluntary movement; in the second degree there is movement, but it is not controlled by the higher perceptive centres.

12392. Is there sensation to pain in both those degrees?—There is some sensation to pain in the first degree, and less, but still some, in the second. I am now speaking of the induction period. Then the third degree is marked by the loss of all movement. The only movements which can obtain at the end of the second degree (all the degrees verge into one another) and in the third degree, are due to reflex excitation, and are not in any way connected with the higher perceptive centres.

12393. Are they accompanied by any sensation of pain?—Absolutely none.

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12394. Is that third degree what is sometimes called surgical anæsthesia?—Yes.

12395. Does that denote the amount of anæsthesia which any surgeon would insist upon in performing a serious painful operation?—Yes.

12396. To take a very familiar example, if you were administering an anæsthetic to a patient who was going to have a tooth extracted, would you go to the second degree or the third degree?—To the third.

12397. Whatever anæsthetic you are using, I suppose the third degree would have to be reached?—That is so. It is more difficult to discriminate the degrees under nitrous oxide (or laughing gas) than it is with chloroform, because the whole thing is so quick.

12398. But that is what you aim at?—You aim at complete anæsthesia, the third degree.

12399. You do not signal to the operator that he can begin until you consider that you have arrived at that condition?—That is so.

12400. Do you say that third degree would affect even the reflex movements?—Some of the reflex movements will persist, but not the superficial ones.

12401. Would you give us a precise definition of what you include in reflex movements. We have had the phrase used very often, but I do not know that anyone has precisely defined it?—A reflex movement, as I understand it, is a movement which is associated with stimulation of the nerve, the nerve travelling from the periphery, from the skin or the mucous membrane, to the spinal cord.

12402. You call that the afferent nerve?—Yes. It thus communicates with the centre of the spinal cord and then by an efferent, along an efferent nerve, it communicates with the muscle and produces contraction. In the chain of communication the higher perceptive centres are not included.

12403. You mean that that reflex movement travels from a nerve, along a nerve to the spinal cord and back again to the extreme of another nerve?—Yes, the muscular nerve.

12404. But it does not cause sensation to the spinal cord and the brain?—No.

12405. There is no sensation of pain in the brain?—No.

12406. It will produce movement although it does not produce pain?—That is so.

12407. Is that a common incident in operation under anæsthesia?—In certain operations, yes.

12408. Does the amount of movement depend upon the amount of pain which would be given by the operation?—No.

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12409. Does it depend upon the locality of the operation?—It depends upon the locality entirely.

12410. And in some operations, I suppose, you may almost rely upon it that, however satisfactorily you have arrived at the third degree, there will be reflex movements?—That is so.

12411. And that to a certain extent you can judge beforehand whether there will be any?—Yes.

12412. We have had witnesses before us who have described movements which some of them have considered to be reflex movements, while others have suggested that they were movements indicating pain in the animal. Have you seen that evidence?—I think I have read most of the evidence; I have read all that has been published, at any rate.

12413. I am afraid that I have not gone through it in view of your evidence sufficiently to put specific cases to you, but perhaps some other members of the Commission may do. At any rate, what you have said to me is a general account of what reflex movement you may expect without necessarily indicating pain?—Yes.

12414. Are you able to ascertain with any certainty, and if so, will you tell us by what means, when that surgical anæsthesia has been reached?—The sign that one relies upon mainly is the loss of the eye reflex, the conjunctival reflex. If I touch my eye it blinks. In the third degree if I lift the lid and touch the eye there is no blinking, the normal conjunctival reflex.

12415. We have heard, for instance, of corneal reflexes being seen, and they have been referred to as being evidence that pain was felt. What do you say as to that?—I say that it is not true.

12416. You say that that is no indication that pain is felt?—I do.

12417. Have you seen that corneal reflex happen when there has been complete surgical anæsthesia?—Yes, certainly.

12418. Have you said all that you wish to say as to how you ascertain when the third degree is reached?—I gave you the salient sign by which it is usually recognised; one would depend not only upon that but upon various other signs. In watching the muscular system, for example, in most cases the muscular system will show rigidity at first, and having passed through that rigidity will become flaccid. Then one would know by that that the patient had gone through the second and passed into the third degree.

12419. Has a skilled anæsthetist any difficulty whatever in assuring himself that perfect insensibility to pain has been reached?—None at all.

12420. There are plenty of symptoms by which he can make himself quite sure?—Yes.

12421. Does the difficulty of arriving at that stage differ according to the kind of anæsthetic that is used, or do you ascertain it by the same means in the case of every anæsthetic?—No, it differs slightly. For example, in nitrous oxide one would judge by the breathing and not by the eye. In the case of chloroform and ether you judge mainly by the eye.

12422. (*Sir Mackenzie Chalmers.*) The eye of the patient or your own eye?—The eye of the patient.

12423. (*Chairman.*) Nitrous oxide is what we call laughing gas?—Yes.

12425. It is only given to human patients?—Yes. cept in special researches undertaken to elucidate the physiological action of that agent.

12425. It is only given to human pitients?—Yes.

12426. You have to watch also that respiration is not interfered with, have you not?—Yes, very closely.

12427. And the necessity for that differs very much according to the anæsthetic employed?—Yes.

12428. Which are the anæsthetics that most affect the respiration?—Chloroform and all the volatile anæsthetics.

12429. Is it always safe to use chloroform as an anæsthetic?—It depends upon the method which you adopt. In many cases it is not at all safe. If you adopt a regulating method, and know the percentage and the danger of using a high percentage of vapour—if you can keep your percentage of vapour below 2 per cent., you may give chloroform with impunity. If you give above 2 per cent. or give it by a method which does not enable you to regulate your percentage, you are treading upon dangerous ground.

12430. And you can mix chloroform with something else, I suppose, which will diminish the danger to respiration?—That is one way of getting a lower percentage.

12431. I think there has been some confusion in some of the evidence before us as to the use of the term light anæsthesia. It has been spoken of by some witnesses—I do not think by professional witnesses, but others—as if it meant something less than complete anæsthesia. Would you explain what the term means as employed by skilled workers?—We mean by light anæsthesia that the patient has been put through the third degree, has been completely anæsthetised, and then the depth of the anæsthesia has been lessened, so that he has been absolutely anæsthetised, and then we find that by giving a smaller quantity later on he remains absolutely anæsthetised and yet he is actually perhaps in the second degree of anæsthesia for a long operation. It is extremely important in long operations to limit the amount of anæsthetic that is given. Light anæsthesia has, of course, to be clearly distinguished from incomplete anæsthesia, where the patient has never been completely anæsthetised. In one case you have the blood at a certain degree of saturation; in the other case you have an irregular distribution of chloroform or other anæsthetic, and the patient has never been completely anæsthetised; he is always hovering between consciousness and unconsciousness.

12432. But in all operations do I rightly understand that if you are to avoid pain you must begin, at any rate, by bringing the patient to the state of what you call surgical anæsthesia?—Yes, that is so.

12433. If it is a long operation, I understand that you rather lighten the amount of anæsthesia as compared with what you would do if it was a short operation?—That is my meaning.

12434. And when it is so lightened under those circumstances, it is your business as an anæsthetist to see that it is never reduced below the condition in which the patient is free from pain?—Yes.

12435. And can that be done consistently with what you have described as light anæsthesia?—Absolutely.

12436. There is no doubt about it?—None at all.

12437. If you spoke of incomplete anæsthesia, I suppose it would mean that the patient was not necessarily free from pain?—Yes.

12438. You use the terms complete and incomplete with reference to pain?—Yes.

12439. Is there any difference, and, if so, will you explain it to us, in the method of operation of anæsthetics upon man and upon the lower animals?—None.

12440. Do you note their condition of insensibility and the degree of it in the same way with animals as you would with man?—Always.

12441. And are the dangers of particular anæsthetics the same with animals, or do they differ according to different animals?—I should say that the danger was practically the same. Animals perhaps die more frequently because they may not have been so carefully watched.

12442. That is to say, they may not have been so carefully watched as regards the amount of stress put upon them?—Yes, the amount of dosage employed.

12443. But if they die of excessive anæsthesia, that of course does not involve pain?—No.

12444. It may involve death, but not pain?—That is so; it is a negation of pain.

12445. Are animals subject to the same stress upon respiration as mankind are?—Yes.

12446. And do they die in the same way that human beings would if they had too much anæsthetic?—Yes.

12447. Will you tell us what are the general anæsthetics that are used in surgical practice?—Nitrous oxide, chloroform, and ether are very common. Chloride of methyl is used occasionally, chloride of ethyl more frequently; bromide of ethyl has been used, and scopolamine; and for local anæsthesia strovaine, eucaine, cocaine, and others of that nature.

12448. Morphin?—Morphin is seldom used in the ordinary sense of an anæsthetic by itself. It has been used largely with chloroform and with ether, but it is not very commonly used now owing to its action upon the respiratory centres. In working with a volatile anæsthetic you limit within a short time the effect of

your drug. Having once injected your morphin, you lose control of the action of your drug, and its action may go on, and your patient may succumb; you have no means of preventing it. You can remove your chloroform; you cannot remove your morphin.

12449. Are morphia and morphin the same?—Yes.

12450. Are morphin and chloroform used together?—They have been used together; they are less used now than formerly.

12451. And chloroform and ether are used together?—Yes, they are, as a mixture. And the A.C.E. mixture is used, which was invented by Harley. Then alcohol and chloroform, the mixture 1 in 10, invented by Professor Schäfer, has been recently used, and a mixture of chloroform and ether which has also been introduced is largely used, and a very large number of mixtures containing different proportions of ether and chloroform. Petroleum ether was used in a mixture or solution with sulphuric ether and chloroform some years ago. The number of mixtures is very large.

12452. Are these mixtures that you make use of governed by the length of the operation or the particular condition of the patient, or the particular choice and fancy of the operator?—They are largely governed, I think, by the choice of the anæsthetist. He would, of course, be regulated by the necessity of the operation and the patient; he would examine his patient, and he would be familiar with the requirements of the operation, and he would make his choice upon that.

12453. Then, besides those anæsthetics which you have mentioned, are there other drugs which are not generally spoken of as anæsthetics, but are soporifics, which, however, will be anæsthetic in their operation if enough of them is used?—Yes.

12454. Is there any advantage in using a strong dose of an ordinary soporific instead of an anæsthetic?—None as a rule in surgery.

12455. You generally use an anæsthetic if you want to operate?—Practically always.

12456. There is no object in using a soporific?—None whatever.

12457. Have the use and the action of anæsthetics been ascertained largely by means of experiments upon living animals?—Certainly.

12458. By testing them upon animals?—Yes.

12459. I think you have already explained to us that if you apply too strong a dose to an animal you may kill it, but you kill it without pain?—Yes.

12460. Therefore, you are not so afraid of testing it strongly upon an animal as you would be, naturally, of testing it upon a human being?—That is so.

12461. (*Colonel Lockwood.*) Have you witnessed many experiments upon living animals under the Act?—A large number.

12462. Is the same care taken as to the degree of anæsthesia of which you have been speaking in the case of animals which are undergoing severe operations as in the case of human beings?—I should say certainly.

12463. And you invariably wait for the same degree of insensibility?—I always do.

12464. And the various precautions to insure that degree being reached which are used with reference to human beings you have always, so far as you are acquainted, seen used for animals?—Yes, you would adopt the same criteria.

12465. That you have always adopted?—Yes.

12466. That is to say, the anæsthesia has always been complete before these severe operations which are performed under licence are begun?—Certainly.

12467. And where, under the licence, the animal is required to be killed before recovery from anæsthesia takes place, that has always been carried out?—Yes, in my experience.

12468. I mean so far as your experience goes?—That is so.

12469. You have never witnessed anything to the contrary?—No.

12470. How many kinds of anæsthetics are used, or is it always the ordinary A.C.E. mixture that is used for animals?—No; it differs a great deal. Sometimes the A.C.E. is used, sometimes chloroform, sometimes ether.

12471. But always chloroform in some form or another?—Sometimes chloroform, sometimes ether.

12472. But always one of those two or the mixture?—Yes.

12473. Not opium, we will say?—Morphia has been used—not in my own experience.

12474. In your opinion is morphia a complete anæsthetic?—Quite.

12475. For severe operations?—Certainly.

12476. No pain would be present?—Not if you had a sufficient dose of it.

12477. Are you acquainted with the general provisions of the Act under which these experiments are carried out?—I am.

12478. Are you of opinion that they sufficiently guard animals from suffering?—Yes, I think that occasionally animals are unnecessarily killed.

12479. You mean under the provisions of the Act?—Yes.

12480. You think that they might safely be allowed to recover?—Certainly.

12481. That instead of adding extra precautions to those under the Act you would be in favour of relaxing some of them?—In that sense, certainly. I think that animals are sometimes extinguished when they might live long and have a happy life.

12482. You mean rather a happy life in the cause of science, not for their comfort?—Possibly for their own comfort.

12483. But you are talking, I presume, more of the scientific life?—Perhaps it is more a side issue, but I mean to say that in cases where an operation has been performed, it not being one to give the animal inconvenience afterwards, it seems a pity that it should be killed.

12484. Therefore, you are not prepared to assist us in any way by suggesting any further restrictions?—No.

12485. You think that the Act goes plenty far enough as regards the animals and the prevention of cruelty on the part of operators?—I think so. I think the well-being of the animals is quite safeguarded.

12486. You have never seen any cruelty in any work that you have witnessed?—Never.

12487. Nor have you seen an animal struggling or crying?—I have seen reflex movements. I have never seen conscious movements.

12488. (*Sir William Church.*) Have you had much experience in giving anæsthetics and seeing anæsthetics administered to animals other than man?—I have actually done it very many times to animals.

12489. We have been told by several witnesses that dogs are particularly difficult to keep under anæsthetics. Is that your experience?—Certainly not.

12490. There is no difficulty in keeping a dog for several hours under anæsthetics?—Not if you understand your business.

12491. We have also been told that it requires a very skilled anæsthetist to anæsthetise a dog. Is that so?—That would depend entirely upon the apparatus employed.

12492. Perhaps you would describe to the Commission the ordinary way in which a dog is anæsthetised for physiological purposes?—There, again, that must depend very largely upon the laboratory and upon what the purpose of the operation is. I should say that the most common method was by means of a Woulff bottle, allowing the air to pass over the surface of the chloroform.

12493. That would be a case in which the trachea is opened for a very prolonged experiment?—Yes.

12494. What is the ordinary way of anæsthetising a dog in a physiological laboratory?—By means of tow or lint with the anæsthetic poured upon it.

12495. Either by means of a mask, the animal being held under the mask, or by placing the dog in a basket with the anæsthetic?—Yes.

12496. And in your opinion is any special skill or knowledge required for anæsthetising a dog in order to produce anæsthesia?—No, I think not. Of course, an inexpert person may produce an overdose by giving too much. I do not think he is likely to err on the other side.

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12497. And in the case of those prolonged experiments in which it is necessary to administer an anæsthetic through a tracheotomy tube, do you think that there is any difficulty in regulating the dose of the anæsthetic?—None whatever.

12498. It is quite easy to regulate the dose of the anæsthetic in proportion to the air supplied to the animal?—Quite.

12499. Therefore, if the dose of anæsthetic was insufficient, the dose of air would be insufficient, and the animal would die?—That is so.

12500. That is your opinion?—That is my opinion.

12501. Have you any experience of administering morphia to dogs before you commence or as soon as you commence administering the chloroform, giving the two together?—I have no personal experience.

12502. Perhaps, then, you would not like to state before the Commission whether or not it is true that subsequent to the injection of morphia a lighter and less amount of chloroform is required?—I can certainly state that, because I have frequently employed morphia in the human subject before other drugs, such as chloroform or ether or scopolamine, and I have always found the amount of general anæsthetic required extremely small when morphia is given beforehand. The same, of course, would obtain in the case of the lower animals.

12503. Is it a common practice in human patients to administer a small dose of morphia subsequently to the chloroform?—Yes, it is very common.

12504. The purpose of it being what?—To keep the patient quiet and to prevent their having any subsequent pain, and to ensure sleep and rest.

12505. Just to keep them quiet?—Yes.

12506. Can you tell the Commission anything about the action of morphia upon dogs apart from chloroform?—I do not think, really, my evidence would be of much value upon the point.

12507. Have you any experience of administering opium, in the form of tincture of opium, to dogs?—No.

12508. You would not like to say what would be the effect of injecting a drachm of opium into the veins of a dog?—No.

12509. Would it be subject to pain?—No.

12510. Taking your division of the effects of chloroform into three degrees——?—I only got so far as the third; there are five degrees really.

12511. I think you said the first degree was until loss of control of voluntary movement?—Yes.

12512. The second degree was when the movements were not controlled voluntarily?—Yes.

12513. And the third degree was that of complete surgical anæsthesia?—Yes.

12514. In labour cases chloroform is frequently used, is it not?—Yes.

12515. Which degree of anæsthesia do you produce then?—The second, for alleviation of pain; not, of course, in a midwifery case necessitating a surgical operation.

12516. I am speaking of natural labour, of course, not labour in which surgical aid has to be rendered?—That would be the second degree. If any operation has to be performed it would be the third degree.

12517. We have heard a great deal of the value of different terms here which I think have been used without, perhaps, due consideration. You would consider incomplete anæsthesia any form in which voluntary movement remains?—Yes.

12518. But would you consider it a condition of incomplete anæsthesia in which uncontrolled movements occurred?—Oh, no.

12519. Is incomplete anæsthesia a term that is used often by anæsthetists?—Yes, I think so.

12520. And would it, in your mind, be equivalent to the term which has also been used before us of light anæsthesia?—No.

12521. It would be something less than light anæsthesia?—Yes, in a way it is quite a different thing. Light anæsthesia is complete anæsthesia; light narcosis is complete anæsthesia.

12522. You would include light anæsthesia under complete anæsthesia?—Yes.

12523. It has also been put before us by a witness here that, short of the abolition of the corneal reflex, you cannot have anæsthesia. Is that so, in your opinion?—I do not admit that.

12524. Is that belief held by any competent chloroformist?—I believe not.

12525. Reflex movements, which are of very different natures, can be called forth, can they not? Reflex movements of various sorts can be obtained in very different stages of anæsthesia?—That is so.

12526. That is to say, there are certain parts of the body which, when stimulated, produce reflex movements in what one might call a deep state of anæsthesia?—Certainly.

12527. That is in a state of anæsthesia so complete that all muscular action almost is lost?—All voluntary muscular action?

12528. I meant all voluntary muscular action?—Yes.

12529. Is so deep an anæsthesia as that a condition which is often called for in surgical operations on man—I mean, absolute abolition of all movement?—No, you cannot do that unless you kill your patient.

12530. But it is often done, is it not, in physiological operations upon animals for the sake of abolition of all movement?—If you have artificial respiration carried on, of course.

12531. Therefore, the fact that occasionally reflex movements may be seen in animals which are being operated upon by physiologists or research workers is no proof that they are not completely anæsthetic, although it is a proof that they are not in the profoundest condition of anæsthesia?—That is so.

12532. (*Sir William Collins.*) Do you hold a vivisection licence yourself?—Yes.

12533. Have you held it for long?—For a good many years.

12534. What form of licence is it?—The ordinary form of licence.

12535. You have no certificate?—No certificate.

12536. You hold it with a view to investigating the action of anæsthetics?—Quite so.

12537. And no pain would occur in any of your experiments?—None.

12538. Are there still some problems in connection with anæsthetics which require elucidation?—I am sorry to say there are.

12539. Should I be right in thinking that the discovery of anæsthetics was not due to experiments on animals?—The actual discovery of anæsthesia was not. Our knowledge of the precise action of anæsthesia is entirely due to such experiments.

12540. Is our knowledge of the precise action of anæsthesia complete to-day?—Not complete. Our knowledge of very few subjects is complete.

12541. Have you witnessed any surgical operation of any magnitude upon a human subject under morphia as the only anæsthetic?—I have never seen any.

12542. Would you advise it?—Certainly not.

12543. You have used, and other witnesses have used the terms, apparently discriminating between them, of light anæsthesia, incomplete anæsthesia, and complete anæsthesia?—Yes.

12544. Would it be troubling you too much if I asked you to give us a definition of each of those three terms now?—Complete anæsthesia is that degree of narcosis where there is no pain felt. Incomplete anæsthesia is that degree of narcosis where pain is felt. Light anæsthesia is complete anæsthesia. You have complete anæsthesia in profound narcosis; or you may have complete anæsthesia with light narcosis. That is as near as I can get to a definition.

12545. So far as I can gather, the appeal in each case is to pain as the differentia?—Yes.

12546. Can you suggest any other test, not subjective, but objective, by which the three varieties could be discriminated?—I have already mentioned various ocular phenomena that guide one as to whether there is pain or not pain, which would direct the anæsthetist's attention to the presence or absence of pain—the ocular phenomena and purposive movement.

12547. Is the conjunctival reflex present in complete anæsthesia?—Conjunctival reflex is absent. It may be very sluggish in very light anæsthesia; it is not brisk. In incomplete anæsthesia it will be fairly brisk.

12548. Then there is a stage of anæsthesia, I understand from you, in which the corneal reflex is present, and even brisk, and yet there is insensibility to pain?—By the corneal reflex, do I rightly understand you to mean the conjunctival reflex?

12549. I meant to use the conjunctival reflex all through, if you make a distinction?—I do.

12550. Will you tell us what the distinction is?—If you touch the conjunctiva you may get blinking. If you produce the condition of narcosis you get very little, hardly any movement of the eye, on touching the conjunctiva. If you touch the cornea you do get movement.

12551. Strictly speaking, the conjunctiva extends over the cornea, does it not?—Yes, I am speaking of pressing upon the border of the eye lid.

12552. Do I rightly understand that there is a period, in your opinion, in which the corneal and conjunctival reflexes are present, and yet there is insensibility to pain?—The patient, having been in complete anæsthesia, the normal conjunctival reflex may, during the continuance of the operation, be known to pass into a condition where there is slight reaction to touching the conjunctiva, and yet be absolutely free from pain.

12553. Is that condition one of a narrow margin which it is difficult to maintain?—Fairly narrow.

12554. Then in regard to the induction period, as you happily termed it, should I be wrong in thinking that the same period might occur—that is to say, absence of pain with presence of corneal and conjunctival reflexes?—I take it that that just in passing from the second to the third degree, when the conjunctival reflex is very sluggish, when the eyeball is almost fixed, there follows a period of absolute freedom from pain.

12555. Would that be a wider or a narrower margin than the period subsequent to induction?—Narrower.

12556. And more difficult, therefore, to maintain?—Yes.

12557. Have you studied the influence of morphia upon different varieties of the lower animals?—No.

12558. Should I be right in thinking that it differs in its action in the frog from its action in the dog?—No, I do not think so.

12559. Have you studied the physiological influence of curare at all as to whether it be an anæsthetic or not?—Only as a reader of literature. I have never worked with curare.

12560. Do you care to give the Commission your opinion on the subject?—Certainly, if they wish it. I can only go by what I have read. I know there is a case reported of curare poisoning, where the patient expressed himself as not having been conscious of any pain while the surgical operation was being performed. That is the case of Mr. Joseph White, of Nottingham.

12561. Do you conclude from that that curare is an anæsthetic?—That is my impression. I believe the general consensus of opinion is that it is an anæsthetic.

12562. Was that the opinion of Claude Bernard?—No; but a good deal of water has flown under London Bridge since Claude Bernard's day. In fact, we know more about it.

12563. Do you think the restriction in the Act which is at present operative, whereby curare is not held to be an anæsthetic, is erroneous?—No; I think if there is any doubt it is better to have the restriction.

12564. Do you think there is a doubt?—I think there is doubt in the minds of some people.

12565. Have you any doubt in your own mind?—I have an open mind upon the question. I do not know enough about it.

12566. Is the same apparatus used for giving anæsthetics to the lower animals as is employed for man?—What apparatus are you referring to?

12567. You referred to the Wolff's bottle?—We have a very similar apparatus, the Junker apparatus. The principle is very much the same; you pump air through.

12568. (*Sir Mackenzie Chalmers.*) You told Sir William Church just now that you would not recommend morphia as an anæsthetic in the case of human beings. Is that because you doubt its anæsthetic power, or because it has other deleterious effects?—Because it has other deleterious effects.

12569. And in a sufficient dose to produce anæsthesia would it be dangerous to life?—I think so.

12570. When you have been experimenting on animals with anæsthetics, what is the nature of the operations you have had to employ?—Taking blood pressures, examining the action of the heart, and examining the action of respiration.

12571. In all your experiments the animal is killed before it recovers consciousness?—Yes.

12572. Have you seen other people as well as yourself experiment?—Yes.

12573. You said, in reply to Colonel Lockwood, that you had never seen any cruelty. May I put it rather more fully, and ask you: Have you ever seen any carelessness in relation to experiments on animals which has tended to increase suffering?—No, I cannot say that I ever have.

12574. So far as your own personal experience goes, the experiments have always been carefully carried out, and the anæsthesia has been carefully maintained?—Yes.

12575. Do I rightly understand, as regards the administration of general anæsthetics like chloroform, that the sense of pain from the beginning of the administration is gradually dulled, or does it go by stages?—It is gradually dulled. The division into degrees is an artificial one. There is a progressive downward curve of loss of sensation.

12576. So that, with what you would call incomplete anæsthesia, you would have a dull sense of pain, but still some sense of pain?—In some particular animals you might have no sensation; in other more sensitive animals you might have considerable sensation.

12577. Has your attention been called at all to some experiments that were performed by Dr. Crile on surgical shock?—I am aware of Crile's experiments in book form.

12578. May I put this to you? Dr. Crile in his work on surgical shock mentions certain experiments which he performed under what he called incomplete anæsthesia. He was then asked whether those animals suffered any pain or not, and his reply was: "Certainly not; they suffered no pain," although the term that he used was incomplete anæsthesia. What should you say to that?—I should think that probably he used the expression, incomplete anæsthesia, somewhat loosely.

12579. In the sense in which you would use light anæsthesia?—Yes, I should imagine so. Even that would depend very much upon how near complete anæsthesia his incomplete anæsthesia was. He probably meant that he had not got the condition where the animal had completely lost the conjunctival reflex and the eyeball had not become fixed, and so on.

12580. But, nevertheless, it might have reached the stage that you call complete surgical anæsthesia?—Yes, but not if it was incomplete anæsthesia.

12581. He called it incomplete anæsthesia, but said that the animal suffered no pain. Could those statements be reconciled if he was using the term in the same sense that you do?—I do not think he could.

12582. You think he was using the term incomplete anæsthesia (if his statement is correct that the animal suffered no pain) in a different sense from that in which you use it?—Yes.

12583. You told us that you had read the evidence already given before the Commission, I think?—I have read the two Blue Books which have been published.

12584. Are there any comments that you wish to make on any part of that evidence?—I merely came here prepared to answer questions.

12585. Have you seen the evidence of Miss Lind-af-Hageby?—I was reading it while I was waiting to come in here. Yes, I have read some of it.

12586. Where were your experiments performed—at what laboratory?—University College, London.

12587. The same building in which she witnessed certain experiments?—Yes.

12588. Have you read her account in the "Shambles

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of Science" or in her evidence here of certain movements and cries which she perceived in animals that were being used for demonstration?—I read it in the evidence before this Commission.

12589. Can you form any opinion as to whether they were merely reflex movements, which she misinterpreted, or whether the animals were actually incompletely anæsthetised?—The impression left upon my mind is that she had mistaken reflex movements for movements due to sensation of pain.

12590. In the case of an unskilled person seeing demonstrations of that kind would it be easy to mistake reflex movements for purposive movements, accompanied by pain?—Extremely easy. I have known surgeons to mistake reflex movements for movements of pain.

12591. In ordinary surgical operations, do you mean?—Yes.

12592. You have administered anæsthetics in a great many thousand cases, have you not?—Very many thousands.

12593. In any considerable proportion of cases do you get either movements, or even cries, from a patient that you are satisfied is insensible to pain?—According to the area of the operation you certainly get reflex movements not infrequently. You get cries from patients quite infrequently, but it does occur. One can recognise them by the character of the sound—the absolute difference from articulate speech.

12594. It would only be a skilled person who would recognise the distinction between such cries and a cry of pain?—Yes.

12595. I suppose it is a nervous action?—It is a rush of air through the glottis. I can quite understand a person not familiar with the thing being taken in by it.

12596. Would you get such a cry in the same cases in which you get strong reflex movement, or would they be different cases?—Quite similar cases.

12597. Would it be specially in rectum surgery?—It is very common in rectum surgery. You may get an ascending action of the diaphragm, the air forced out by the diaphragm, when you get what is spoken of as the rectum crowing; the patient makes a curious crowing sound.

12598. Were any of these experiments which Miss Hageby describes such as would be likely to give rise to reflex movements?—I am afraid that I do not remember what the particular experiments were.

12599. (*Dr. Gaskell.*) Have you any definite opinion as to where these different anæsthetics act—on what part of the central nervous system?—I take it that they act upon the whole central nervous system.

12600. Do they act equally, do you think, on the whole central nervous system?—In the case of the general anæsthetics, chloroform, ether, and such like things, I should think their action was one gradually extending from the higher centres downwards if you push it far enough.

12601. That is to say, in your opinion the anæsthetic acts on the higher centres first?—Yes.

12602. Most thoroughly, I mean?—Yes.

12603. The highest centres of all you call the cerebral hemispheres?—Yes.

12604. Would you consider that the anæsthetic action of the drug affected the cells in the cerebral hemispheres distinctly earlier than those in the spinal cord?—I have no doubt of it.

12605. Have you a definite opinion at all where pain is located in the central nervous system?—The perception of pain is in the higher centres.

12606. Do you think that an animal or person deprived of the cerebral hemispheres can feel pain?—No.

12607. You feel sure, for instance, that there would be no pain felt in the spinal cord?—Absolutely none.

12608. When you spoke of reflex action to Lord Selby you mentioned only the spinal cord. I presume that you would allow that reflex actions of the same kind would exist above the spinal cord?—Of course. I only referred to spinal cord reflexes by way of explanation.

12609. So that if the cerebral hemisphere were removed you might still get by reflex action cries?—Yes.

12610. And movements apparently of pain?—Certainly.

12611. Still, in your opinion, there would be no pain?—No.

12612. Do you think that it is very difficult to distinguish between an animal which is making struggles owing to feeling pain, and an animal which is making struggles through these lower centres reflexively. Does it require experience to be able to distinguish that?—Distinctly, I think so.

12613. But, in your own experience of vivisection experiments, you have never felt any difficulty in distinguishing that in an animal?—None of my own knowledge.

12614. For instance, a cry would not be sufficient to enable you to judge whether there was pain?—Certainly not, neither cry nor movement.

12615. Then with respect to such a light anæsthetic or anæsthesia as is given by laughing gas, is it not the experience in giving laughing gas for a dentist that the patient cries often out and struggles?—Very commonly.

12616. And even goes so far as to hit the dentist in the eye?—Yes, I have known it happen.

12617. So have I. Is that a sign of pain?—No.

12618. On seeing that yourself, would you be able to say that the patient was not suffering pain?—Certainly.

12619. And the patient afterwards says he has felt nothing?—Yes.

12620. Have you ever seen any cases of complete consciousness under an anæsthetic, and yet no pain?—I never have; they have been recorded, but I have never seen it.

12621. I once saw Sir George Murray Humphrey give bichloride of methylene to a patient in a hospital, who came round, and I chatted with him during the whole time of the operation. You have never seen a case like that?—No. I have seen cases recorded; I have never seen them myself.

12622. May I take it that in your opinion most definitely a comparatively small, what you would call not a very deep anæsthesia is requisite, and yet you may be sure that the animal does not suffer pain, pain goes first?—Yes.

12623. It goes very early?—Yes, certainly.

12624. You spoke of being able to regulate with certainty the dose of anæsthetics by means of Woulff's bottle, the respiration of it?—Yes.

12625. Could you feel certain that even if that animal had curare by the same method of regulation of the bottle, that animal was all the time completely anæsthetised, using chloroform and ether?—Certainly, with the air passing through, I say so.

12626. You know the case of Bayliss v. Coleridge?—Yes.

12627. In your opinion in that case was there any doubt whatever about the animal being completely anæsthetised?—I have no doubt whatever.

12628. And there is no reason for us to take it, that that is a mode which would give no certainty that the animal was not suffering pain?—Certainly not.

12629. There is only one other question I want to ask you. Have you any experience of urethane?—None.

12630. You cannot give us any advice upon that?—I am sorry I cannot.

12631. (*Sir Mackenzie Chalmers.*) Have you ever experimented yourself with a local anæsthetic on an animal?—Not on a lower animal. I have experimented, that is to say, I have employed it on man.

12632. At present local anæsthetics are not used. They certainly would not be authorised under licences for experiments on the lower animals?—I do not think they are. I have no doubt that local anæsthetics do produce complete loss of pain.

12633. What sort of severe operations have you seen done without pain by means of local anæsthetics?—The removal of breast, tumours of breasts, the radical cure for hernia, removal of testicle, removal of lower parts of the rectum, and amputation of fingers.

12634. Under local anæsthesia?—Yes, you inject it.

12635. By cocaine or eucaine?—Or strovaine.

12636. Is that a new anæsthetic?—Yes.

12637. Have you actually discussed with a patient how far there was no sensation of pain?—Yes.

12638. Or dull sensation?—Yes.

12639. What has been the result?—In most cases they have felt no pain at all. Occasionally they have experienced discomfort.

12640. (*Dr. Gaskell.*) Do you consider that the pulse gives you any indication whatever as to whether the anæsthetic is sufficiently deep to prevent pain?—No.

12641. You could not judge by that at all?—No.

12642. (*Mr. Tomkinson.*) With regard to morphia, morphia, I suppose, was a substance well known, and was discovered long before chloroform?—Yes, opium was known.

12643. It was never used, was it, as an anæsthetic?—Opium was used.

12644. But not to any material extent for deadening pain, was it, in operations?—I believe so. I believe that in the pre-anæsthetic days opium was largely used.

12645. In operations?—In operations, to lessen the sensation of pain.

12646. I thought I had often seen descriptions of the terrors of the operating room?—I daresay. It was not universally used.

12647. I mean, when entirely relieved by any anæsthetic, and consequently the wonderful blessing to mankind of the discovery of chloroform as a real anæsthetic?—I never said it was universally used. I believe it was used in some cases.

12648. Do you hold that persons under the influence of morphia, or insensible, say, from alcohol—that is to say, insensible in the way of taking care of themselves, apparently in a profound sleep—are not subject to pain if they are cut?—I have no doubt of it, that they are not sensible to pain.

12649. You say that the value of anæsthetics has been discovered by experiments on living animals?—The properties of anæsthetics, I said.

12650. How is that specially so, more than experience of the use of it for human beings. You have to rely upon what human beings tell you as to their insensibility to pain afterwards surely, more than anything an animal can tell you?—You can examine the patients' hearts, you see; you cannot open their chests and examine the action of chloroform on the heart, nor can you examine their brain, nor can you examine the spinal cord. With an animal you can discover the action of chloroform or of ether, and you can tell what dose is dangerous, and what dose is not dangerous—whether it affects respiration or whether it affects circulation or other things.

12651. In physiological experiments on dogs, of which you have seen a great many, they are all strapped down first of all, are they not, before the chloroform is administered?—No, certainly not.

12652. Is there not some difficulty in administering it? The dogs struggle against it?—They are in a box; you can put them into a box.

12653. The dog struggles, I presume, to get out of the box?—No, it generally lies down and goes to sleep.

12654. Not at first. In the early stages does it not struggle or bite the box?—As a rule, my experience is that the dog turns round once or twice and goes to sleep.

12655. And then is it strapped down?—Yes, when it is unconscious, and tied on a board.

12656. That, you say, is to prevent reflex action?—No, I did not say anything of the sort.

12657. What is the object?—To keep them in the position required for the operation. In general anæsthesia for man, we strap the man up for operating on his anus. We do not do it to keep him still. We want him in a particular position there, therefore we tie him there, the same as with the dog.

12658. I forget whether you said that you had often seen or administered curare?—I have not.

12659. Do you approve of it?—In what sense do you mean?

12660. Do you think there is danger in it as a paralysing element, and thereby to some extent depriving the animal of the power of showing consciousness in the event of sensibility returning?—I do not think there is any danger, because it is always employed with chloroform or morphia—with some competent anæsthetic. I should not employ it myself alone, because I am not sufficiently satisfied of its action.

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12661. And in your experience it has not been necessary; you have hardly ever used it?—That is so.

12662. Allusion has been made to Dr. Crile's experiments; you have read of them, I suppose?—Yes, I have.

12663. They were of a tremendous nature, many of them?—Yes.

12664. Do you think there was not danger, in such shocks as that being administered to an animal, of pain being felt, even through the anæsthetic?—None. I think an anæsthetic would be absolutely of no use if you could not safeguard human beings and animals from the possibility of any shock, however severe, making itself felt through the anæsthesia; it would cease to be an anæsthetic. An anæsthetic, to be an anæsthetic, must be capable of preventing pain of any character or kind. By shock, you mean sensation of pain.

12665. In at least one of those operations it was admitted that the dog did show signs of feeling and of pain?—Then it did not have enough anæsthetic.

12666. Exactly; the anæsthesia was imperfect. I think a burner was applied to the dog's nose; it had attempted to struggle as far as possible, and it did show distinct signs of consciousness and pain. You have seen experiments on frogs, have you not?—Many.

12667. Are they anæsthetised?—They are pithed.

12668. Always?—Certainly.

12669. Does that mean the severance of the brain from the body?—Yes, from the cord.

12670. And does it absolutely preclude sensation?—Absolutely.

12671. Is that the case with other cold-blooded animals, too, which are operated upon?—If you destroy the higher brain they no longer can feel pain.

12672. Is pithing resorted to in the case of all cold-blooded animals?—The only cold-blooded animals I have pithed have been frogs.

12673. They are the only cold-blooded animals on which you have seen operations?—I think I may say so.

12674. (*Dr. Wilson.*) But even with pithed frogs it has been contended here that they do exhibit purposive movements. For example, they can reproduce the species—so it has been stated by witnesses. Would that modify your opinion as to whether all possibility of pain is banished by pithing?—Not in the slightest.

12675. Then with regard to these prolonged operations on human beings under chloroform, when slight anæsthesia is maintained towards the close of the operation, have you not to be very careful about varying the dose?—You have always to be careful in giving chloroform to a human being.

12676. You could not keep up a continuous dose as you would through Woulff's bottle?—Certainly you can.

12677. With safety you can do that?—Yes, certainly.

12678. In anæsthetising animals, no matter how severe or prolonged the operation, do you think there is no difficulty at all in maintaining absolute anæsthesia throughout?—I have no doubt of it whatever.

12679. But can there be absolute assurance that there is no pain?—I have no doubt whatever.

12680. In those deaths which sometimes take place during severe operations on the human subject is it not sometimes difficult to tell whether death is due to the anæsthetic or to the shock of the operation while the patient is on the operating table under a severe operation?—You have to decide that upon the merits of each case really. I do not think I can answer generally. I might go so far as to say that I should think you can soon tell. You really have to decide the case upon its own merits.

12681. You believe complete narcosis, of course, is complete anæsthesia, that with narcotics you can induce complete anæsthesia?—Yes, I do.

12682. You have been asked about morphia, and you stated about eucaine, and also urethane; have you any experience with regard to paraldehyde as an anæsthetic?—None.

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12683. Do you think it could be used for anæsthetising animals?—I have no idea. I have no knowledge of it.

12684. Have you ever anæsthetised animals for operations on account of disease or accident?—Yes, I have.

12685. And do you observe the same rules then as are applicable to human beings?—Yes, quite.

12686. But not exactly the same rules as are followed in a physiological laboratory?—Probably it is done in a more formal manner. I spoke about a dog being put into a box. On occasions when I have had to chloroform a valuable dog, or the dog of a friend, I have not put it into a box; I have put it on a table, patted it, and given it chloroform, which it has inhaled, and gone off without any sign of discomfort or fear.

12687. You would not think it necessary to use Woullf's bottle?—I should think it was very convenient to use a Woullf's bottle, but not necessary.

12688. (*Sir William Church.*) I should like to ask you one question, because I think, perhaps, some of the public and some of the Commission may not put a right interpretation upon the illustrations, which show the boards that animals are strapped upon for some of these operations. That is to keep them not quiet, but to keep them in a special position in order that the necessary operation can be performed, is it not?—Quite.

12689. It is equivalent to the fact that even still, for a large number of operations for men, it is found that it is advisable to strap the patient into a certain position before the operation begins?—That is so.

12690. Then from your experience you tell us that that is a necessity which very frequently arises in the practice of human surgery?—Certainly.

TWENTY-EIGHTH DAY.

Wednesday, 24th July 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.,
Mr. G. WILSON, LL.D., M.D.
Captain C. BIGHAM, C.M.G. (*Secretary*).

The Right Hon. Sir JOHN FLETCHER MOULTON, called in; and Examined.

12691. (*Chairman.*) You are a Member of the Privy Council, a Fellow of the Royal Society, and a Lord Justice of Appeal?—I am.

12692. For many years you have taken an interest in the progress of curative science?—Yes, I have taken a very keen interest in it both from the scientific side and also from what is called the ethical side. Having been a politician, and realising that the public take to themselves the right and the duty of controlling everything that goes on in the kingdom, I have realised for many years that the question which this Commission is directed to consider would come on in some form or other in the way of an inquiry; that, in other words, the justification for scientific research in connection with curative science would be examined. As I have in many other things observed, the advance of science takes the workers in science more and more beyond the ken of the ordinary public, and their work grows to be little understood and much misunderstood; and I have felt that, as in many other cases, the need would come for interpreters between those who are carrying on scientific research and the public, in order to explain and justify their work. The consequence is that I have considered the subject very much from the very point of view from which this Commission has to consider it. I have come, many years ago, to a very deep conviction upon it, and therefore I am glad to have the opportunity of putting before this Commission the reason for the faith that is in me.

12693. You say that you have taken great interest in the subject for a long time, and I believe you have very closely watched what has been going on with reference to it on both sides?—Yes, I have watched it very closely on both sides. Of course, you quite understand that so far as the scientific side is concerned, though I am qualified to judge of the scientific results and of the evidence for them, I am as an outsider immeasurably less qualified to speak with authority on the scientific points themselves than are some of those whom you have had before you; but, on the other hand, I think that with regard to the scientific methods and with regard to the bearing and meaning of the results upon the subject of this Commission, I am, as a close observer from outside——

12694. A highly qualified observer, I may say for you?—I am qualified to give some assistance to the Commission.

12695. In your long experience at the Bar, before you went on to the Bench, you were specially interested and employed in matters that dealt with questions of scientific investigation of all kinds?—Yes; I have had, I think, unrivalled experience in being what I have described as an interpreter between those who are doing the pioneer work in science and the people who have to judge of the results of their work; and it is, perhaps, more from that standpoint than from any other that I desired to put my views before the Commission with regard to this question.

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12696. In your view, I understand, the real issue raised by the agitation against experiments on animals is, whether or not curative science shall be studied experimentally or whether it shall become a purely observational science?—Yes. I think that in the controversy a great deal has been lost by looking at it in too narrow a light. It is not a question of vivisection—that is to say, the performing of cutting experiments on living animals; that is a mere nickname, a mere catch phrase which was originally invented, I have not the slightest doubt, for purposes of prejudice. I am not saying that with any desire to find fault. One always likes to get a name which expresses one's own point of view without further explanation; but it is a most imperfect representative of the real issue. The real issue is whether curative science is to be an observational science or whether it is to be an experimental science, and, as I shall hope to show this Commission, the importance of the decision of that issue is impossible to be exaggerated. But I shall deal with that a little later on, after I have dealt shortly with the grounds of the opposition to its being an experimental science.

12697. You have something to say, I think, on how far the opposition is based on humanitarian grounds?—Yes, the opposition purports to be based on the principle that we are bound to regard the sufferings of animals, and that the avoidance of the suffering of animals is a motive which ought to influence every man with a conscience. It is a consequence, no doubt, of a change which has come over the views of mankind decidedly in the right direction. Of old, cruelty was thought nothing of, whether it was to men or animals. You cannot read the history of the world down to, say, the seventeenth century, without feeling that there was an extraordinary callousness to suffering of every kind. A great change from that time has been passing over the world. First, there came the regard for the sufferings of men, and cruelty towards men gradually passed out of favour; and now the thought of human suffering and the desire to avoid it is one of the most unquestioned and influential motives that guide the action of mankind. Presently there followed on that the extension of the feeling to the sufferings of animals, and there is no doubt that now, with the best part of

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humanity, sympathy for the sufferings of animals and the desire to lessen those sufferings are most influential motives of action. Now that appears to me all in the right direction, and so far as my opinion goes it is not only not contested by either party to this controversy, but both of them would in words accept it as a duty to lessen so far as possible the sufferings of animals as well as of men. I do not wish to be misunderstood when I talk about the sufferings of animals in the same breath, as it were, with the sufferings of men. I must therefore point out a very broad distinction between the two, which must be borne in mind if we are to come to right conclusions in this matter. The sufferings of animals are substantially physical only. The sufferings of men are not solely physical. Almost as important a factor in that suffering is mental suffering, arising partly from a man's relations to society—that is to say, that death or sickness will cause pain and suffering and trouble and misery to those who are intimately connected with the man, and partly in the individual himself. He is capable of feeling anxiety, regret, dread, and many other things which are of the most serious importance in measuring human suffering, but which are practically absent from animal suffering. I do not say this in any way to detract what I have said about the importance of avoiding and preventing, so far as we can, animal suffering; it is only that we may keep true our ideas of what suffering is, and to do that, one must bear in mind the broad distinction I have just mentioned.

12698. You mean that if a man and an animal have each to be subjected to a serious operation under anæsthetics, the whole of the pain and suffering that the animal experiences after it is properly anæsthetised and dies under the anæsthetic, is covered by the anæsthetic?—Yes.

12699. And it has not suffering before or after?—No.

12700. But in the case of a man, he has the suffering of anticipation, thinking of the future, of those who belong to him, and if he dies he has the suffering of his family. If he survives, he has suffering from that again, which may amount also to disablement and loss of some of his functions?—Yes, that is a very striking example of the difference, and it brings out one other point which you must also bear in mind, and that is, our responsibility with regard to mankind as compared with our responsibility to animals. In the case of an animal, if the future is to be a future of pain we unhesitatingly kill it. No matter how certainly the future must be a future of pain in a man, we must by universal acceptance keep him alive. Therefore in deciding on our duty to men and animals we have to realise that no matter what be the consequences we have no right to decide with regard to man as to whether it is better to terminate his sufferings or not. If you take the case of a man with some terrible disease over him which may perhaps be cured by an operation, he has the terror of the uncertainty of the cure. The operation is fortunately under anæsthetics, and he does not feel it, but afterwards there may come an only partial cure, and a painful life ahead of it. Not one of those elements exists in the case of the animal. The animal, if the pain is going to be greater than the value of life, is put out of the way painlessly. So that we must remember that distinction, because it is not permissible in a serious inquiry like this to shut our eyes to any truth. There is no safe side on which one may lean in the way of exaggeration in the examination of a question like this, and although you are going to be loyal to the duty of lessening the pain of animals, you must not mistake the relative position of pain with them and pain with us.

12701. You admit that there is a responsibility on everyone who operates on animals to avoid unnecessary pain?—Oh, yes. In fact, I go further, and I think that one ought to admit as a fundamental principle that it is the duty of all men to work for diminishing pain. The diminution of the totality of pain in animals and in men ought, I think, to be a guiding motive and a prominent motive in the case of people in the position of those who are working in curative science, and also in the position of people in ordinary life; and in my opinion it is just as operative, and, if I might give my private opinion, it is far more operative in those who are connected with the curative sciences than in the people who attack them so bitterly. And it is for this reason: Because those who are dealing with the curative sciences are perpetually brought in face of pain, and it is a point of honour with them to conquer that pain and relieve it, till that aim becomes to them a kind of second nature—a kind of all-pervading motive.

12702. You are speaking rather, are you not, of the practising physician or surgeon than of one who devotes himself entirely to research?—I am; but the reason why I cite that is this, that there is an almost unanimous opinion among those who are engaged in the practical work of the curative sciences in favour of experimental research in connection with them. That support has been discounted by people saying: "Oh, those who are engaged in curative science get callous to pain." Now, in my opinion, they are more sensitive to pain, because it is perpetually appealing to them, and, if I might say so, it is the life-long foe which they are engaged in fighting. I mentioned therefore the extent to which the desire to diminish pain was present to those who are engaged in it, not so much as dealing, as you say, with those who are engaged in research, as with those who, with a full knowledge of the matter, give them their support in order to point out that that support has the very high sanction of coming from people who all their lives are fighting pain, and fighting it often at very great personal discomfort and self-sacrifice, till it has become to them, as I say, an almost all-dominant motive. The difference between the two schools is this: Those with whom I range myself, who are desirous of extending and supporting experimental research in the curative sciences, consider not only the pain that is inflicted, but they consider the pain that might be prevented, and they hold themselves responsible for permitting pain which they could stop, just as much as for inflicting pain deliberately—they look at the two together. The other school consider only inflicted pain. They think it their duty to prevent pain being inflicted, even though the infliction of pain may lead to the prevention of many times that much pain in the future.

12703. Of course, your argument, if carried out—I am not disputing it, or offering any opinion about it—would lead to this, that if anæsthetics had never been invented, you would still be justified in carrying out experiments on living animals, and inflicting the necessary pain upon them for the sake of the knowledge that would be acquired in the saving of pain in the future?—It would; but if you will allow me I will put my practical conclusions latest. I want first of all to lay down the foundation of the decision. I want to get before the Commission the fundamental principles as I view them, and to establish them, and then I propose to take the practical conclusions, when I will deal with such a matter as you have referred to.

12704. But, of course, the actual fact that anæsthetics and their use have been discovered now does not render the argument I was putting a very practical one. It is rather a logical consequence than a practical one?—Certainly; I quite understand that. Now I want the Commission to allow me to take what I may call a simile or an illustration. I am going to take a case which in my opinion exactly represents the attitude of the two rival schools; it is not connected with experiments on animals at all, and that is one of the reasons why I have chosen it. I want the Commission to understand it that I take as explaining my views, and I shall trust to bring the question which is before this Commission up to the level of this illustration. I will suppose that a ship which is plague-stricken, and has got rats on board, arrives at a port. A man with a sense of his responsibility, knowing that there is high probability that rats convey the plague, would unhesitatingly extirpate those rats, even though his only method of doing so was by putting them to a painful death, whether it was by poisoning them with phosphorus or by stifling them, or by even more painful methods. He would not hesitate; he would feel it his duty to extirpate them. Now, supposing that a person were to come and say, "I could not bear to see those poor creatures running for their lives and in danger, trying to save themselves from their relentless pursuers, and so I let as many as I could escape," I have no doubt that the person who did that would think that he acted from humane motives. But what would be the consequence? It might communicate plague not only to a town, but to a whole nation. It might bring positively measureless misery. The first man would be right, because he would look at the inflicted pain which would be to the bad side of the ledger. No one would have a right to inflict that suffering merely capriciously. But he would see beyond that pain he was inflicting—that in acting thus he was preventing an amount of pain which was beyond all measure greater than that which he was inflicting; and if he was a man to whom pain appealed, who had a heart which felt keenly suffering whether in men or animals, he would do it all the more unhesitatingly. The other man would

think only of the inflicted pain, and say, "I am too tender-hearted to inflict it." He would not consider that by not doing it he was causing preventible pain on such an enormous scale. That is typical of the struggle between the two parties. I have chosen that example, not because it is an exaggerated one, or because it is an unfair one. I have chosen it because in that case there is no veil between the act and its consequences. One can see plainly that the letting free of those infected rats might produce those consequences. But now just let me take a hypothesis. I will suppose that, instead of its bringing a plague, the killing of those rats would lead to the discovery of the antiseptic treatment. That antiseptic treatment put an end—substantially, of course, I mean—to an amount of human misery that we can scarcely realise. The suppuration of wounds, accidents leading to months of painful sickness and a recovery which was only partial, and left the people maimed, the horrors of war doubled, the deaths in war enormously increased were all consequences of sepsis — it was just as bad as a plague. Now, instead of its being a question of "either you extirpate the rats or you have the plague," it had been "either you extirpate the rats or the antiseptic method will not be discovered, and the suppuration of wounds and all the horrors which follow from that must remain in the world," you see at once that to the thoughtful man the argument in the two cases is precisely the same—"If I do not inflict this pain I permit an unmeasured amount of pain which I could prevent." In my opinion, when you once see clearly the causal connection between the pain you inflict and the diminution of pain which follows from it, it makes no difference in what way it follows. Your duty is to take that line which produces the minimum total pain, and whether the pain is inflicted pain or whether it is preventible pain which is not prevented is in my opinion one and the same thing.

12705. Supposing that you have not got an immediate prospect of preventing pain or of preventing septic conditions, but are pursuing these experiments for the purpose of acquiring knowledge that you do not possess, with the hope and belief that it will lead to some good, how do you deal with that?—You have anticipated the very next step which, in my opinion, we have to take in the examination of the subject. The reason why I gave this illustration was that I wanted to put before you by an example the criterion which I say every humane man ought to follow. It is that he ought to take that line which before his conscience he thinks will lead to the diminution of total pain, and if he is satisfied that the infliction of a certain amount of pain is the right way to diminish the total pain he is bound, from his very feelings of humanity, to take it. And now you say—and, if I might respectfully say so, quite rightly—if that is going to guide you in a matter of this kind, you must be satisfied that a man ought to realise that the practise of the experimental method is the wisest way to work for the diminution of the totality of pain. If I have brought the question to this point it is all I want to bring out of the example which I have taken. I admit that those who claim that they should inflict pain are bound to show that it is the wisest thing to do with the view to reduce the totality of pain. But I want to point out one other thing, and that is this—that a man would be bound to take the same course with regard to the rats if there was not a certainty, but a reasonable ground for thinking, that plague would follow. He would have no right to say, "It is not demonstrated; I will risk it."

12706. There is a case, of course, beyond that, where a man is making experiments upon a living animal with a view to ascertain precisely what the action of certain parts of the body is—what their functions are?—Yes.

12707. Not that he can point to any particular saving of pain or good that can be done by it, but he is doing it to acquire knowledge which he does not possess, with a philosophic belief that it will lead to the good of reducing pain?—It is a real pleasure to me to have such points put to me, for this reason—it convinces me, as indeed I knew before, that I have the honour of speaking before those who have thought very seriously over this matter, and realise the various points which arise. You will find that I shall take that point up in its proper place; because, in my opinion, it is of the greatest importance that we should face specific questions of that kind. You cannot, in this matter, come to a wise conclusion by simply dealing with vague generalities. But if you will allow me, I will take it up later, and you will find, I think, that I have put it in its logical position. Now, I said that I took that example of the plague-stricken ship, not because it was exaggerated, but because there was an obvious connection between the act and its consequences, and then I pointed out that if you could get an equally obvious connection between pain inflicted and a great discovery it would be the same thing. I said that the parallel I have taken was not unfair to either of the two parties, and I want to digress for a moment to show that I am justified in saying so. I think if you were to ask a man who had given thought to the matter, who, of all living men, had been privileged to do most to reduce pain, the answer would almost, without exception, be "Lord Lister," who introduced the antiseptic method, which has been followed now by the various aseptic and antiseptic methods which obtain all over the world. Yet I saw the other day in a paper that at an anti-vivisection meeting Lord Lister's name was mentioned, and it was greeted with shouts of "Brute." They called a "brute" the man who had done most of all living men to lessen pain. And that was, if I might say so, perfectly consistent. They knew that he had voluntarily inflicted pain in his experiments, and that was all that they looked to; and they did not realise that if their real motive, the thing which was the real spring of their action, was the desire to lessen pain, Lord Lister's name should have been the most highly honoured of all. Now, I am not speaking bitterly at all of these people, though I oppose their action so much; I only want to point out that they are acting according to the effect on their emotions of the contemplation of pain; not according to the pain itself. The prevented pain, which has made Lord Lister's name so esteemed, is not present to their minds. The inflicted pain strikes their imagination, and the consequence is that they, with the best of motives, denounce him because he is willing to inflict pain, without realising that he, who is the most capable, at any rate one of the most capable, of all men to know the ultimate result of his actions, has been doing it for the purposes of reducing suffering, and has succeeded beyond all hope. So that I am not treating those who oppose me unfairly in pointing out that they do not regard the more distant consequences of their action—they only regard the question of the immediately inflicted pain.

Now, I want to go on to what the Chairman has put to me, viz., that I must establish that experimental research is the wise mode of action if we wish to diminish pain. I start from the following fundamental facts. I say that if you look round the world, the only way in which we can diminish pain is by human action. Animals go on regardless of the pain they cause to one another. In fact, the universe is built on pain. Whole races of animals live simply by killing other races and killing them without the slightest regard to suffering. So that we must look to human action alone to diminish pain. Now, what is it that prevents our diminishing pain in the world? One feels at once that it is not the absence of desire to do it. I believe that desire is very prominently present. It is because we do not know how to do it; it is our ignorance. Take the case of doctors. Doctors are as desirous as they can possibly be of lessening suffering in the world, and yet how often they stand by the bedside and the patient goes on suffering and they can do nothing. Now, the absence of our power of diminishing pain is due to ignorance, to our not understanding two things. First, not understanding the causes which excite pain; and, secondly, not understanding the action of those causes on the organism—that is to say, how it is that those morbid causes, if I might use the phrase, produce pain? I call the first one (simply to distinguish it) the pathological factor—that is to say, what are the springs of disease; and the other is the physiological factor—that is to say, how those springs of disease cause pain in the organism; that is just a division for convenience of the knowledge which we must possess in order to be able to lessen and control pain. Let me give an example of the way in which lack of knowledge increases suffering. The discovery that the organism could, by means of anæsthetics, pass into unconsciousness without passing into death, and that all the living operations could go on equally vigorously while consciousness—that is to say, the possibility of pain—was entirely suppressed, led to the use of anæsthetics, and, as a consequence, to the diminution of suffering, both directly during operations, and indirectly in increasing the possibilities of surgery. Without the knowledge obtained from that discovery men were obliged to perform operations in the brutal method of the old surgeons, because there was no other alternative. This is only an example of the way in

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which our power is limited by our knowledge. And I wish to point out with regard to this point, that here again we have no difference of opinion. Not only the whole medical profession, but the whole public agree that doctors ought to be armed with all the knowledge of the time. There is no difference of view as to this. Every attempt is made to increase the efficiency of the education of the doctor; because we feel that by arming the doctor with the knowledge of the time we arm him as best we can with the means of diminishing pain. That, of course, is true with regard to the knowledge that has been attained during the last, let me take it, 40 years—the only years as to which I can speak; and that knowledge has been mainly due to experimental research. Therefore all must feel that this knowledge which has been obtained by experimental research is useful for the purpose of lessening pain, because we try our very best to make all those who are engaged in practical curative work masters of it. I do not think I need point out to this Commission further how all-important knowledge is where you are dealing with organisms, and you want to stop suffering.

There is one other thing I want to say, and that is this. That our inability to stop suffering, owing to absence of knowledge, is not only to be found in the case of obscure diseases. They afford striking instances of it, no doubt, and are by no means rare. I should think there are very few of us who have not seen those near to them suffer from diseases which have baffled the doctors, and whether or not they could be eased or saved was decided by whether or not the doctors could read what was the cause of the trouble. A friend of mine has just died, apparently of a painful disease, which the doctors could not touch. A *post-mortem* showed that it was only because they did not realise what the true cause of the disease was; it was one which might almost with certainty have been relieved. Such diseases may have marked symptoms, but the doctors are unable to read from those symptoms what is the cause, and therefore they cannot fight the cause. But, although these cases are striking instances of the principle, they are nothing like the most important; the most important are diseases which are quite common, of which we cannot yet find the cause, and not finding the cause all we can do is to deal with the symptoms.

12708. I think we all agree that there is a very vast field of medical knowledge yet; I think every medical man would agree to that, as well as ourselves. But really, the question is how far that knowledge can only be found in experiments upon animals?—Precisely. That, I think you will find from the *précis*, is the next point I want to mention. But I wished to put in the forefront that it is want of knowledge which prevents our diminishing suffering, because I shall trust to show to this Commission that the pole star by which we steer is knowledge. It is ignorance that hampers us. I am using the term ignorance in a sense that will not, I trust, be misunderstood. I am aware of the gigantic amount of knowledge that the present and past members of the medical profession, and those who have been engaged in research, have already accumulated, but what I mean by ignorance is the vast field of what yet may be known, but is not at present known. I desire to impress upon the Commission that that is the real cause of our impotence. Now, let me take the question of how we are to get rid of this ignorance—and here we are on firm ground. No man who knows anything of science has any doubt whatever that the right way to advance knowledge is by experiment. You can take the whole range of the sciences, and I would challenge an opponent to name one in which advance, if it has been rapid and striking, has not been through experiment. Where we are reduced to observation a science crawls. Where and in proportion as you can use experiment, the science advances rapidly. I could take any science to show that; it is much easier to show that experiment has led to advance than to find a case in which advance has been made purely by observation; because a scientific man rushes at once to experiment if he can do so. He adds, of course, the testimony of observation—that is extremely valuable for the purpose of guiding and starting his experiment; but everybody knows that each observation becomes only the nucleus, or, rather, the basis, from which you start your experiment. I was puzzling to think of some branch of science where there was only observation, and the only thing I could think of was the case of volcanoes. Now, there is not anything that has attracted human attention more, or fascinated it more, or has been on a bigger scale or more open to observation, than volcanoes; and yet we are almost in the same state of ignorance of the origin and the *modus operandi* of volcanoes as Pliny was.

12709. The difficulty is just as great now as then for anybody to go down a volcano when it is in eruption?—Precisely. In no way can you bring experiment to bear upon it. As soon as you can bring experiment to bear upon a subject you are free; but as long as you can merely observe, your progress is very slow. The reason is that experiment is like cross-examination. You can put the question you want, and Nature always answers it. She does not answer the question you meant to put; she answers the question you actually did put. She swears by the card in the most shocking manner. She does not care in the least what you meant to ask, but she does care what you asked, and she answers it with perfect truthfulness. And the consequence is, that when you adopt experiment, the great experimenter can put a question, the answer to which lets the whole secret out. I am going to give an example that I remember impressed me very deeply. I cannot tell how many years ago, but it was just about the time when it was first discovered that guinea-pigs were susceptible to tuberculosis. I heard from a friend that a doctor of his acquaintance, a practising physician, I suppose, had been troubled as to whether consumption was a communicable disease or not: I should say that the vast preponderance of opinion at that date was that it was hereditary. I remember, as all of us remember when we were young, the way in which consumption would, as it was called, go through a family, and it was thought that this was because it was in the strain. Then some bold people suggested that it was possible that it might be due to its being communicable, although probably to the ideas of that time it looked about as likely to be communicable as a spinal disease. What that doctor did was this: He took a dozen guinea-pigs, divided them into two sixes, and he let one six have the run of the wards of a consumption hospital, and the other six have the run of the wards of another hospital, just about in the same conditions of temperature; he left them there for the requisite number of weeks, whatever it was, and then he killed the whole dozen, and he found that the six which had been running in the consumption hospital had tuberculosis, and that the six that had been running in the other hospital had no trace of it.

12710. (*Chairman.*) The other was not a consumption hospital?—No, it was not.

12711. (*Sir William Collins.*) Whose experiments were those?—I cannot tell you the name of the doctor, because it was not given me, but I heard it long ago, in the early days of research upon tuberculosis, and I believe it to be true. But if it was not actually performed, it would still serve as a typical example of experiment. I have no reason to doubt that this experiment was actually performed, because the man who told me was a most intelligent man, and he told it to me at the time as having been done quite privately, by a doctor whom he knew, in order that the doctor might settle the doubt in his own mind.

12712. (*Chairman.*) You would say that it is rather an illustration than a typical example if it is not given us at first hand in some way?—You may take it simply as an illustration, although the very thing has been done, I know, by other people. Klein did it. If you remember, Klein put guinea-pigs into an air shaft which led from the wards of a consumption hospital, and found that they developed tuberculosis. But we will take it merely as an illustration. I am not basing anything on the fact. What I want to point out is this, that such an experiment would settle the question more than would twenty years of observation—I might say a century of observation—for this reason, that you had placed these similar animals under entirely similar circumstances, with the exception that in the one case, but not in the other, there were present in the immediate neighbourhood persons who were sick of the disease, and you will find as a result that in the one case they become sick, and in the other they do not. There is no question of heredity, and there is no question of the acquired disease being the consequence of different circumstances of living, of different circumstances of nutrition, or anything of that kind. The only thing that is present as a cause is the neighbourhood of the sick people in one case and not in the other. In the result the difference is disease in the one and not in the other.

12713. What was the fact that it was wanted to

discover there—whether guinea-pigs could take the complaint?—No, whether the disease was communicable from one person to another.

12714. But there are plenty of examples of people who have not tuberculosis daily frequenting hospitals and not acquiring it, are there not?—Oh, yes; but the point of the experiment was not under what circumstances was the disease communicable, but whether it was communicable at all. Just take the case of persons who are ill of spinal disease.

12715. I follow you?—Their relatives go freely among them, attend to them, and try to make their sickness as easily borne as possible. And so it undoubtedly used to be when members of a family were sick of tuberculosis. Everybody thought it was only kindness that the brothers and sisters should attend to their sick sister, and nobody thought that there was danger. But when you had found that this was a communicable disease, your duty with regard to children and other people in the presence of the disease became very different, and it was only to find out this that the experiment was tried. The experiment showed affirmatively it was communicable. That was only, of course, the first of a necessary series of experiments to show how it was communicable; but from that moment that doctor knew how great was the responsibility of allowing people to attend or be in intimate contact with one who was affected by consumption. Just try and see how you could get an equal degree of certainty from observation. You take a case where it has run through a family. Yes; but it might be that it was hereditary in the family. Then you find that in a particular place it is very largely prevalent. Yes; but that may be because, as compared with other places, the climate or soil of that place produces a predisposition to it. Neither group of cases points necessarily to communicability. Some people will be struck in one way with a series of these cases, others will be struck in another way, and there will be a perpetual conflict as to which is the true explanation. But when you take such an experiment, there is not a doubt; you cannot explain it, except that the disease is communicable. And the consequence is that you commence your further examination with the consciousness that it has been demonstrated that it can pass from the sick to the well. Now, I was very much struck—as illustrating the impossibility of arriving at these things by observation—by a piece of cross-examination by Sir Willian Collins of one of the witnesses before the Commission; it illustrated it in the most striking way. He was dealing with the result of the serum treatment of diphtheria. An appeal was made to the statistics of diphtheria hospitals in London, and the witness said: "Oh, the serum treatment is demonstrated as being a great success, because you have here the same hospital in the same place dealing for a consecutive number of years with the same population, and the statistics show a great diminution of the percentage of deaths after the introduction of the serum treatment." Sir William Collins, if I might say so, quite justifiably cross-examined on that. He will know whether I am fairly representing him, but, as I remember it, he asked are we now getting the cases earlier; is it not true that we have made such advances in the diagnosis that we know better what is diphtheria and what is not? And he went on pointing to differences in the circumstances of the earlier years and the later years as taking away from the conclusiveness of the statistics.

12716. (*Sir William Collins.*) As an element that required consideration?—Exactly, as an element that required consideration. That is perfectly fair. Statistics that are got in that way must be rightly open to that criticism. And this is the reason: You dare not let the children wait in the later years so as to make the time at which they are taken up for the purpose of treatment comparable with what it was in the earlier years, because you are bound to do for every child the best thing that you can, and the consequence is that you dare not make your experiment scientifically more perfect, because there is the paramount duty towards human life. Now, you could not have had a stronger case, *i.e.*, one in which the observational method was working under greater advantages. You had hospitals devoted to this disease alone, dealing, as I have said, with the same population, in the same place, under, as far as possible, the same circumstances; but you could not, and you dare not, and you ought not to, make the circumstances identical because of your duty to the individual. Compare that with the case I was putting forward of the guinea-pigs; I could give another and more striking example, which is the classical experiment in connection with anthrax, in which, after Pasteur had worked out the treatment by inoculation against anthrax, he took 50 sheep for his experiment; 25 of them were inoculated and 25 were left uninoculated. Then the whole of the 50 were inoculated with virulent anthrax, and within four or five days every one of the 25 that had not been inoculated died, while of the others 24 were alive, and one was shown to have died from things unconnected with anthrax. You see by these examples how in experimental research you can make the circumstances identical, and one experiment of that kind (properly repeated, so as to eliminate error) gives you a certainty that years of observation could not give. And all those years of observation have paid their toll in suffering. It is no use saying, Well, we shall learn in a few years hence, instead of learning it now. All that time there has been preventible pain, which has not been prevented because you have not known how.

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What, therefore, I want to point out is that we know that the way to acquire knowledge with certainty is by experiment; it follows from its very nature, and from our universal experience in all sciences, that this is the way in which you best acquire knowledge, because you can make each experiment answer some critical question. You can arrange matters so that you have two cases precisely similar in all respects, save that the one is without that cause present and the other is with that cause present, and the contrast gives you a light which you could not get from observing instances where you cannot eliminate other causes. The more complex the subject is, the more factors there are at work, the more essential the experimental method is; and the most complex of all phenomena are those that relate to living beings. I am sure there are those here who have made experiments in connection with life, and know from experience the truth of what I say. The number of factors at work is so large, the difficulty of isolating them is so great, that it often takes very much longer to realise how you can put the question to Nature than it does to put it, and to draw your conclusions from it when you get the answer. But the more complex the subject is, I say the more are your driven away from observation, which only gives you the total result of many causes which are varyingly present in all cases, to something which will enable you to isolate several of the causes and determine separately their effects.

12717. (*Chairman.*) That brings you, I think, to the question which I was putting to you about pure experimentation, and the justification of experiments on animals where you have no specific disease in view?—Yes. Many will think me a heretic on this point, but I have a strong view that, when you have once realised that experiment is the right way to acquire the necessary knowledge, it is a bad thing to work too directly and too consciously for practical results.

12718. When you call that a heresy, of whom are you speaking as the orthodox?—Most people say that you should only make such experiments with the direct view of some practical application for the benefit of mankind.

12719. We have had a good many witnesses before us who take what you call the heretical view?—I am delighted to find that I am not alone.

12720. As well as those who take the other view; and others who would say that you do not go far enough?—I am glad to find that I have got my comrades. You must not misunderstand me. I think that the object and the reward and the ultimate justification of research is the benefit it confers. But what I wish to urge is that it is unwholesome to keep that before your mind when you are working, at all events, in the earlier stages. May I give a parallel which I think explains and justifies this view? I take the history of alchemy. For centuries men of the highest intellectual attainments—for there can be no doubt of the ability of the men who worked at these subjects—worked hard at the transmutation of metals, and the result of those centuries has only been snippets of knowledge, if I might use such a phrase. I do not undervalue them; they gave us, I believe, some very valuable reagents, but that is practically all. And the reason was that they did not commence by working so as to get to know the nature and properties of the things on which they were working; they worked straight for a result.

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12721. For an impossible result, was it not?—It may have been an impossible result. But whether it was impossible or possible would have made very little difference if they had commenced by laying the foundation in knowledge, because, although we should not have obtained the transmutation of metals, we should have had modern chemistry some three centuries earlier than we did. But they only worked for a result.

12722. They did not care about chemistry. Their researches were not chemical researches?—Well, transmutation of metals was intended to be a chemical triumph. If I might take a parallel in curative science; this work was like the shots at specific cures for cancer—violet leaves and all that sort of thing—made by people working straight for a nostrum that will cure a disease like that, the nature of which is shrouded in such mystery.

12723. I only meant those people were fighting a particular disease which they had before them, and which was evident under their eyes, and they were seeking remedies blindly?—Yes.

12724. The other people were seeking something in the clouds altogether; they were not pursuing what you would call chemical research, although they used certain chemical experiments?—But if they had only laid a basis; if they had only worked at it as you try to reduce a fortress, by parallels, where each parallel brings you nearer to the fortress. If you attempt to take it by a rush and fail, you are no further than you were before; but if you make your parallels you are so much nearer, and that is a permanent advance. Just in the same way I think that if I were to ask any scientific man of position in the medical profession, "Where do you look for the solution of the great problem of cancer?" he would say, "I look to a gradual acquisition of knowledge about it, and the breaking up of the obstinate silence of the disease which will tell us nothing, neither its preferences nor the things it dislikes, nor its origin, nor anything." Such men do not give much heed to the investigations aimed directly at its cure, because they do not believe they will succeed. They believe that we shall have to know a great deal more about the disease itself before we know in what direction success can come. But what they do think is this, that by gradually acquiring knowledge of it, knowledge acquired quite independently of whether it is a thing we could use or not, the disease will gradually cease to be a mystery, and when it has ceased to be a mystery we shall find some weak point in its armour. Therefore I am satisfied that the wisest thing to do is to realise broadly that our ignorance of the springs of disease and the nature of the organism that suffers, are the cause of our powerlessness, and that all research which is intended to lessen our ignorance is in the right direction, and that it is in vain for us beforehand to say which will soonest lead to a beneficial result. I feel, therefore, strongly that this is the principle which, with proper regulations and restrictions, we ought to accept; and that we ought to fix our eyes on knowledge and not on immediate promise—immediate promise is likely to be delusive—and when I come to the instances by which I trust to justify the serviceability of experimental research, I think I shall be able to show that those things have sometimes reaped the richest harvest in which the promise came quite late.

12725. You think, to sum up that part of your evidence, that the experimental method is justifiable even though you have no immediate practical object in view beyond the acquiring of knowledge and the hope and belief that it will lead to some ultimate useful purpose?—Yes, you have the hope and belief that it will, and you have the certainty that it is the right way to work for it. Where you are ignorant of the evil and ignorant of how it operates, you are like a hunter who is not yet on the trail of the fox. He has to cast about in order to come upon it. Similarly, it takes a very great deal of experiment to know in which direction practical success may be looked for.

I want to say one more thing before I go to the examination of whether, as a matter of fact, the experimental method has shown itself to be of this great practical value, has thus justified its use. So strongly do I hold that what we ought to fix our eyes upon is the acquisition of knowledge and the removal of ignorance, that if I was here without being able to show a single practical result of experimental research in curative science I should give evidence in the same direction, and so far as I personally am concerned with the same confidence, for this reason: Supposing I was to go back, say, 40 years (I am not suggesting for a moment that prior to 40 years ago there was not a great deal done in research, but I take that period as including the great outburst of experimental research, beginning with the early sixties and since then) I should have all my data. I should have the fact that ignorance was vast, vaster even than now, that ignorance paralysed our power of preventing suffering, and that the right way to get rid of that ignorance was by the experimental method; and therefore, even though I could not show that we had obtained any practical results, I should say to this Commission, if it were sitting in the early sixties, that it was their duty to take care that experimental research was not hindered. The idea that you do not justify experimental research unless you can show that it has already produced practical results would make it never justifiable to commence it, and, in my eyes, it would be just about as ridiculous as to revile winter sowing because in April there were no crops. The moment when the practical results come depends upon the nature of the subject. You cannot foresee it; they may come at a burst. After long investigation which apparently leads to nothing you may suddenly find your reward.

12726. It might not have been unreasonable in Adam, but since his day there has been a very long experience of winter-sown crops?—There has been a long experience of winter crops, but what I mean is, that a man has got no business to say that because the crop has not yet come last winter's sowing has failed.

12727. It would have been an unreasonable thing in Adam?—I agree with you; but, if I may say so, I should use that in my favour; because I should say that if you look at any science you will find that theoretical investigation has always led ultimately to practical power. I could give examples in industries, but they will occur to most of those present. Investigations which have been purely scientific have almost always led to practical results, because the knowledge has been able to be used, and it has produced great results. So that, just as since Adam, we have had great experience in winter sowing, so I should say that since the days of Bacon or Newton we have had great experience in this—that you never can have an increase of knowledge in any subject bearing on practical life without its leading to increased practical power.

I want now to deal with the justification of the use of the experimental method by what it has actually performed, and I am going to ask you to allow me to confine myself to the work of the last 40 years, because that is the period which has fallen under my direct notice. I shall group it in the way I group it in my own mind in order to make out this justification. As I have said, to my mind this direct appeal to practical results is not necessary, though it is immensely helpful, to enable one to make up one's mind as to what is the right course to pursue; but it is all important in dealing with the many well-meaning persons who do not go along with me in their views with regard to it. The difference is not, I think, because there is a difference of fundamental principle—they are as loyal, probably, according to their lights, to the doctrine that we ought to do our best to lessen suffering as I am; but they have to learn that which scientific training easily teaches those who have had much to do with it, that the right way to do so is to make full use of the experimental method. I wish, therefore, to take the results of experimental work in the last 40 years, and point out how it has removed ignorance—that is to say, how it has given us knowledge and how that knowledge has become of practical value in reducing suffering.

12728. If you please?—Of course the first example which one must take is that of sepsis and the antiseptic method. If you look at the books of 40 years ago you find that the idea that sepsis in the case of open wounds was due to organisms derived from outside, was rarely held; I doubt whether it was held by any, it was certainly held by very few, and it was not put into practice in any way. And the work of those who demonstrated that sepsis was in all cases due to organisms, and where access was possible, it was almost always due to organisms coming from outside the animal attacked, gave rise to the antiseptic treatment, the value of which it is quite unnecessary

to dwell upon. It was purely the result of experimental research, and it is a triumph the practical value of which puts it at the head of all the changes in the last 40 years. But I am not going to deal with that any further than just to refer to it, because I am sure that its value and its origin are present to the mind of everyone here.

I wish chiefly to deal with the case of microbic diseases, which for reasons that will appear, I shall divide into infectious diseases and diseases communicable otherwise than by what is ordinarily called infection. If you go back 40 years you will find that these diseases were complete mysteries; they were, alas! recognised as facts, because they could not be otherwise; they were ever present with us. They were known as things which ran through a certain course, their symptoms were studied, and the doctors of that time did their best to alleviate them, no doubt, by nursing and treatment. A great number, no doubt, tried to do so also by drugs, which, I think, most authorities to-day would say were practically of little use. But the diseases themselves were, so far as their causation is concerned, perfect mysteries. Now experiment has changed that from top to bottom. The bulk of these diseases are known now to be the result of an invasion of the system by specific organisms which multiply in the system, and the disease is due to the presence and action of these foreign organisms. In fact, I do not think that I should be giving an incorrect idea of the result of the work of these 40 years if I was to say that it has revealed the fact that we live not as isolated living organisms, but that we are surrounded by and penetrated by infinitely small living things of very varied descriptions—that we are, as it were, bathed within and without in a sea of microbic life; I am using the word microbic in its broadest sense. Within us we have them permanently and probably normally; in fact, if you include the organisms which live in the blood, certainly normally. We are surrounded by them, and instead of diseases of this kind having to be looked at as due to defects or failure in normal action in the organism itself, we have grown to realise that they are the result of attacks upon the organisms by external foes.

12729. Do you regard all this state of things which you have been describing as wholly due to experiments on animals?—Absolutely. It is absolutely due to them.

12730. We have had a good deal of evidence from other witnesses on the subject, and we need not trouble you to describe the instances?—Quite so. It is difficult for me to say which is the earliest case in which this was established; but I will take one of them, because I wish to show how baseless is the idea that the work which has been done in this respect is work which has only led to doubtful or hypothetical results. I wish to show the actual and certain advance of knowledge. Then I wish to show next the way in which that is due to experimental methods. Then I wish to show the practical methods to which it has led. I will take a single example—any one will do—but I will take the example of anthrax. When Pasteur took up anthrax, it was only known as a terrible disease among cattle, leading to a vast proportion of deaths. He ascertained that it was due to an invasion of a particular microbe, which is known as the microbe of anthrax, which multiplied with infinite rapidity, and which ultimately led to the death of the animal. He then found out a method of inoculation (I am not going to deal with the details of his method for a moment) against it, and in the classical experiment to which I have referred, he exposed to anthrax the 25 sheep which were inoculated, and the 25 which were not inoculated. He might have done it by putting them in company with diseased animals. As a matter of fact, he took the severer test of deliberately inoculating them with the disease. The result was that those which were treated with his process did not die, and the others did. Now, that is a scientific result as certain as the action of nitric acid would be on iron; I mean that it was a definite experiment, capable of being repeated, and showing conclusively that there was a way of defeating this disease, a way which might, or might not, be availed of by mankind, according as the importance of warding off the disease was great or was not. There was nothing hypothetical in it; it was a fact which was just as much demonstrated as any fact that has been demonstrated in chemistry or in any other science. I have there taken a case which relates solely to animals. I have not approached the question of the applicability of the process to mankind, because, fortunately, anthrax is very rare among mankind. But if you confine yourself to the consideration of it as a disease of animals, you see the advance that has been made. First of all, there is the knowledge of the cause—that is to say, that it is due to the presence of microbes. Then (as I shall presently point out) the fact that it was due to the presence of microbes led to the discovery of the method of combating it; and that led to the practical application of that method, and, if you choose to avail yourself of that method, the disease is conquered. The whole is based upon the discovery of the origin of the disease. It was the knowledge that it was due to microbes which led to the whole of that discovery. Persons might say: "What good does it do to mankind to know that this which they call a disease is really a consequence of a particular type of small life which has got into the organism and multiplies there? It does not alter the nature of the disease; people die of it all the same." And you would think that the pure theoretical knowledge that it was due to this particular cause left the practical problem where it was. May I point out how that piece of knowledge did not leave the problem where it was, but put it in a totally different position, and necessarily led to its solution? You can see it in this way. It instantly put the phenomenon of recovery in a new position. When an animal was healthy, and there were only a few microbes put in it, when you would say it was best calculated to repel invasion, they multiplied furiously. At the moment when it was weakest, when the microbes were strongest, most numerous, and the animal was most exhausted (if it was a case of recovery), the tide turned, and the enfeebled organism was able to fight the strengthened assailants successfully. That necessarily led to the conclusion that there must have been a change in the organism during that period, which entirely altered the relative strength of the two forces; and that was the beginning of the realisation that in these diseases there comes some protective change in the organism that is attacked which brings recovery at the time when you would least expect it. This led to the inquiry: What is that change? It must be something that survives the disease, because we know that when a man is recovering from an infectious disease, he can give it to other people. Yet, although he is more exposed to infection from himself than any other person could possibly be, he is not infected. Therefore it must be something that survives the disease. Then the question came: If it survives the disease, is it possible to produce it under circumstances so that it may be present in the organism independently of the disease? In fact, men came to realise that recovery in a case of microbic disease means the "turn of the tide" in a process where, on the one hand, the invading microbes are multiplying, and on the other hand these protective forces are multiplying, and recovery or death means whether the turn of the tide comes too late or not. The moment you see that that is so, the whole question becomes—Can you antedate this turn of the tide? Can you contrive that this protective condition shall be produced earlier, before the mischief is done? It is no use putting a fire out when all that is valuable in the house is burnt. From these ideas there sprung almost immediately the two main methods of treatment. The one seeks to produce this protective state during the period of the disease, and it is a treatment of this type which succeeded in rabies; the other seeks to produce the protective state before the disease comes, and is the method that was illustrated in anthrax. Finally, they led to the third line of discovery. Is it possible to get the protective substance from somewhere else, and introduce it during the disease, in order to antedate the time when the system will become strong enough to repel the attacks of these foreign invaders? Now, if you think of all those three lines of discovery which have respectively led up to the treatment of rabies, to the prophylactic methods which are used in the case of anthrax, pneumonia, and other diseases, and to the serum treatment of diphtheria, you will find that they all came from the knowledge that the cause of the disease was a foreign organism, capable of multiplication, and that Nature defeated it by a change in the organism which made it more capable of resisting. These practical applications were the direct outcome of those two facts, which were pure pieces of scientific knowledge. So long as the disease was considered merely as a disease which ran its course, which was due to an unknown cause, whose consequences were only known by its

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symptoms, no one of those things could possibly be discovered. Their discovery was solely due to the experimental examination of natural processes. I was much amused by some evidence given by one of the lady witnesses who have given evidence before you. She seemed to be very much disgusted with the consequences of all this knowledge, and, if I might paraphrase what she said, it was "Nature has made such nice drugs and put them in the plants all around us. Why should we not use these, and not go to such messy things as serum?" Those are not her words, but they express the effect of them. Nature has put all kinds of useful drugs in plants, but she never dreams of using them for curative purposes. Her method of curing is entirely by means of the messy things with which the researches of the last 40 years have led us to work. We are now, thanks to the theoretical knowledge that we have acquired of the cause of these diseases, working to help Nature along the very lines which she has from the foundation of the world herself taken in fighting these diseases; and therefore, in my opinion, if we are to be guided in our action by our respect for what Nature has so kindly done for us, our course would be to pay very little attention to drugs and to pay very much attention to those lines of treatment which are based on the study of the two closely analogous phenomena of recovery and immunity.

12731. Assisting our natural friends in fighting our natural enemies inside us?—Yes, fighting our natural enemies by those means which, in the cases in which we do beat them, are the weapons which we use.

12732. (*Chairman.*) I think you were going to deal with the question of the influence of experiment on communicable disease?—I have been pointing out that the whole modern conception of the nature of these diseases and the mode of their cure arise from discoveries which have been arrived at purely by experimental methods, including necessarily experiments on living animals, namely, the discovery that they were due to foreign organisms that entered and multiplied in the system, and the discovery that the changes in the system which unquestionably produce recovery are closely allied to those which also produce immunity after recovery. So far I have only spoken of our attempts to effect a cure by introducing those protective substances which Nature, when there is need, produces in some strange manner. But it looks at this present moment as if we were going to combat some microbic diseases still more successfully by stimulating other agencies by which she effects her cure. I do not know whether Sir Almroth Wright is to give evidence before this Commission, but if so he will point out how he has devised a method of stimulating the action whereby the white blood corpuscles actually destroy physically the organisms themselves, so that we are gradually utilising both of Nature's methods of cure (and utilising them exactly as Nature does) for the purpose of antedating what I have called the turn of the tide, that is, strengthening the resistance of the organism that is attacked more than Nature would do merely by her methods of reaction. I now want to pass from these to another class of diseases in which equally we have been studying during the last forty years the cause of the disease and its action on the organism, but where research has led to a totally different method of fighting them—I refer to the communicable diseases which cannot be called infectious. Take the case of malaria, the tsetse in animals, and Malta fever.

12733. And rabies?—Rabies belong to the first set, because we fight it by generating in the body a protective substance. The way in which we fight it is, we have got a method of giving an attack of rabies which is very quick but very slight, and the consequence is that it produces very swiftly a small amount of protective substances. We then give another one which is also quick, but is stronger, but which can be borne by reason of the presence of the protective substance, and continue doing so while the main body of the attack is moving up. We have thus gradually accumulated a body of protective material, which, when the main attack comes, which would have been inevitably fatal, enables the system to resist it.

12734. I merely thought that the gnat was serving the same purpose and function that the mad dog was; therefore I thought it was really in the same class of disease?—May I apologise. You are quite right. It is not infectious but is communicable in a different way, and ought to be in the second class. The reason I already mentioned it with the first class was because we there use the method of cure of which I have spoken—that is to say, we attempt to produce a protective substance. It was I that was wrong. I want now to pass to cases where we cannot imitate the method of cure, but because we know the cause of the disease and its method of communication we are able to intercept it. Take the case of malaria. I do not know that our methods of curing malaria have advanced substantially, but we now know the cause of malaria, and we are destroying the possibility of communication. The work that has been done at Ismailia is an example of this. They have got rid almost entirely of the mosquito, which communicates the disease by its bite, and have thus almost got rid of the disease.

12735. Would you say that that was discovered by experiments on animals?—Certainly. They had first to discover in what way this disease was communicated. As soon as they had discovered in what way it was communicated, the destruction of the communicating animal presented itself at once as a method of preventing the disease. A similar case is that of the tsetse, which is absolutely fatal to cows in districts where the tsetse fly is found. They find that it inoculates an organism which produces death. I do not think they have made any advances in fighting the disease when the disease is once communicated; but knowing that it is communicated in that way, our attention is at once turned to discovering how one can fight the tsetse fly, or how one can prevent the tsetse fly itself getting the organism into it which it passes into the animal that it bites. In certain places in England, where there used to be ague, ague is now stopped. It is quite possible that that ague was communicated by the bite of an insect that derived the organism which is the cause of ague from biting some affected animal. We have drained the country; the insect probably exists, but in far smaller numbers, and there are fewer people there, if any, who are affected with ague from whom it can get the infection. The consequence is that the insect is harmless, because it cannot communicate the poison, or rather the poisoning organism, because it cannot get it. And, similarly, we can, when we know the cause of the disease, set to work to prevent the occurrence of those circumstances which enable it to be communicated. This is now being done with yellow fever and many other diseases. It may be that the disease is so bad that the only method we have of fighting its spread is to kill the animal. Take the case of glanders. Now that we thoroughly understand what glanders are, when we thoroughly understand the method of testing whether an animal is suffering from glanders, the animal may be killed at once, and that stops the propagation of the disease. Without that knowledge we could not ascertain so soon what animals we ought to kill, and they would be left as centres of contagion. So that it may be equally effective from the point of practical utility whether we find out the means of communication and stop it, or whether we attack the disease when it is communicated. In both cases it is the knowledge of the cause of the disease and the knowledge of the way that that cause comes to operate on the organism which enable us to devise these practical methods. And all that knowledge has been derived entirely from research work, conducted for the purpose of acquiring knowledge, and from the knowledge has come the power. I do not suppose the investigation of the cause of these diseases was carried on upon the lines of any direct idea of a protective method, but the object was to find out the real nature and operation of that against which protection was necessary. When this is done, the step to practical utility always comes sooner or later.

I have been dealing thus far with the cases of disease. I will now turn to another matter which I see has formed the subject of a great deal of evidence, and that is the study of the organism on which these diseases operate. A vast amount of experimental work of the last forty years has been directed to discover the physiological actions which are going on in man. Mere anatomy, mere structure, will not tell you physiological action; the knowledge of physiological actions must be obtained by experiment from a living creature, because as a rule as soon as death comes they cease. The importance of this work is just as great as is the importance of the investigation of the causes of disease. Take, as an example, that which I see has been the subject of evidence before this Commission—the localisation of function in the brain—the discovery from the sypmtoms of paralysis, or epilepsy, of the place where the interference with the

nervous system is taking place. It would be quite impossible to ascertain the laws which govern this excepting by experiments on a working animal—that is to say, a living animal. In many cases it need not be a sentient animal; the experiments may be done when consciousness has ceased; but it is absolutely imposible to ascertain them unless you make experiments on an animal that is still living. But there are similarly important experiments which go to blood pressure, to digestion, to the function of the great organs, such as the liver, or the pancreas, or the kidney, or the suprarenal capsules, in which it may be necessary to trace out consequences which do not occur immediately. In that case you must perform them on animals which are not only living at the time of the experiment but which are allowed to live afterwards. The examples which have been given you, I submit, show how all-important such investigations are for several reasons. One of the reasons is that without the knowledge so acquired you cannot read causes from symptoms. I should define the great change between curative science, as it exists now, and curative science as it used to exist many years ago, as being that now we try to fight causes, and then they only tried, or mainly tried, to fight symptoms. Fighting symptoms may be an extremely dangerous thing. A man who, not knowing anything about an engine and finding an overdriven boiler blow off, should fight the symptoms by screwing down the safety valve, would come to serious trouble. And in the same way, if in the case of a symptom—say the symptom of the rise of temperature going up—you say, the disease makes the temperature go up; very well, then, we will help the organism by putting it down. You may get into serious trouble supposing it should turn out that the rise of temperature, by lessening the resistance to the circulation, eases the heart and makes its work less onerous; it may be that the fast-beating heart has been able to do its work without injury only because the temperature has risen and its work is lighter. If, now, you put the temperature down without removing the cause of fast-beating, it may be that you overstrain the heart—do mischief just as the man who screws down the safety valve. I am not saying that this is so. That is not a matter on which I can speak. All I want to point out is that it is only by intimate knowledge of the organism that we know how far it is safe to fight symptoms. But, more than that, it is only by such intimate knowledge that we are able to diagnose causes. You get a headache; it is a mere symptom which may come from many different causes. Unless we have perfect knowledge of the organism we may imagine that a thing which has been successful for one headache is useful for another, whereas it may positively aggravate it instead of doing it good. The aim of all this investigation into physiological action, which is so varied in its character, and relates to every different part of the organism, is to put the medical man of to-day in the position of a man who knows the construction and operation of the engine he is working, who knows what is the meaning of every symptom of irregular action; and the increase of his power of alleviating suffering and staving off death is enormously increased by this increase of his knowledge and power of diagnosing which has arisen from these experimental researches. As I have said, these experiments must be experiments carried on upon living animals, because their object is to ascertain what is the mode of working of each particular organ, and what is the effect of any particular interference with that organ. Sometimes these experiments have resulted in curative methods. You could not have a better example than that which was put before the Commission, namely, the relation of the thyroid gland to myxœdema, and cretinism, where, I think, to the surprise of everybody, it was discovered that we have glands within us which are producing protective substances for the normal life, just as nature will, when there is an attack of a microbic disease, produce *pro hâc vice*, a protective substance against the foreign invader. When you take away the thyroid gland, and there is not this protective substance, which is no longer formed, then you find the body gets out of order because that is no longer present which was necessary in order to keep down the inherent forces of dissolution. Then it was discovered that, if you supplied artificially that which this gland produced, you could keep down this form of disease just in the same way as you can stop the effects of diphtheria, if you put in from another animal that which nature would produce in order to repel the microbic attack. Just as in diphtheria you get recovery, so you find in myxœdema that when you supply this substance which the body has ceased to have the power of producing, you get the remarkable discovery. And this appears to be only the commencement of a series of analogous successes. But, apart from the question of direct curative methods so arrived at, the power of diminishing suffering that is given through increased knowledge of the organism is so great that it is difficult to over-estimate it. I do not think it is necessary to give further instances to this Commission. I only want to put those forward to show that there is abundant justification for saying that knowledge which, from its very nature, must be obtained by experiment has already been of the very widest benefit to mankind. I will go on to the question of whether experiment is permissible for the purpose of acquiring such knowledge. Here I must go back for one moment to the example which I gave, as a typical one, of preventing plague getting into a town by killing all the animals which could convey it. In my view, we are just as much bound to prevent suffering due to ignorance, and just as much justified in inflicting such pain as is necessary for the purpose of doing so, as we should be in killing noxious animals. I have pointed out that this necessary knowledge has in the past been and must be obtained by experiment. It must be done by experiment, it can only be done by experiment on men or by experiment on animals. I unhesitatingly say that our right to experiment on men is extremely limited. In the first place, we have no right, voluntarily, to allow death to occur, even though the man would be willing to permit it. Nor do I think we have any right, voluntarily, to allow serious damage to occur, unless it perhaps may be where it is directly for the purpose of saving life. So I do not think, in view of mankind's power of anticipation, of memory, and the possibility of future suffering, we ought to take the responsibility of allowing experiments which might produce serious consequences to be made on man. Let me give an example. I do not think that we should have the right for experimental purposes to allow a man to infect himself under special circumstances, even though they would assist science, with a disease like syphilis, in order to add to knowledge. It is too serious a responsibility to take to allow that to be done, considering that that man will live for years afterwards, and cannot divest himself from social relations.

12736. Supposing he was sacrificing himself in that way, and by his sacrifice he could make probable the saving of a great many lives in future, why should you prevent him on the principle that you have to look forward to ultimate results of torture or sacrifice?—Well, it should be the last resource. But I will tell you what, in my opinion, I think constitutes the real difference. I do not think that you ought to lessen in any way the feeling of sanctity of human life, because the consequences are so very wide-spread. To let a man, in a moment of enthusiasm, do a thing which might, or perhaps must, have permanent evil effects throughout all his life, approaches, in my mind, very close to an infringement of the great principles which make us refuse to consider whether it would be a benefit that one man's life should be taken. Everyone of us knows men whom it would be a blessing to mankind to remove, whose lives are a curse to those around them, but, however much we feel that this is the case, we do not allow ourselves to act upon it, because if we were once to commence to act in that way towards men, the consequences that would follow would be so far-reaching that we have found it best to lay it down as a rule that it has been, except in special cases prescribed by law, not permissible under any circumstances for an individual to take human life or to permit a man to die if he can prevent it; and you will find that doctors will not only work as hard for a bad man as they would for a good man, but they will actually bring a man back from the doors of death, although they know that he is going to be hung for what he has done; because we find it is absolutely necessary to take as an unchallengeable principle that human life is sacred. Therefore I think that although in small matters you may use experiment on man, and for the very last stage of experiment you probably must do so, you ought to stop there.

12737. (*Sir Mackenzie Chalmers.*) You mean with the consent of the man himself?—Yes, even with the consent of the man himself. Where you have established a well-founded scientific probability that the thing is

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safe, then I think you are justified, but before you attempt to experiment on man you must have done everything to prepare for it, and you must know with fair scientific certainty what will happen. And that is the reason why I do not think that experiments on man are to be considered at all when you are thinking of the enormous amount of work that has to be done in scientific research. The necessary work must be done otherwise. Now, I turn to the question of whether we should use animals for the purpose of research. In my opinion, unquestionably, it is not only permitted to us, but it is our duty to try to remove ignorance by, if necessary, experiments on animals; and I want to give the Commission the principal reasons which make me have such a confident opinion on this point. The first reason is the overwhelming preponderance of the prevention of suffering by this method. The great bulk of the necessary experiments, as you know, can be made without any pain to the animals at all, because the animal never need be conscious, and therefore no pain is caused at all. But taking the others, the amount of experiment, if properly done, which causes pain is incredibly small compared with the results of the knowledge obtained, even if they be measured by what has already been achieved. Just look at the complete change that there is on our knowledge of all those great groups of diseases which I have called infectious or communicable. We are no longer fighting them blindly; we are no longer striking in the dark; we know what they are. This very knowledge enables us to avail ourselves of experiments which are not painful for the purpose of assisting us in the task, and every man who feels his responsibility does all he can without occasioning pain. But what is done is done with a knowledge of the enemy we are fighting, so that the experiments grow more telling. The number of painful experiments, as is shown by looking at the actual experiments which have been made, is quite infinitesimal. I read the description of the experiments done under licence in England, where excellent research work is done, and most carefully done too; I read through the list of those which were thus done in a year, and I very much doubt whether the total amount of suffering caused by those experiments would be much greater than would be caused in a single fairly large shooting party, where there were one or two bad shots. If you consider the amount of suffering that is caused in the world, not only thoughtlessly, but even deliberately done by people who are ordinary normal men, and may be able to show a reasonable defence for what they do, it dwarfs so utterly the amount that is requisite to produce this useful knowledge that if the matter was not so serious it would be almost ludicrous to think that there was this organised opposition to the pain caused in scientific research in proper hands. Why, what is attacked here is the only bit of fruitful pain in the world. The greater part of pain had better not be. A man suffers and dies, or suffers and gets well, and all the pain he has suffered has benefited nobody. And in the case of animals there is all this vast mass of pain which is inflicted or permitted, and people tolerate it and say nothing about it, and look upon it as an ordinary thing; but there is one little bit which brings return in lessening the sufferings of the world, and the people are to be found to organise themselves against it, and throw the whole of their strength into denouncing and preventing it. In my opinion, if you look at the medical science of to-day and the medical science of 40 years ago, and realise that the advance has been caused by physiological research, which is largely carried on under the very Acts which restrict, which regulate, but which distinctly permit painful experiments, and then consider on the one hand how much pain that has caused and how completely it has changed our power of dealing with disease and of alleviating suffering, it is really incomprehensible that anybody who sees the totality of misery in the world should think that the small amount of pain that has produced that gigantic result is the first that ought to be stopped. That brings me to the next point, and that is this. Mankind not only claims, but perpetually exercises and properly exercises the right to cause or permit suffering in animals when needful or desirable. It requires considerable thought to realise the extent to which that is admitted by all, and is, indeed, unchallengeable. Take the case which I gave of the rats on board the ship. I pointed out that it was right to kill those rats in order to prevent the possibility of their spreading the plague, but no such serious reason is necessary to justify killing them. I do not suppose there is anyone who would challenge the right of the captain to do it if they would injure the cargo, if they would produce a small pecuniary loss under these circumstances it would be considered that it was perfectly right for him to do it. We unhesitatingly kill vermin—we kill things that do pecuniary harm, and no one dreams that it is a thing which a man of high principle will not do. So that nobody ever dreams of questioning the right of causing pain for the purpose of a useful result, when that useful result is merely pecuniary. But when instead of the result being pecuniary, it means diminishing the suffering of the world for all time and for all the world, forsooth, it is challenged. Take the case, for instance, of a herd of cattle attacked by some painful disease, from which, we will say, 50 per cent. of those attacked survive. A man would be considered perfectly justified in allowing them all to go through the disease. The 50 per cent. that would die would die, the rest would have the pain of the disease and get better; and he would say, "Well, but I could not afford to sacrifice the 50 per cent. that would survive—it would be too big a loss to me." For the £10 apiece that they were worth he would have permitted them to be killed on the spot; but he considers that he has a just and perfect right to allow them to suffer all that pain in order to save himself from the loss of the 50 per cent. that survive. And I do not say he is wrong, because the comfort of his family and other things of importance to him may depend upon the question of how great is the loss inflicted on him pecuniarily by this attack. But if it is allowable to permit the whole of that suffering in the form of in order to avoid a pecuniary loss, the question as to whether it is right to increase by a little the suffering by well-designed scientific experiment to secure a return, not in money, but in the power of stopping suffering, seems to me literally to permit of only one answer. No man could consider the matter without realising that such infliction of pain was justified if he would do what I hold he ought to do: add inflicted and preventible pain together. The moment you take as your guiding principle that you are responsible for pain which you could prevent, and that this and the pain which you inflict must be taken together in order to guide your action, it appears to me that there is no painful treatment of animals which is nearly so completely justified as that which leads to knowledge. Nobody, so far as I know, would hesitate to overdrive a horse in order to save a life or to bring assistance to a life that was in danger, and yet that would be suffering inflicted for the purpose of benefit to a single individual. The people that are engaged in scientific research are working for something which will benefit not one single individual, but will benefit the race for all time.

I feel myself that the justification for the necessary use of even painful experiments, where they are required for the purpose of obtaining knowledge, can only be questioned by shutting the eyes to the further consequences of our action—putting a veil between our action and what it ultimately leads to. And I cannot help thinking that the people who lead the movement on the other side must be aware of this, because they deal so persistently in misrepresentation. It is very easy to misrepresent the work of those who are a long distance in advance from the ordinary man. I have known this illustrated again and again in manufacture. Processes have been stigmatised as adulteration, as falsification, and everything that was bad, whereas if people had followed out the advance of science they would have known that they were simply better methods for doing the same thing as earlier processes, and doing it with greater certainty and with greater cheapness. But they had not followed it out, and because the new processes were different, a contrast to the old ways, and people thought that therefore they were something to be stigmatised, so it is here. If we could get people to learn what science is and what it has done, they would never attack those who are at work on it. And it is to keep them from learning that there is this frightful misrepresentation. I remember, and I think the Chairman of the Commission probably remembers, how in the seventies the walls of London were placarded with a poster representing a rabbit in the process of being roasted alive.

12738. (*Chairman.*) I do not remember it, but we have been reminded of it; we have heard about the

roasted rabbit ?—I remember it well, because I wrote about it to the late Sir Michael Foster, and I asked him, What is the lethal temperature; because I recognised at once that the experiment which was thus misrepresented must have been one to find out when temperature alone would suffice to occasion death.

12739. You may take it for granted that we know all about it; we have had it explained by Sir Lauder Brunton ?—Very well. It turned out that the temperature was something like 109 or 111 degrees.

12740. (*Sir Mackenzie Chalmers.*) 114, to be exact ?—It differs very much, of course, in different animals.

12741. (*Chairman.*) That was the outside temperature, not the blood temperature ?—I was giving the temperature of the blood. Anyone would realise, of course, that the object was to do it under such circumstances that no reaction should be set up—that there was simply an increase of temperature, and that therefore there must be no disturbance of the organism, and that the poster was absolutely false. Yet that placard was all over London. It is not surprising that a great number of people join this organisation, because they get their ideas from these very serious misrepresentations. They believe themselves to be humane, but when one realises the evil that ignorance does, and that the only way in which ignorance can be removed is by the experimental method, and the enormous advantages in the way of saving pain that these results have produced, one must feel that the truly humane men are the people who are defending scientific research.

Now I should like to turn to the practical conclusions to which I have come with regard to this subject. You see that the good basis of all these movements is the desire of saving pain in animals and men. Now if that is so, the whole motive ceases with the conscious life of the animal. As soon as it has finally passed out of the conscious life the whole *ratio agendi* ceases. It is absurd to object to an experiment on an animal that has ceased to be conscious and will never again be conscious as it would be to object to the dissection of a dead animal. The only motive is the desire to save suffering, and suffering then is as impossible as if death had actually occurred. Now what is the consequence of that ? Hence there ought to be no interference with experiments on animals which are in anæsthesia, except that they should be permitted only to persons who can and will take care that the anæsthesia is complete, and that the animal is killed before it comes out. There is no other principle on which any interference could justifiably take place. There must be somebody in charge of or superintending the experiment who, from his character and position, can be trusted with the responsibility of seeing that there is what I may call permanent and effective anæsthesia. Therefore, with regard to such experiments, it is not only not necessary to interfere with their being used for educational purposes, but if the Commission may trust my experience they will indicate that in their opinon it is most important that they should be so used. For there is no comparison between the living effect of an experiment that you have seen and one that you have merely read of. May I give my own experience ? I have many and many a time read over carefully-described processes, and have imagined that I understood them; but I knew of old what that meant, and used generally, in cases of any difficulty, to insist on what we call "having a view." I used to go down and insist on being taken, not necessarily by an educated person—I much preferred a workman—right through the whole process, and seeing it done before my eyes. I went back with a knowledge of the case, with a power of handling it, and with a power of drawing conclusions as to the processes which were quite new-born. So it is with the man whom you are educating for this serious duty of fighting disease. If he has actually seen an experiment, it lives in his memory. But that is not the only thing—it remains in his memory as a living thing, and when he acquires new experience or new knowledge connected with it, he will find that it will make the old experiment mean more than it did to him even at the moment when he saw it; because he sees the whole thing before his mind's eye, and not only reads a description which, after all, only states certain results. And, as there is no ground, therefore, from the side of humanity to interfere with these experiments which cause no pain, I think they should be largely used educationally. Medical education is of prime importance to the community. The greatest difficulty in the way of giving to the world the full benefit of all the accumulated knowledge of this last 40 years is that you have to educate your practical men up to the level of it. Just as in war it is of no use having medical appliances at the base if you cannot get them up to the front of the army, so it is of no use to have this accumulated knowledge unless the power of using it to prevent suffering is in the hands of those whom you train. Therefore the efficacy of our methods of tuition ought not to be looked upon as a matter of little importance—it is a matter of the very gravest importance; and I should put no restrictions upon the use of painless experiments in connection with it, excepting that the experiments must be under the supervision of someone possessing a certificate such as I have indicated, which should be a certificate granted to persons who could be trusted with regard to anæsthesia. And, whether you have reports of those experiments or not I do not think is of the slightest importance, provided it is thoroughly understood that it is the duty of those possessing the certificates to see that the anæsthesia never ceases, and they are held responsible for so doing.

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I turn now to the more difficult question, a question which require more careful examination—namely, that of experiments that are in themselves painful. There, I think, you should divide the certificates into two classes. Certain men of acknowledged capacity in research, who have shown that they are not only capable of serious research, but have already done it, should have general certificates entitling them to perform such experiments, subject only to one thing, and that is that they should report fully every experiment. They are immeasurably better able to judge what is worth doing than any body of men that you could put over them. They are the people who are working at the subject itself, and they are, as a rule, men of such high position and high character in England that there can be no ground for being afraid of trusting them with this power. But I am satisfied that, both for the satisfaction of those who desire to proceed cautiously and for the good of mankind and also to prevent any possible abuse, they ought to report all that they have done. I do not think that a painful experiment should be allowed to pass into oblivion. It is made, in my opinion, for the benefit of the race and for the increase of knowledge; therefore I think such reports should be made, and I do not think that anybody concerned would really object to that being done.

12742. Do you mean that that report should be made by the inspector or by the operator ?—By the operator.

12743. Sent into the Home Office ?—Sent into the Home Office. I think he ought to do that. I would put him under no restriction as to the character of his experiments; I would not put him under any restriction as to the place in which they are done; but I would have a full report sent into the Home Office. Of course, a certificate of that kind would be given only in recognition of an acquired position and high worth in research. You can only do harm by interfering with men of that class. There is no danger of such men developing recklessness. I have not examined anything excepting the experiments that have been done in England, and I am therefore speaking only with regard to them. I have never, in the years that I have followed the matter, found anything which from this point of view I call recklessness of suffering in any experiment made for research.

12744. Have you found levity ? We have heard accusations of levity on the part of those who see experiments ?—I feel very strongly that such an accusation is unfair even in the case of students. I do not find that the medical student of to-day is a man of the Bob Sawyer type. I think they work extremely hard. Those who see them in their classes can give you better information on this point, but certainly those I have come across are hard workers and are not guilty of any levity.

12745. (*Mr. Ram.*) Would you have any inspection of these experiments ?—I would have no inspection of the first-class certificates.

12746. (*Chairman.*) You do not mean the first-class certificates described in the Act ?—No; I mean my first class. Just let me give an example. Supposing it was a question of giving a general certificate to Sir Lauder Brunton. The idea of controlling what he

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should do with it would, in my opinion, be nothing but a blunder. But he would have to report all that he did; and if, by some accident, a mistake was made in any case, if a person developed levity, or even recklessness or cruelty, his reports would show it, and his certificate might be withdrawn by reason of it. He might then be relegated to the second class, which I am going to describe.

12747. (*Sir Mackenzie Chalmers.*) How do you suggest that a man should get into the first class? You are imposing a new duty upon the Home Office?—I know it.

12748. (*Chairman.*) You begin by supposing that he is a first-class man?—Yes. I am quite prepared to say how I think the Home Office ought to decide. I think it ought to decide by the opinion of the leaders of the medical sciences, because I am satisfied that they possess, to an equal degree with any other body of men in the world, the moral qualities necessary to enable them to advise; and beyond question they possess, better than anybody else, the intellectual qualities and the qualities of knowledge which would enable them to do so efficiently; and a certificate of this kind should only be given where there was a consensus among the advisory body that the person held such a position in medical science that he might have this general certificate. I now pass to those who have not attained to that position—which, as I have said, would be an exceptional position, although I am happy to say that there are many men in England who would merit it. I think that in the case of younger men or men who had not yet attained to the commanding position of those to whom I would give this general certificate the lines of the present Act are the right lines. I think it is reasonable to expect them to accept specific limitation of the type of experiment, of the scope of the research, and of the place where the experiment is to be performed, in order that there might be the security of the supervision of those who are over the laboratory, if there be any such persons, and also the security of inspection. Limitation of this kind, which the people would be bound to respect, and I believe would respect, should prevent all danger of abuse, which, though I think it would rarely become a reality, is more possible to occur than it would be in the case of the exceptional men to whom I have before referred. I see no reason why those restrictions should not be put on; and, as before, full and complete reports should always be made, so that a record of all these experiments and their results should be preserved. The line, then, that I should advise, would be as follows:—Certificates with regard to non-painful experiments—that is to say, experiments where the animal was kept under anæsthetics and not allowed to survive—should be quite general, but should only be issued to persons who would perform or supervise those experiments, and could be trusted to see that the anæsthesia was complete and permanent. For painful experiments there should be a small but select class who should have general certificates. The other class should have special certificates, specifying the nature of the research, the nature of the experiment, and the place where it was to be done; and there should be full and adequate inspection of those places while the experiments were going on.

12749. Strangers you would not have present, I understand?—I would not have strangers present, first of all, because, in my opinion, it would not assist the experiment, and, secondly, I do not think it would be a good thing for unqualified persons to see these things. They are the serious work of research, and persons who are not trained by medical or physiological study would gain no good from witnessing them. But I would encourage students being associated with this research. It is the finest training that they can have. So far as it can be done without interfering with research work it is a mistake to keep people out who are capable of learning from it; but that the public should be admitted would, I think, be a still greater mistake. In my opinion, they have no business there, and it is much better to exclude them wholly.

12750. (*Sir Mackenzie Chalmers.*) You mean that the public would go there in the same spirit that the public used to go to an execution?—Exactly. Now I hope that I shall be forgiven if I say one or two words about the two restrictions which have been recently suggested to the Home Office.

12751. (*Chairman.*) Yes, we should like to hear your suggestion about that?—I thoroughly disapprove of them. The first relates to foreign savants who are not naturalised Englishmen. There is no possible reason why they should be put at a disadvantage as compared with British subjects. Of course, for the first class of certificates under the system recommended they must have a recommendation from English people of the proper position to show that they deserved the privilege; but I am not talking about that for the moment. I am talking about the existing system. Foreign universities and foreign men of science have been extremely good to our students. They permit them to take part in their courses; they permit them to use their laboratories, and without good reason I would not show any want of reciprocal kindness towards foreigners. No reason is put forward for thus placing them at a disadvantage, if they possess the qualifications which would justify the certificate being granted to an Englishman, excepting that it is suggested that you cannot enforce the performance of the conditions. That is quite a mistake. The only difficulty in the way of enforcing the performance of the conditions is that they are capable of changing their residence; but an Englishman who wants to break the conditions is equally capable of changing his residence. The true remedy in both those cases is to have a responsible head of the laboratory, who has to see that the conditions are duly performed step by step as the work goes on, and, in my opinion, there would be no greater difficulty in ensuring this being done in the case of a foreigner than there would be in the case of an Englishman.

12752. That would be so in the case of a foreign doctor working in an English laboratory; but, supposing that he wanted a laboratory of his own, and hired a laboratory of his own, and started it for the work for a year in England, would you have a superviser over him then?—I think you might quite fairly require that there should be some person, resident in England, responsible for the observance of the conditions—that there should be somebody, such as the head of the laboratory, to whom Government can look to see that the conditions are performed, not in a lump, but *pari passu* with the work done. You are justified in recognising a difference in the case of foreigners to that extent, but no further.

12753. Do you not think that if he took a laboratory by the year, we will say, the country has got just as much hold upon him as upon any Englishman who took it?—Yes. There are a great many foreigners in England who should be trusted to the same extent as an English person who is well known in England. But if there is any question about it, you must have somebody in England responsible.

12754. (*Sir Mackenzie Chalmers.*) The difficulty is that they may be well known to scientific men in England, but not known to the public?—Yes; but I think the action of the Home Office should be regulated by the knowledge of scientific men. These public reputations are apt to be misleading in both directions. You will find, if you take the public estimate of men, that the best lecturer is considered to be the greatest man in research. My experience is by no means so. There are men with a magnificent power of exposition, who are thereby so well known to the public that they are supposed to be the leaders of science, whereas in truth they are merely bearers of good news from the front, and are in no sense the leading workers. May I pass to the second restriction recently suggested to the Home Office?

12755. (*Chairman.*) If you please?—The second is that a medical diploma should be required. In my opinion, specialisation has gone so far that the distinction between people who actually work practically at the curative sciences, and those who devote their lives to research in those sciences is becoming marked. A man who is actively employed in practice, as a first-rate man ought to be, has not usually the time for consecutive research; he has to get his time in fragments just when he can. On the other hand, there are many men who are so devoted to research that they give up medical practice as a method of earning their livelihood, and devote themselves entirely to research. The wide diffusion of wealth in this country facilitates this. England has always been noted in science for the numbers of its first-class amateurs. Cavendish was an amateur, and I could mention many other great

names of men who have had the wherewithal to devote themselves to research, and have made it their life's work without seeking any remuneration for their labour. What should guide the Home Office entirely is, whether or not the applicants for certificates or licenses are suitable for research. Whether or not they have taken out a medical diploma which gives them the nominal right to practice when they have no intention to do so, but mean to devote their whole time to research, is a matter which should be quite immaterial. I am quite satisfied that there are now, and there will be yet more such in the future, men in the very first rank of research work who do nothing in the way of the practice of medicine and who deliberately abstain from it because it would interfere with their scientific research by taking their time and their attention. Those people ought to be encouraged rather than discouraged. Therefore this proposed restriction would, in my opinion, be a retrograde step. So that I should strongly advise that neither of those two things should be adopted as a principle.

12756. Have you studied the Act with a view to suggesting amendments?—You see, I agree with a great deal of it, excepting that I think there are a certain class of men who should have these unrestricted powers of experiment, and that the practice of the Home Office with regard to non-painful experiments should not have regard to the nature of the experiment, but simply to the reliability of the person on whom is placed the responsibility of the anæsthesia. With these modifications I do not quarrel with the Act, because I do not think that the real worker if he is not really hampered in his research, chafes against these restrictions, which are, after all, only a recognition of what I might call the fundamental principle of his action. He is working that pain may be lessened. He is a person whom I have always found extremely sensitive to the cry of suffering, and he has no wish to protest against this Act, which does homage to that principle provided that it does not hamper him. I have not specially considered the provisions of the Act, because I thought it had been dealt with so much by people who knew its working, and are so much more qualified to deal with it than I am; but I doubt whether my conclusions clash very much with the Act as it at present stands.

12757. (*Sir William Church.*) I should like to ask one question with regard to those to whom you would grant these first-class certificates. Of course, you consider that any person who is placed in the position of head of a laboratory connected with a University or a public institution approaching in importance to a University would necessarily be a person of sufficiently good repute?—May I put it another way? I should hope that a University would never choose a man for the position who did not deserve it.

12758. Then, besides persons in charge of laboratories, you would grant these what you call first-class certificates to such men of repute; and in their case I understood you to say they should not be necessarily restricted to working in licensed laboratories?—No; I should leave them perfectly free, subject to report.

12759. So that they might work either in their own rooms or wherever was most convenient for their work to be done?—Yes, certainly.

12760. Then, with regard to all others who hold licences, I presume that you look forward to their all working in licensed laboratories?—Certainly. Of course, I should trust that the best of those who commenced with a licence would pass in time to the higher degree; but, as long as they only had a licence, they would work in proper laboratories subject to inspection.

12761. So that, even as you propose, it would not be possible for a research student, whether holding a medical diploma or not, for him to perform experiments on living animals even in a licensed laboratory, excepting if he held a licence himself?—No; not painful experiments. He could not perform painful experiments.

12762. And, of course, all those who held what you call first-class licences would be enabled to perform the experiments which are now performed under Certificate B without having to get anything further than their licence?—Yes, certainly.

12763. (*Sir Mackenzie Chalmers.*) I should like to take first the two points that you have raised about the Home Office practice. I suppose you agree that, apart from the wording of the Act, the Home Secretary is bound to have regard to public opinion to some extent—that there is a real danger of the powers being lost if suddenly public excitement were got up against them?—He is bound to have regard, I should say, to the Act. I think that he has no need to regard public feeling beyond the extent to which it is expressed in the Act.

12764. You think that his sole duty is to administer the Act, without regard to whether he is imperilling the existence of the Act or not?—I think that the safest way to maintain the Act is to administer it in the way in which it is best for research. The real defence of research will come from the good that it has done; and I think it would be an unwise thing to administer the Act in a niggardly way, in the hope that it would lessen the possibility of a popular storm. I do not myself fear very much the popular storm. I know perfectly well the power of a certain small number of determined people to prejudice the minds of the public, and I appreciate that it is considerable; but the more that the public has experience of the good results of research, the safer it is in the hands of the public.

12765. Of course, as regards the foreigner himself, the man who is temporarily over here, he is to some extent free from the law. He performs certain experiments, and the results of those experiments are not published, probably, until he has returned; and then the Home Secretary may be blamed for allowing the law to be broken by a man over whom he had not effective control. You recognise that difficulty, do you not?—He ought to have effective control, and I think that this would be best obtained by requiring that the reports should be sent in from time to time, and that there should be some responsible person to see this is done.

12766. Would you go so far as this, that supposing a foreigner were authorised to experiment, say, at University College, you would make the head of the laboratory there surety for him?—Yes; I would make him see that the thing is done. And if the man is not performing his duty, it is the duty of the head of the laboratory to report to the Home Office that the experiments are being carried out without due observation of the conditions.

12767. But what practical power of enforcing it have we? Would you withdraw the licence of the head of the laboratory if there were any contravention of the conditions by a foreigner working in his laboratory?—Not unless he connived at it. But, in my opinion, the report of an experiment should be written out as nearly as possible at the time; and if he neglects to see that this is done it appears to me that he is responsible for the breach.

12768. Now, coming to medical diplomas, is there not this difficulty: that when once you get away from the medical diploma, you have no guarantee whatever as to a man. If he belongs to the medical profession, he has been trained up in a profession whose sole object is, as you say, to diminish pain. When once you get outside that, where are you to get your information from as to the qualifications, moral and scientific, of a person who is to have this licence?—Would you say that we had no knowledge of the qualifications of Frank Balfour, before he was killed, so unfortunately, years ago. You will remember that he started a biological school at Cambridge which was full of the greatest promise? Do you think that the addition of a medical diploma would have made Frank Balfour a more reliable man?

12769. Personally I do not. But then, you must take the case of a good many persons who are not known to the public, and you must remember that public opinion is nervous on the question?—I should require the most exceptional testimony in the case of a man who had not done scientific study at a university and under proper training; but that it should have taken the form of medical study seems to me unnecessary. You must understand that the scientific degrees, though they are often taken by medical men, and ought to be taken by them, are in themselves independent. They may not qualify for a diploma, but they connote an amount of knowledge which would quite warrant your recognising the bearers as fit men, if they were duly recommended by those who advise the Home Office. Of course, you will occasionally meet with a man who without any scientific study in a recognised university is a man of pre-eminent value. Michael Faraday had had, I think, no university training at all. But those cases are extraordinarily rare,

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and would only be dealt with when vouched for in a way which was quite unexceptionable. So that, generally, you would have the security of university recognition that the applicant was properly qualified.

12770. Do you think that the existing certifying authorities, which are mainly medical authorities, under the Act, would be able to deal with the case of the people you refer to without medical qualification?—I think so.

12771. Or ought the advising body to be enlarged?—Those who examine up at the Universities usually possess medical degrees, and a great many of them possess also science degrees. I do not know whether Sir Michael Foster had a medical degree.

12772. (*Dr. Gaskell.*) Yes, he had, and practised?—I did not know that. But you can easily imagine that there may be some who have not.

12773. (*Dr. Wilson.*) Pasteur had not; he was a chemist?—No, Pasteur had not; that is a good example.

12774. And Metchnikoff was a chemist?—Yes.

12775. (*Sir Mackenzie Chalmers.*) Then I should like to ask your opinion upon this. It has been suggested to us by Mr. Coleridge, whose evidence, perhaps, you have not seen?—I have not.

12776. It has been suggested that a certificate of humaneness should be required before a licence was given. What is your opinion on that? He suggested, I think, that a certificate should be given by one Justice of the Peace and one minister of religion. Perhaps you would rather not express an opinion?—It is not enough to say that it is unnecessary. It would be an absolute insult to the people whom you would be consulting. The suggestion that the heads of the medical profession are not judges of humanity, of humaneness, I ought to say, is a piece of the most intolerable insolence. It is shocking when you consider the way in which, as a rule, medical men disregard their own comfort, and put themselves to any amount of trouble and discomfort for the purpose of helping people who are sick, very often when it does not bring to them the slightest *kudos* or the slightest pecuniary return. To suggest that such people do not know what humaneness is and are not moved by suffering is intolerable.

12777. I think the suggestion rather was from the evidence that we have had, that the present certifying authorities regard only scientific ends and qualifications, and do not consider as within their province the question of humanity?—So far as Englishmen are concerned (of whom I can alone speak from personal experience) the question as to whether they are likely to use power properly from the point of view of regard for suffering is from my knowledge of the people who are likely to take out licences practically settled before the application is made. But the certifying authorities have to advise that licences should be granted to the applicants, and if they think that *par exception* they are people who have not got any care for suffering, and will make a recklessly painful use of their powers, they are bound to say that they are not fit persons to receive licences.

12778. Do you think that ought to be expressed in the Act or the rules in any way?—I should say "fit and proper."

12779. You think that would cover it?—I think that would cover it.

12780. Now, coming to the ethical side of the question; may I rightly sum up your statement in this way—I want to know if I have understood it—that all pain is an evil, perhaps the supreme evil?—Yes.

12781. And that we are justified in the present infliction of a lesser evil when there is a reasonable prospect in the future of avoiding a very much greater evil?—Yes, quite so.

12782. That would be the test and the touchstone which we should apply to all animal experiments?—Yes, that is the touchstone. And in applying that touchstone we must conscientiously use the whole of the teaching that the history of science and the history of medicine have given us. The particulars I have given were in order to show that where there is serious research there is an overwhelming probability that the answer will be in the affirmative.

12783. Of course, as you know, by other ethical witnesses, we have had other tests suggested. For instance, one of the witnesses suggested to us that the test would be the conscience of a progressive people. I must say it did not convey very much information to my mind?—It conveys none to my mind. I feel satisfied that that is an extremely bad definition. "Conscience" is too often used where the true expression should be "emotion." Emotion may be a good motive power, but it is a bad guide, and, in my opinion, there is a very great deal of force in the phrase which somebody has used: "If you want to do good in a particular way, and want to know how you can do it effectively, give your heart a rest and your brains a chance." The only thing that can safely guide you is your reason. Your emotions may give you a motive just as I say that the motive which should dominate the whole of this quesion is the desire for the suppression of pain. When you have got that motive and desire to obey it, it is your reasoning power which ought to tell what you ought to do.

12784. What is pressed upon us continually is this: Admitted that pain is an evil, you have no right to inflict a definite voluntary amount of pain when the future saving of pain is absolutely problematical, what do you say to that argument?—It is simply because the people have not studied the question that they talk about its being absolutely problematical. What you have to do in life is to act on probabilities. It is quite impossible to prophesy exactly what will happen in the future, but when you see from the teaching which the past has given us that there is an overwhelming probability in favour of a thing happening you are bound to act upon that probability. Would a person refuse to take a medicine because it had not always cured people? It would be a doctor's duty to give it if there was a probability that it would do so.

12785. Then passing from what I call first principles to the secondary principles, if I have understood your evidence, you say that at any rate we have no right to prevent experiment for the purpose of increasing knowledge and controlling pain and disease while we allow the infliction of death and pain in sport for the purpose of providing food for mankind and for a mere commercial purpose, such as spaying sows and castrating horses?—Yes, and in infinitely more cases than those—cases in which, as I have said, you inflict pain for the purpose of preventing pecuniary loss. If you have a valuable dog you see it through the distemper. If it is not valuable you kill it to begin with. Yet you might be perfectly willing to sell that dog in any case. That is simply permitting suffering for the purpose of avoiding pecuniary loss. Otherwise, I really do not understand why people consider it right to kill beetles. Beetles will only destroy a certain amount of their food, and yet they extirpate them, just as they extirpate mice and they extirpate rats, and consider that they are justified in doing so, because they say that such animals do harm. That harm is really pecuniary harm.

12786. Perhaps the fairest way of putting some of these ethical points that have been put before us is to call your attention to a little publication I have just received from Mr. John Page Hopps, who has given evidence here. May I ask you to look at the second paragraph on page 3, beginning with the word "But." May I divide it into paragraphs and ask you whether you have any comments to make upon it. May I read the first words that I want to ask you about: "But when all is said on the score of results, and the 'artist in vivisection has done his best to convince us that he is the apostle of mercy, many grave considerations 'give us pause.' For instance, what law of God or Natures justifies this treatment of our poor relations? Who gave to this amazing enthusiast the right to say off-hand that he was at liberty to exploit 'the lower animals' for experiments?"—May I point out what it is that gives the whole force of that to the people he is talking to? It is the use of the word "experiments." They do not know what it means; they do not know its importance. Supposing I was to put, instead of "experiments" "saving life or stopping suffering," the whole appeal would fall to the ground. "Experiment" to an uneducated person does not connote what it does to persons who are acquainted with the nature and results of research. If you read it in that way: "Who gave to this amazing enthusiast the right to say off-hand that he was at liberty to exploit 'the lower animals' for saving human life?" the answer would be "Why not?"

12787. As regards that expression of his, "What law of God or Nature justifies this treatment of our poor relations?" what is your comment?—It is all based on this want of appreciation of the meaning and effect of scientific research. Supposing I was to put it, for instance, "What law of God or Nature justifies our killing things, or permitting them to suffer, in order to save us from pecuniary loss?" Yet we do this, and must do it, every day. A beetle is a poor relation, or a mouse is a poor relation, just in the same way as a guinea-pig is; and yet we treat our poor relations in the shape of mice and beetles with phosphorus, and let them die in pain, because we say we must keep them down.

12788. If his argument was right, as a logical conclusion we should have no right to keep a cat who destroys mice by a cruel and lingering death?—Certainly not. We should first kill all the birds of prey, and then kill the birds they prey on because they prey on insects; and then we should kill the greater part of those insects because they live, many of them, on still smaller things. You get back to the state of the Hindoo. Nobody ever dreams of doing that. It is only when it is applied to scientific research, the advantage of which they do not know, that these qualms come in.

12789. They do not believe in scientific research and its results. They honestly do not believe in it?—They do not believe in it; but that is entirely due to their not having studied and mastered the subject, otherwise language such as that would not have any effect.

12790. And Nature herself nearly always provides a cruel and lingering death for every wild animal?—Yes, it is terrible. I think it is Seton Thompson who says: "The end of every wild animal's life is a tragedy." And it is perfectly true.

12791. Now may I take you to the next sentence: "For laboratory purposes, his spectacles would not be safe in the streets; and if he put his proposal into effect, and got his babies on the dissecting table, the very costermongers would raid his laboratory, and even medical students might help them. Why? The cutting-up of one baby might save the lives of thousands of other babies; and the torture of one for an hour might save thousands from suffering for years. Why object?"—I object because you ought to get that knowledge in other ways. Everybody who has studied the subject realises that the knowledge can be obtained in other ways. The analogy between living creatures is so marvellously close that you can, without any experiments on mankind (excepting that you ultimately apply to them the methods which you have worked out scientifically), acquire all the knowledge that is necessary.

12792. It has been suggested to us that if we encourage experiments on living animals, it *per se* will tend to encourage experiments on human beings. What do you say to that argument?—It is the alternative of it. I remember a discussion in society on the subject of these experiments, and a lady at last put this question to the anti-vivisector: "Would you prefer an experiment to be tried on a cat or a baby?" And the person replied, "I would have no experiment tried at all," not seeing that when you have not got knowledge, when you are working blindly, all you do is one long series of experiments on mankind. You are just as really trying experiments; you are not trying experiments under such circumstances that they enlighten you, and the consequence is that the result of what you do is to add very little to our knowledge.

12793. You mean that as long as you are treating disease empirically in mankind, you are experimenting?—Purely experimenting. You are either just sticking to ignorance, or, if you are trying anything new, you are experimenting on mankind.

12794. Pressing Mr. Hopps' argument to its logical conclusion, it would come to this, I suppose: If experiments on animals lead to experiments on babies, the eating of animals for food would lead to eating human beings for food?—You are honouring his argument by attempting to follow it out. That is what it would naturally lead to. But, in the case of experiments, it is because we will not try the experiments on babies that we do them on animals.

12795. Would you kindly look at the last paragraph on page 35, and the first paragraph on page 36. I think you have really dealt with that question. Mr. Page Hopps says, talking of voluntarily incurred experiments: "There is a splendid opportunity here for the suicide. Instead of a coward in retreat, let him be a hero as an offering. 'The world,' he says, 'is full of misery—sordid, diseased, and despairing. I will get out of it.' He is full of pity for mankind, and is disgusted at its lot; so he decides to give it no help, but to go. What a lame and impotent conclusion! What an opportunity for heroism lost!" Does that strike you as somewhat unpractical?—I have dealt with that. It is not only impossible, but to my mind it is immoral in the highest degree, and it shows an absolutely crass ignorance of the nature of scientific research to think that by a few suicides you could replace the experiments on animals which, in a complicated matter like this, must be numerous, but need rarely be painful, and which, if they are painful, can, in the great majority of cases, be stopped by inflicting death.

12796. I suppose another difficulty would be that you would not get any scientific man to experiment on the would-be suicide?—If he did, he would probably be saved the trouble of suicide himself.

12797. Then Mr. Page Hopps, at page 36, makes another suggestion as regards minor experiments, as he calls them: "Or might it not be possible to connect some minor forms of vivisection with crime? Here is a criminal who has earned his fourteen years of penal servitude. Let him commute it for six months in the hands of a reliable vivisectionist, within limits. And so throughout the whole scale of penalties"?—I think that is loathsome levity, that is all I can say.

12798. Then, leaving that and coming to what you have said yourself, you thought that a man is not justified in experimenting on himself when there is any prospect of real danger?—Yes, I think so, unless he is obliged to do it—and I think he rarely is.

12799. Take a well-known instance. Haffkin invented a prophylactic serum, or injection rather, for plague, as you know?—Yes.

12800. The first person he tried it on was himself. Was he justified or not?—Certainly he was justified if he had scientifically worked it out till he had a well-grounded right to expect that it would not lead to evil consequences. At that stage a man may justifiably, and to the great benefit of the human race, experiment upon himself.

12801. Either on himself or any other person who thoroughly knows what he is doing and consents?—Yes.

12802. (*Chairman.*) It is very like Columbus taking out his ship to discover America?—Yes, except that Columbus ought to have made a few more observations before he did it.

12803. He had good reason for believing that there was land out there?—No doubt.

12804. (*Sir Mackenzie Chalmers.*) Would you justify the course of experiments of Dr. Leonard Hill and his assistant experimenting on themselves in the way of testing how many atmospheres they could subject themselves to?—Yes. That is a most interesting instance. He had worked at the subject until he felt clear that he had got, if I might use the phrase, the rule of danger, and having worked it out he had the confidence to submit himself to it, and in that way he enormously assisted the acceptance of his methods. I think he was right. But he would have been perfectly wrong to go trying one method after another upon himself before he had worked his method out to that high degree of scientific probability.

12805. I think the only difficulty that I feel is this, where do you draw the distinction between the man who, for a scientific purpose, submits himself to a dangerous experiment, and a doctor who goes into a hospital reeking with typhus at the imminent risk of his life, to carry on the ordinary work of his profession?—I said that I made an exception when it was necessary to save human life. You see, if a doctor knows that if he does not do that many lives in that hospital will certainly be lost, I think then that he is perfectly right to say, Well, I will risk my life in order to save those lives. But in the other case, I think that he is a bad scientific man if he does not find a way of working it out, without exposing his life, up to a point when he is justified in doing so. That is my feeling. In giving this evidence, I have in my mind all the time what science is. Science is not the bungling, haphazard sort of thing that the world outside believes it to be. It is organised common sense, enlightened by appeals to actual fact, which are framed for the express purpose of giving

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you the information which is necessary for your common sense to act upon. And when you realise this you feel that the man who is content to follow scientific methods is not driven to these heroic steps, he can, with the minimum of suffering to animals and without endangering human life, work out the problems which are before him. If he is impatient he would like, perhaps, to experiment upon himself or on men, but if he is patient he need not do so. And I want the scientific man to be patient in that way.

12806. There is only one other question that I want to ask you. You urge the importance of painless experiments before students?—Yes.

12807. On the old principle *Segnius irritant animos demissa per aures*?—Yes.

12808. But what we have been told is that the performance of these experiments tends to demoralise students, tends to create a morbid curiosity as to pain in their minds. What is your opinion as to that?—My opinion is that that fear is entirely bred in the minds of the people who express it. I do not believe there is the slightest justification for it. We compel students to be present at operations on living men and living women because we feel that we must do so, and no one would listen to the suggestion that they ought not to be present there. How in the world can it have a bad effect when they see operations on animals if it does not when they see them performed on man? I do not believe that there is the slightest justification for the fear, and, as a rule, I do not like or attach any importance to suspicions as to impurity in other people without adequate evidence.

12809. (*Mr. Tomkinson.*) All your evidence, I think, is pretty well based upon the assumption that practically perfect anæsthesia can be and is maintained in these physiological experiments?—Yes.

12810. And you have no qualms about it as to the possibility of perfect and continued anæsthesia in operations?—I have no doubt whatever about it. Some one expressed it to you in a way which I think exactly represents the truth, viz., that consciousness is the first thing to go and the last thing to come back.

12811. (*Dr. Gaskell.*) Consciousness of pain?—Yes, consciousness of pain, I mean, is the first thing to go and the last thing to come back. We have the experience of people who have had the most serious and long-continued operations performed upon them, and have come back to consciousness, and their universal testimony is to absolute unconsciousness of pain.

12812. (*Mr. Tomkinson.*) Then that being the case, and anæsthetics being an established fact, there is no reason for me to ask you how far you would press your theory of the justifiableness of inflicting pain, even intense pain, upon individual creatures in order to prevent a much larger amount of suffering spread over a vast number of individuals; in other words, the infliction of vicarious sacrifices?—You ought only to do that where it is necessary for you to get the knowledge which will enable you to prevent pain in future, just in the same way as if in my simile of the plague-stricken ship. I think the man ought to take the most painless method that is applicable to kill those rats, but he must kill them even though it should be painful.

12813. But then that is to avoid and prevent a gigantic danger?—Yes.

12814. That would hardly be on all fours with a very painful experiment—an awfully painful experiment, say—upon an animal for a possibility of discovery?—That is just the point. There have been many painful experiments in discovering the true nature of disease which, as I say, makes us think so differently about all these microbic diseases that we cannot put ourselves back into the ignorance of 50 years ago; but they have been for the purpose of ascertaining the nature of these diseases in order that we might fight them. The number of very painful experiments is very few—extremely few. The greater number of the experiments have been of inoculations, and as soon as the disease has developed itself the animal is killed, because what the experimenters have wanted to find out was aye or no, would the disease be taken under such circumstances. The number of cases in which you have to allow really painful experiments is confined to these—that is, the cases where you are examining the distant effects of certain lesions of the nerves of interference with the digestive organs in order that when you see symptoms which are so produced you may know where the evil is. They are never, I think, performed casually; the very painful ones would almost always, I should think, in the history of English research in physiology be performed for some very definite object, in order to give us this very essential knowledge.

12815. And as a matter of fact, with regard to the destruction of dangerous or destructive animals, the common way of trapping them is about as cruel as can be adopted?—Quite so.

12816. And yet it is recognised?—Quite so.

12817. You probably would agree with me that some more stringent law in the direction of prevention of cruelty to animals would be very desirable in those regards?—Yes, I agree. That is just what I feel. I feel that this infliction of pain for the purpose of knowledge is the last thing you ought to touch. I should be very strongly in favour of legislation for lessening pain, if such a thing were possible; but I am happy to say that the need grows less. I think that there is a growing of increased sensitiveness towards suffering which would help legislation of the kind that you speak of, and which is even rendering such legislation unnecessary. I see it everywhere.

12818. Another instance, that entirely misleading statement of baking rabbits prompts the question of the boiling of lobsters?—Yes. There may be much that one might advantageously inquire into there.

ROYAL COMMISSION ON VIVISECTION.

FOURTH REPORT

OF

THE COMMISSIONERS.

Presented to both Houses of Parliament by Command of His Majesty.

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1908

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ROYAL COMMISSION.

WHITEHALL, 17TH SEPTEMBER 1906.

The KING has been pleased to issue a Commission under His Majesty's Royal Sign Manual to the following effect :—

EDWARD, *R. & I.*

EDWARD THE SEVENTH, by the Grace of God, of the United Kingdom of Great Britain and Ireland and of the British Dominions beyond the Seas King, Defender of the Faith, to

Our right trusty and well-beloved Cousin and Councillor WILLIAM COURT, VISCOUNT SELBY ;

Our right trusty and well-beloved Councillor AMELIUS MARK LOCKWOOD, Commander of Our Royal Victorian Order, Honorary Colonel of the 4th Battalion of the Essex Regiment ; and

Our trusty and well-beloved :—

SIR WILLIAM SELBY CHURCH, Baronet, Knight Commander of Our Most Honourable Order of the Bath, Doctor of Medicine ;

SIR WILLIAM JOB COLLINS, Knight, Doctor of Medicine ;

SIR JOHN MCFADYEAN, Knight ;

MACKENZIE DALZELL CHALMERS, Esquire, Companion of Our Most Honourable Order of the Bath, Companion of Our Most Exalted Order of the Star of India, one of the Under-Secretaries of State to Our Principal Secretary of State for the Home Department ;

ABEL JOHN RAM, Esquire, one of Our Counsel learned in the Law ;

WALTER HOLBROOK GASKELL, Esquire, Doctor of Medicine ;

JAMES TOMKINSON, Esquire ; and

GEORGE WILSON, Esquire, Doctor of Medicine ;

GREETING !

Whereas We have deemed it expedient that a Commission should forthwith issue to inquire into and report upon the practice of subjecting live animals to experiments, whether by vivisection or otherwise ; and also to inquire into the law relating to that practice, and its administration ; and to report whether any, and if so what, changes are desirable ;

Now know ye that We, reposing great trust and confidence in your knowledge and abililty, have authorized and appointed, and do by these Presents authorize and appoint you, the said William Court, Viscount Selby (Chairman) ; Amelius Mark Lockwood ; Sir William Selby Church ; Sir William Job Collins ; Sir John McFadyean ; Mackenzie Dalzell Chalmers ; Abel John Ram ; Walter Holbrook Gaskell ; James Tomkinson ; and George Wilson to be Our Commissioners for the purposes of the said inquiry

351. 2500—Wt. T.L 306.—3/08. Wy & S.

And for the better effecting the purposes of this Our Commission, We do by these Presents give and grant unto you, or any five or more of you, full power to call before you such persons as you shall judge likely to afford you any information upon the subject of this Our Commission ; and also to call for, have access to and examine all such books, documents, registers and records as may afford you the fullest information on the subject, and to inquire of and concerning the premises by all other lawful ways and means whatsoever.

And We do by these Presents authorize and empower you, or any five or more of you, to visit and personally inspect such places as you may deem it expedient so to inspect for the more effectual carrying out of the purposes aforesaid.

And We do by these Presents will and ordain that this, Our Commission shall continue in full force and virtue, and that you, Our said Commissioners, or any five or more of you, may from time to time proceed in the execution thereof, and of every matter and thing therein contained, although the same be not continued from time to time by adjournment.

And We do further ordain that you, or any five or more of you, have liberty to report your proceedings under this Our Commission from time to time, if you shall judge it expedient so to do.

And Our further will and pleasure is that you do, with as little delay as possible, report to Us under your hands and seals, or under the hands and seals of any five or more of you, your opinion upon the matters herein submitted for your consideration.

And for the purpose of aiding you in your inquiries We hereby appoint Our trusty and well-beloved Charles Clive Bigham, Esquire, Companion of Our Most Distinguished Order of Saint Michael and Saint George, Captain in Our Army, to be Secretary to this Our Commission.

Given at Our Court at *St. James's*, the 17th day of September 1906, in the sixth year of Our Reign.

By His Majesty's Command.

H. J. GLADSTONE.

Note.—His Majesty was subsequently pleased to create Mr. Chalmers a Knight Commander of the Most Honourable Order of the Bath.

ROYAL COMMISSION ON VIVISECTION.

FOURTH REPORT.

TO THE KING'S MOST EXCELLENT MAJESTY.

MAY IT PLEASE YOUR MAJESTY,

Availing ourselves of Your Majesty's permission to report our proceedings from time to time, we beg to submit to Your Majesty, as an Appendix to this our Fourth Report, Minutes of the Evidence taken during the months of October, November and December, 1907, as we think the present publication of these documents is desirable.

ALL WHICH WE HUMBLY SUBMIT FOR YOUR MAJESTY'S MOST GRACIOUS CONSIDERATION.

SELBY (*Chairman*).

M. LOCKWOOD.

W. S. CHURCH.

W. J COLLINS.

J. McFADYEAN.

M. D. CHALMERS.

A. J. RAM.

WALTER H. GASKELL.

JAMES TOMKINSON.

GEORGE WILSON.

CLIVE BIGHAM (*Secretary*).

December, 1907.

ROYAL COMMISSION ON VIVISECTION.

APPENDIX

TO

FOURTH REPORT

OF

THE COMMISSIONERS.

MINUTES OF EVIDENCE,

October to December, 1907.

Presented to both Houses of Parliament by Command of His Majesty.

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ROYAL COMMISSION ON VIVISECTION

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351*a*. 1,500.—Wt. T.L. 306. 3/08.—Wy. & S.

ROYAL COMMISSION ON VIVISECTION.

LIST OF WITNESSES.

In the order in which they appeared before the Royal Commission.

Date.	Name of Witness.	Profession, Occupation or Residences.	Representing.	No. of First Question.	Page.
1907. 29th Day - Oct. 23rd.	Mr. JOHN GLAISTER, M.D.	Professor of Forensic Medicine and Public Health, Glasgow University.	Faculty of Physicians and Surgeons of Glasgow.	1,2819	1
30th Day - Oct. 29th.	Mr. S. F. SMITH, M.R.C.S.	Surgeon to the Anti-Vivisection Hospital.	- - - - - -	13,030	10
31st Day - Oct. 30th.	Mr. F. GOTCH, M.A., D.Sc., F.R.S., M.R.C.S.	Professor of Physiology, University of Oxford.	Faculties of Medicine and Science, University of Oxford.	13,539	31
	Mr. M. S. PEMBREY, M.D.	Lecturer on Physiology at Guy's Hospital.	- - - - - -	13,968	49
32nd Day - Nov. 5th.	Colonel D. BRUCE, C.B., F.R.S., R.A.M.C.	- - - - - - -	Committee of Medical and Scientific Societies.	14,184	58
33rd Day - Nov. 6th.	Mr. G. G. BANTOCK, M.D., F.R.S. (ED.)	Consulting Surgeon to the Samaritan Free Hospital for Women.	Parliamentary Association for the Abolition of Vivisection.	14,530	76
34th Day - Nov. 12th.	Mr. J. N. LANGLEY, M.A., D.Sc., F.R.S.	Professor of Physiology, University of Cambridge.	University of Cambridge.	15,099	98
	Mr. G. SIMS WOODHEAD, M.A., M.D.	Professor of Pathology University of Cambridge.	University of Cambridge.	15,426	113
35th Day - Nov. 13th.	*Sir VICTOR HORSLEY, F.R.S., F.R.C.S. (App. A., I. and II.)	- - - - - - -	British Medical Association.	15,583	118
36th Day - Nov. 19th.	- - (recalled)	- - - - - - -	- - - - - -	15,966	138
	Mr. F. HOBDAY, F.R.C.V.S., F.R.S.E.	10, Silver Street, Kensington.	- - - - - -	16,284	150
37th Day - Nov. 20th.	Mr. W. OSLER, M.D., F.R.S., F.R.C.P.	Regius Professor of Medicine, University of Oxford.	Faculties of Medicine and Science, University of Oxford.	16,524	157
	Lieut.-Col. E. LAWRIE, M.B., I.M.S. (Retd.)	- - - - - - -	- - - - - -	16,793	167
38th Day - Nov. 26th.	Mr. JOHN HUGHES -	Secretary of the National Canine Defence League.	National Canine Defence League.	17,109	175
	Mr. J. ROSE BRADFORD, M.D., D.Sc., F.R.C.P. F.R.S.	Professor of Medicine, University College, London.	Committee of Medical and Scientific Societies.	17,621	189
39th Day - Nov. 27th.	Mr. H. HEAD, M.A., M.D., F.R.S., F.R.C.P.	Physician to the London Hospital.	Committee of Medical and Scientific Societies.	17,801	195
	Mr. A. D. WALLER, M.D., LL.D, F.R.S.	Lecturer at the University of London.	London University -	17,996	203

* *An asterisk is placed against the names of those witnesses by whom statistical tables, etc., were put in, which have been reprinted as Appendices, and the number of the Appendix put in is indicated in each case in brackets after the witness' name.*

Date.	Name of Witness.	Profession, Occupation, or Residences.	Representing.	No. of First Question.	Page.
40th Day - Dec. 3rd.	Mr. J. LORRAIN SMITH, M.A., M.D.	Professor of Pathology, Victoria University, Manchester.	Victoria University, Manchester, and Pathological Society of Great Britain and Ireland.	18,168	209
	Mr. J. H. LEVY - -	Honorary Secretary of the Personal Rights Association.	Personal Rights Association.	18,463	217
41st Day - Dec. 4th.	Mr. W. E. DIXON, M.A., M.D.	Professor of Materia Medica and Pharmacology at King's College, London.	Therapeutical Section of the Royal Society of Medicine.	18,646	225
42nd Day - Dec. 10th.	Mr. C. R. J. A. SWAN, M.B., M.R.C.S., L.R.C.P.	Consulting Physician to the Consumption Home, Gloucester place.	National Canine Defence League.	19,192	243
	Mr. R. J. COWEN, L.R.C.P.I., L.R.C S.I.	- - - - - - -	National Canine Defence League.	19,364	248
43rd Day - Dec. 11th.	Mr. L. E. SHORE, M.D.	Lecturer on Physiology, University of Cambridge.	- - - - -	19,428	250
	Mr. A. G. SCOTT, R.S.P.C.A.	Chairman Royal Society for Prevention of Cruelty to Animals.	Royal Society for the Prevention of Cruelty to Animals.	19,484	252
	Sir F. BANBURY, Bart., M.P.	- - - - - - -	- - - - -	19,498	252
	Hon. S. Coleridge, (recalled)	- - - - - - -	National Anti-Vivisection Society.	19,842	264
44th Day - Dec. 17th.	Mr. D. J. HAMILTON, M.B., F.R.C.S. (ED.)	Professor of Pathology, University of Aberdeen.	University of Aberdeen.	19,984	270
45th Day - Dec. 18th.	Sir G. KEKEWICH, K.C.B., M.P.	Honorary Secretary of the Parliamentary Association for the Abolition of Vivisection.	Parliamentary Association for the Abolition of Vivisection.	29,380	282

MINUTES OF EVIDENCE

TAKEN BEFORE THE

ROYAL COMMISSION ON VIVISECTION.

Wednesday, 23rd October 1907.

TWENTY-NINTH DAY.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. JOHN GLAISTER, M.D., called in, and Examined.

12819. (*Chairman.*) You are a Professor of Glasgow University?—I am—Professor of Forensic Medicine and Public Health.

12820. And you have been desired by the Faculty of Physicians and Surgeons of Glasgow to give evidence here?—I have.

12821. Is that one of the licensing bodies qualified to give diplomas in medicine and surgery?—Yes, and also diplomas in public health.

12822. And you are a Doctor of Medicine yourself, I believe?—I am, of the University of Glasgow.

12823. And you are a Fellow of the Faculty of Physicians and Surgeons of Glasgow, and a Licentiate also of the Royal College of Physicians and Surgeons of Edinburgh, and you have other qualifications?—That is so.

12824. Have you yourself ever taken out a licence under the Act?—No, I have not had facilities until now. I have a place which has now been registered in my new buildings, in my new department. I have not done so before, because I had no accommodation for animals, as a matter of fact. Now I have a building which is registered under the Act for the purpose.

12825. (*Sir William Collins.*) Do you hold a licence?—Not yet; I have not applied for one.

12826. (*Chairman.*) Do you propose to do so?—Yes, immediately.

12827. The subject is one that you have studied?—Yes, for 25 years I have been a teacher on the subject.

12828. But you have never had a licence yourself?—I have never held a licence up till now.

12829. At the same time you are familiar with the Act of Parliament?—I know it, and I think I know all about the certificates.

12830. And you are familiar with experiments on animals?—Yes, very familiar. I may add that on several occasions I have had to sign the certificates of others. Being one of the statutory certifiers under the Act I have had for some years to act as a signator of the certificates of others.

12831. Have you found any difficulty in carrying out your duties as a certifier under the Act?—No, I have never had any difficulty on the question as to the need for experiments upon animals. Before signing a certificate I have always satisfied myself of two things. First, that the applicant was a capable person to perform the experiment; and, secondly, that the experiments, in my mind, ought to be performed.

12832. And you have always been able to deal satisfactorily with those points?—Yes.

12833. In that respect, as regards certifying, is there any amendment that you would like to suggest in the Act?—The only suggestion that I would make is this. So far as I am able to see, the certificate "A," which is usually confined to inoculation and feeding experiments, is not large enough for such work as my own, for example. As Medico Legal Examiner to the Crown in Glasgow and the West of Scotland, charged with the duty of examining *post-mortem* the bodies of those who die sudden and violent or suspicious deaths, I am required to report to the Crown as to the cause of the death. In some cases the death has been due to poisoning; it may be from food or ordinary poisons, or more occult poisons, and, if one in Certificate "A" laid oneself down to ask for a certain number of experiments, on account of the uncertainty of the amount of work to be done in the year, one might have some difficulty in having the number of animals named in the certificate not exceeded; that is to say, I might require more animals in one year than in another, and if I put down a certain number, or, if the number were laid down for me by the Home Office, I might not be able to conduct the work. I would like to say further that I understand there is a certificate, which has not, I think, been mentioned in the evidence, so far as I have read the three volumes, called "A 2," which has been issued by the Home Office to a small number of investigators. I have only seen it scheduled in the last few days. The terms of that certificate would be quite satisfactory to me, inasmuch as the number of experiments is not named, the time of carrying out the work is not laid down, and the range of work would cover all that I personally would require.

12834. (*Mr. Ram.*) Have you got a copy of it?—I have only got a copy in writing.

12835. (*Sir John McFadyean.*) May I ask is it quite the rule that in Certificate A the number of experiments is limited?—I cannot say. I have only seen one of these certificates, and the only certificate that I saw did not bear any number of experiments.

(*Sir Mackenzie Chalmers.*) Have you ever applied for a certificate with regard to your duties and been refused?—As I have already explained to his Lordship, I have never applied for a licence or certificate at all, because up till now I have not had facilities. I have now, and my place is registered, and I am just waiting to get a comprehensive enough certificate to make an application.

Mr. John Glaister, M.D. 23 Oct. 1907.

12837. (*Chairman.*) Your attention has been particularly directed, I presume, from what you have said of your duties, to the question of discovering poisons in the human body?—To the discovery of poisons in the human body and making investigations there anent.

12838. And as to the symptoms which those poisons develop?—Yes; and I may add further that there is a range of causes of poisoning due to food, of which I have had several cases in the last few years, which are more difficult to deal with than many cases of mere mineral poisons. My assistant, for example, is Medical Officer of Health of a burgh, and he works with me in any epidemic disturbance that there is. There was an epidemic of milk poisoning in which 600 people were involved, and I should like, of course, to be able to include in my work such investigations as that kind of work would demand—food poisoning.

12839. You mean in the new registered building which has been provided?—Yes.

12840. That is specially what you would like to experiment upon?—Precisely.

12841. Other things besides milk, of course?—Yes, any kind of food.

12842. What kind of poisons are developed there. How do you speak of those?—The cases which come under one's investigation are, for example, in the case of shell fish, the case of the typhoid fever bacillus, the bacillus coli communis, which is a type of bacillus, Gaertner's bacillus which is called the bacillus enteritidis, and a third group, the sausage bacillus, the bacillus botulinus. These form groups of meat poisoning micro-organisms, some of which form toxins, that is to say, the products of their growth, which of themselves are poisonous, apart from the existence of the organism itself. I had a case, for example, within the last three months in which the criminal authority of a neighbouring county sent me the internal organs of a girl, one of a series who were made ill and had died after eating sausages.

12843. (*Sir William Collins.*) Could you not deal with that case then?—I could only deal with that case chemically, and I got an experiment made, which I adopted, by a colleague whose licence covered such an investigation.

12844. (*Chairman.*) Do you consider that it is necessary to make experiments upon animals for the purpose of making these investigations about poisons?—I have no doubt about it. I think it is essential. I have mentioned in my précis the very well-known Dr. Lamson poisoning case, in which aconitin was the active poison, of which the poisonous doses are an infinitesimal part of a grain, and in which case Sir Thomas Stevenson discovered the poison along with Dr. Dupré, partially by experiments upon himself and upon mice.

12845. (*Sir Mackenzie Chalmers.*) Everything that was required was done in that case, was it not?—Absolutely. I am only pointing out that you require animals to test whether you are dealing with aconitin or not. I say without this physiological test you cannot detect your poison.

12846. (*Chairman.*) You cannot detect it by a chemical process?—No, you cannot prove it by chemical process.

12847. (*Mr. Tomkinson.*) The effect must be watched upon living animals?—Precisely.

12848. (*Sir Mackenzie Chalmers.*) Dr. Stevenson, in fact, experimented upon himself as well, did he not?—I have just said so. No chemical test would reveal that poison, but a physiological test would.

12849. (*Sir John McFadyean.*) Was that before the passing of the Act?—No, it was in 1882, I think.

12850. (*Sir William Collins.*) What animals did he use?—Mice.

12851. (*Chairman.*) That investigation of food, I suppose, is of growing importance, there being so much preserved food—tinned food?—I think it is absolutely essential—that we must have animals to experiment upon in such cases as those. The subject is yet in its infancy; it has to be opened up yet.

12852. Are there any other aspects of public health which make experiments of this kind necessary?—If you take a disease, such as cerebro-spinal meningitis, we have been having a large outbreak of it in Scotland during the past two years. I have the official figures here, and during this present year, 1907, from February to September in Scotland there have been 1,611 cases reported to the Local Government Board. Of these 1,028 occurred in the county of Lanark, in which Glasgow is situated. During 1906, up to March of this year, in Glasgow there were 312 cases and 230 deaths, a death rate of about 70 per cent., 73.7, to be quite accurate, and in the present year, between the 29th of March and the 11th of May the number of cases was 229. The question at issue with regard to that disease is how is the disease propagated. We have great difficulty in knowing how the disease is propagated now. An investigation has been started with regard to an examination of the air passages of apparently healthy persons acting as passive carriers; that is to say people who have been in contact with a case of the disease have been found to carry passively in their upper air passages this organism, and it has been found in these. That subject must be elaborated, and the nature of the organism itself has to be discovered. This disease simulates certain cases of food poisonings. In 1902 I was asked by the Corporation of Paisley, an adjoining burgh to Glasgow, to make an investigation regarding the causes of illness of several persons; I forget whether six or nine, three of whom had died, to make a *post mortem* examination on the body of one of the victims, and to report. I was able in that case to discover and isolate and photograph the organism of this disease. We do not know much about it yet, and one must make experiments upon animals in that direction.

12853. When you say an organism of this disease you mean a microbe?—A microbe. Of course we know the microbe, and we know how to grow it, but it is in its relation to its spread and other things that we want to make investigation.

12854. (*Sir Mackenzie Chalmers.*) Are animals susceptible to this disease?—That I cannot tell you. That is one of the things we want to find out—whether animals are the vehicle of contagion or not.

12855. (*Chairman.*) Have you experimented on animals?—No, I have not. I am eager to begin.

12856. Has it been done with regard to this disease?—Not so far as I know up till now. It has not been done in this country much so far as I can make out.

12857. (*Mr. Ram.*) It could be done to-day?—Yes, it could be done under the Act.

12858. (*Sir John McFadyean.*) When you say that it has not been done in this country have there not recently been some experiments carried out by Dr. Stuart McDonald in Edinburgh with regard to monkeys?—Yes.

12859. (*Mr. Tomkinson.*) Do you hold that this microbe must be either communicated by infection or caused by some specific food or other, and is never a natural growth?—I did not say that. All microbes, of course, must proceed from previous microbes, but the question of their communication from one person to another is the question in which difficulty is experienced just now—how was it spread? If we knew that we might devise preventive measures.

12860. But it does spread?—Yes, undoubtedly it does spread.

12861. By infection?—I think so.

12862. (*Chairman.*) Then at present all that you have to say about that is that you look forward to experiments upon animals to assist you in discovering how it spreads?—I think investigators will require to call in the help of animals to elucidate these points.

12863. You are not in a position to say that any experiments have been made which have thrown any light on the subject at present?—No, because the information which one of the Commissioners kindly referred to just now with regard to monkeys would not throw any light upon the possible spread, as of course we do not have monkeys in this country except as caged animals.

12864. Then as regards the wilful poisoning, the criminal poisoning of people, what is your experience?—I think I can safely say that there are a certain number of alkaloids which are so poisonous that I do not think any chemical means could possibly detect them, of which we know comparatively little now, and which must be investigated by the agency of animals.

12865. You do not think that any improvement in the knowledge of chemistry would do it?—I do not think so, for this reason, that it is a more difficult task than even looking for the proverbial needle in a haystack.

If the poisonous dose, for example, of a drug be half a grain (and that is an exaggerated case with regard to some of them), and that is taken into the body and causes the death of the individual, the most accurate chemical processes would not be able to isolate from the remains of that body anything like half a grain, and even if rescued, as in the case of aconitin, chemical tests would not enable one to say what it was; physiological tests might, and probably would.

12866. Have experiments increased your knowledge of those poisons that you speak of—aconitin and alkaloids?—They have materially, regarding their action, and it is because of their action upon animals having been ascertained by experiments that one is able by taking an alkaloid, of which we do not know the precise nature, and putting it into an animal, to get to know that the poison is aconitin. I may point out in that connection that there are other questions associated with poisoning of very great importance; for example, the best antidotal remedy for acute arsenical poisoning, assuming a person wilfully or accidentally poisoned by arsenic, was really brought into practical operation by experiments on animals. We had never known about this except theoretically until it was distinctly found to be valuable from researches upon animals. In the early thirties of last century in theory it was believed that the use of a certain preparation of iron would be beneficial. Between 1834 and 1840 certain experiments were made on the dog and on the horse, which showed that this hydrated ferric-oxide, freshly made, would be distinctly antidotal. In 1861 Dr. Watt published in the "Ohio Medical and Surgical Journal" for March of that year certain experiments which he performed on five dogs, in which he gave them poisonous doses of arsenic, and at varying intervals thereafter this freshly-prepared iron oxide, and showed the beneficial effect which resulted therefrom.

12867. He gave it as an antidote?—He gave it as an antidote, and his object was to show that this substance was an efficient antidote if used after the poisoning within a given period.

12868. You say that he found that it was a satisfactory antidote. Did the dogs recover?—I will just tell you the facts. He administered arsenic alone to five dogs in different doses from three grains upwards, and all of those dogs died from the effects of the poison within eight hours after the administration. To a second series of twelve dogs he administered varying doses of arsenic, all of them lethal doses, but afterwards administered to them the antidote, in some instances not until the poisonous symptoms had appeared, but all the animals recovered; and it is upon evidence of that kind that we now in all our books and all our teaching recommend this as an antidote to arsenic.

12869. And is it used for arsenical poisoning of human beings?—Yes, it is the antidote used to-day.

12870. Does it have the same effect as it had upon the dogs, of curing them?—Yes, there have been many cases recorded of recoveries from arsenic poisoning where it has been administered.

12871. Do you attribute that discovery entirely to those experiments?—Surely. Before that it could only be theory.

12872. What gave rise to the theory?—The theory is that if you take a solution of arsenious acid you can precipitate that as the insoluble salt of iron with an iron preparation. You can do that in a test tube—everybody knew that—but the question came to be how could it be practically applied to the human stomach? Could you utilise in any way the human stomach as you would use the test tube? And it was with the object of discovering that practically that these and many other experiments were carried through.

12873. Was this dose that was given as an antidote to human beings the same liquid that was put in to operate upon the arsenic in the tube?—The same preparation precisely, only in the human stomach the quantity of iron oxide must be considerably increased in order to get the same effect.

12874. Then this was a case in which from chemical tests you had discovered the probability of the remedy being effective?—Yes, through the physiological medium of application to individual animals.

12875. And it was to make sure of the results in the human stomach that you tested it on animals?—Yes, and in order to know how it could be made sure—by what doses.

12876. (*Sir Mackenzie Chalmers.*) What I understand is that this remedy was first tried in a test tube, then tried on a dog, and then, having answered those two tests, it was used successfully with human beings?—Yes; the experiments on dogs determined the amount of the antidote to be used to the poison—the method of use.

12877. (*Chairman.*) What I wanted to get, and what I think you have given me, is that the scientific fact that this was an antidote was discovered in the tube by chemistry?—Yes, exactly.

12878. But it was not safe or certain as a remedy to human beings until you had tested it upon animals? Surely not, because we cannot use the human stomach as a test tube—the two things are different.

12879. Are there any other special cases of valuable testing of poisons to which you wish to call attention?—I desire to draw attention to one more, and it is a rather interesting one. It is the case of a very fatally poisonous arsenical gas called arseniuretted hydrogen. A few years ago I had two cases of this occurring in a very curious way. A man engaged in chemical works in the manufacture of bleaching powder in a Welldon's retort suddenly became overpowered, and had to be taken out. He died within three days. The man who followed him also was overcome, but he recovered. The process of making bleaching powder by a Welldon's retort is this: Hydrochloric acid is mixed with a certain proportion of manganese dioxide; the manganese dioxide acts upon hydrochloride acid, liberates the chlorine gas, which saturates the lime and forms the bleaching powder. The retort is made to work continuously, night and day, for from two to three weeks. At the end of that time a workman has to proceed in from the top by means of a rope ladder after the retort has been steamed out with live steam to dig out and remove what is called by the workmen the mud, the débris, which lies at the bottom of the still. I was called in to consult with other doctors regarding this man before his death, and after seeing the man I went to the chemical works and got the details of the process just as I have described them. I came to the conclusion that the man died from this gas. The case required to be reported to the Home Office. It was reported, and the man having died, a *post-mortem* examination of his body was made, and Sir Thomas Stevenson, who chemically examined the remains, found arsenic therein. The interesting thing about that case is that this arsenical gas did not exist in the retort when the men went into it. As the implements for the removal of this mud they took an iron shovel and a zinc galvanised iron pail, and they themselves manufactured the gas which caused their death. They manufactured on a large scale what we know as the Marsh's test for arsenic, after Mr. Marsh, who invented the test in 1836. They generated the gas which killed them. Since these cases I have investigated the literature of the world, and I have found a large number; I have collected over 100 cases of deaths from this cause in a large variety of occupations, and perhaps your Lordships will be aware that in the State Ballooning Department of France several deaths have already occurred from this gas being present in the impure hydrogen by which the balloons are inflated. All the evidence that we have regarding the physiological effect of this gas is from experiments made on the Continent. I am not aware of a single experiment having been made in this country on that subject.

12880. What is the sort of experiment on an animal that you would suggest, if it had to be made?—The experiments on the Continent have been by subjecting dogs to varying percentage proportions of this gas mixed with air, and studying the symptoms that follow. My attention will be directed, not to the result of the gas, but to the prevention or cure of it when it happens. Nothing has been done in that way so far as I know. There have been various suggestions.

12881. You would have first to ascertain what in all probability would be the antidote, I suppose?—Yes, for this reason. This gas acts differently from arsenic itself; it breaks up the corpuscles in the blood; it acts directly upon the blood, and thus an antidote is more difficult to be arrived at.

12882. But when you have discovered the antidote you will have to test it upon animals?—Yes, we must use animals. I would not dare use anything else than an animal for this.

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12883. Is there any other fact you would like to bring before us as regards poisoning and experiments upon animals?—May I just finish by adding in a group answer that our knowledge of antidotes with respect to a large variety of mineral and vegetable poisons has been derived from experiments upon animals of different kinds. I have a whole list here of those of which I have data, and to which I can give references.

12884. Perhaps you will furnish them to the Commission?—These are some of them: Mercuric chloride, examined by Sir Benjamin Brodie on rabbits and cats, and the reference is "Philosophical Transactions," Vol. CII., page 222 (1812). Tartar Emetic: Magendie (*Mémoire, sur l'émétique*), experimented on dogs with and without the gullet being tied. (Orfila-*Toxicologie Générale*, Vol. I., page 469.) Lead Poisoning: Orfila, Christison and others examined the effects by its action on animals. (Christison on Poisons, 4th Edition, 1845, p. 548 et seq.) Prussic Acid: Magendie experimented on dogs and birds (*Annales de Chimie*, Vol. VI., page 347; Christison on Cats, Op. cit., page 757.) Hemlock: Orfila on Animals. Christison in 1835 found it a deadly poison for dog, cat, rabbit, mouse, frog, fly and flea; and Geiger, for kitten, pigeon, swallow, glowworm and earthworm. Laburnum: (Ross on Dogs. Christison on Rabbits, op. cit.). Strychnia: Marshall Hall's test with frogs. (Wormley on Poisons, p. 584.) Digitalis and Digitalin, by Tardieu, on frogs in the Pommerais case (Author's Manual, page 493).

12885. You said, I think, that a certificate which was in use at the Home Office now was one that you would be satisfied with?—Yes. I only discovered it after writing my *précis*. That would be quite satisfactory to me.

12886. An alteration from the ordinary certificate to that is the alteration that you would recommend?—For certain heads of departments. I would like the Commission to distinguish, if possible, between the man who is the head of a department supervising the whole department, and a junior working under him as assistant or a research student. The head of the department is the responsible person. I have a department, and there will be men working under me in different directions.

12887. The head of the department holding a licence?—Surely. The head of the department holding a licence ought to have more liberty with regard to the range of work, always subject, of course, to the provisions for inspection and other things, than an assistant or research student should have, and a little more elasticity should be shown in this regard in the certificates by the Home Office.

12888. (*Sir William Collins.*) In regard to the interesting evidence which you have given us with reference to the antidote for arsenic by hydrated oxide of iron, did I correctly understand you to say that the use of that dates from 1861?—No, what I did say was that theoretically the iron was known as an antidote since the early thirties; indeed, between 1834 and 1840 the first experiments on dogs, I think, were made. The reference I have here is to experiments made on dogs between 1834 and 1840.

12889. Did not Bunsen point out the antidotal effect of hydrated peroxide of iron in the case of arsenic?—I do not know the date when Bunsen did it, but he only did it as a chemist would naturally, knowing the reaction between the iron salt and the solution of arsenic forming an insoluble arseniate of iron.

12890. But you stated that ever since the 1861 experiments this antidote had been recommended?—Yes.

12891. I suggest to you that it was recommended before 1861?—I should not like to contradict you, but if you take "Christison on Poisons" I do not think you will find Bunsen's name mentioned in connection with it. I am quoting from Christison just now.

12892. And I think you will. When was "Christison on Poisons" published?—The edition that I have is 1845, the 4th edition.

12893. Do you say that in that you will not find reference to Bunsen and Orfila and Douglas Maclagan making experiments with regard to the antidotal influence of hydrated oxide of iron on arsenic?—I certainly agree with you as to Orfila and Douglas Maclagan, but I cannot remember seeing Bunsen mentioned. Of course, to err is human, and I will not contest the point with you, but my recollection is that Bunsen's name is not mentioned; I may be wrong.*

12894. Should I be wrong in thinking that in 1845 or 1846 in Christison's work hydrated peroxide of iron was recommended as the antidote for arsenic?—Yes, it was recommended then.

12895. You state in your proof, I understand, that it was since 1861?—No, I think not.

12896. I have it here "Ever since that time," that is in 1861, "this antidote has been recommended to be used for human beings in such cases"?—Yes.

12897. I ask whether it was not recommended to be used in such cases prior to 1861?—I have no doubt that it was, and what makes me say that all the more freely is that in Christison's work the results of the experiments were so varied that Christison set himself to find out wherein the inconsistency lay.

12898. Was not the inconsistency largely a question of the quantity of hydrated peroxide used?—Very largely.

12899. Had not the fact that the inconsistency was due largely to the amount of hydrated ferric oxide used been clearly explained by Douglas Maclagan some time in the forties, showing that it was the amount of the dose that gave the varying results?—Yes. Christian made it quite clear, I think, at that time that it was merely a question of the sufficiency or insufficiency dependent on the quantity of the antidote in relation to the poison.

12900. That was known in the forties?—Yes, in 1845. Prior to that many of these experiments were made on dogs to which I referred.

12901. The experiments on dogs, I gathered that you referred to, were by Watt in 1861?—I mentioned other experiments on dogs between 1834 and 1840.

12902. I understood you to give us the date 1861 as the time from which the use of this ferric oxide had been recommended in the case of arsenic poisoning?—Yes, I know that since 1861 in every book on toxicology you will find the remedy given.

12903. But you are not so clear that it was not recommended before?—In some books it was, and in some books it was not.

12904. And in Christison you think it was?—Undoubtedly—I have no doubt regarding Christison.

12905. (*Chairman.*) Christison recommended it to be used by a physician when he came in contact with a case?—Yes, but I would like it to be understood without any doubt that Christison's opinion came after experiments on dogs conducted by M. Soubeiran, M. Miquel, and M. Boulay, and those researches were published in "Annales d'Hygiène Publique," volume XIV., p. 134.

12906. (*Sir William Collins.*) You did not think that Christison referred to Bunsen?—My memory does not serve me that Bunsen's name is mentioned, and I may say that I read over Christison on Saturday night.

12907. You also said that you had searched the literature of the world?—With regard to asseniuretted hydrogen.

12908. And it is a more limited research that you made in regard to arsenic?—I may say that I have here my book on the subject—"Toxicology and Public Health." I have a large number of references in it, and I have read very largely the literature on the subject.

12909. You have yourself conducted researches in forensic medicine?—Yes, many.

12910. But you have never had occasion to use vivisection?—I should not say that. There have been occasions when I should like to have had the advantage of vivisection.

12911. Have you had occasion to use vivisection yourself?—I have never used it.

12912. Then in such researches as have enabled you to enrich the subject of forensic medicine it has not been the result of vivisection by yourself?—No.

12913. You lecture, of course, in Glasgow on forensic medicine?—I have been a teacher of the subject for 25 years.

* *The witness subsequently wrote that he was wrong, having misquoted from his notes, and that Cristison does mention the name of Bunsen associated with the antidote in* 1834.

12914. Do you illustrate your lectures on forensic medicine by vivisection. I suppose you do not?—No, I have no licence, and I have had no accommodation for animals in my department in the University till the new buildings were opened in April of this year.

12915. In your opinion is it necessary to illustrate your lectures on forensic medicine by vivisection?—Surely not. I do not see any necessity for repeating experiments before a class with regard to poisoning, but I think it is essential that we should have facilities for legal work and work of that kind.

12916. You are aware that under Section 12 of the Act special provision is made for that?—Yes, I understand that that is so.

12917. (*Sir John McFadyean.*) With regard to this Certificate A, is it not a fact that the ordinary form of Certificate A bears that the research may be limited either as to the number of experiments or as to the time or as to both in the opinion of those who certify, and that considerable latitude is always allowed by the Home Office in regard to that?—The rubric on the margin of Certificate A, which I have before me, says that it may be given for such time or for such series as the person signing may think expedient.

12918. That is precisely what I wanted to bring out?—But may I add that the rubric further says, if the certificate is unlimited or limited by time only, the Secretary of State usually imposes a limit on the number of experiments to be performed; and I submit to the Commission that in Certificate A 2 there is no limit to the number of experiments put down.

12919. You think there should not be any such limitation?—May I put it in this way, that there are individual cases in which it is of the utmost importance, from a medico-legal and public health point of view, that the number of experiments should not be limited?

12920. That it should be large perhaps—would not that do? If you put down 500, for instance, do you think there would be any likelihood that the Home Office would override your opinion that that number was necessary?—It seems to me that there would be no advantage in doing that, because each investigator has to return the number of experiments he has made at the end of each year, and in any case the Home Office gets all the information with this advantage, that the operator is not limited as to numbers.

12921. (*Sir Mackenzie Chalmers.*) The existing certificate covers everything; it depends upon the form in which it is granted?—I do not wish to push this, but I never knew until recently that there were any other certificates than A, B, C, D, E and F; but I understand that there is a certificate called A 2.

12922. (*Dr. Gaskell.*) Would you read it?—This is a copy of the certificate which I saw. It is called Certificate A 2. The "2" has been put in in typewriting. It is headed "For Public Institutions." The number of experiments in that certificate is blank, and the time limit is blank.

12923. (*Sir Mackenzie Chalmers.*) To whom was it granted?—I prefer not to say unless the Commission desire it.

12924. (*Mr. Ram.*) Was it from the Home Office?—Yes.

12925. (*Sir Mackenzie Chalmers.*) Not granted by the Home Office; the Home Office do not grant certificates; they are approved by them?—It was not given to Sir Thomas Stevenson, I may say.

12926. (*Sir John MacFadyean.*) Is it possible that the figure "2" is merely to distinguish that particular Certificate A from a previous Certificate A given to the same holder and held at the same time?—That I cannot say.

12927. I suggest that it is. However you are satisfied with that form?—May I read the kind of range of work that is permitted under this certificate?

12928. Certainly?—"Hypodermic intra-venous and intra-peritoneal injection of living cultures of bacteria, bacterial poisons, chemical substances, animal and vegetable extracts, blood and other medicinal poisonous and infective substances. These injections may be repeated at intervals and in various combinations. Immunisation experiments with any of these substances may be performed. Inunction into the skin and administration by the mouth of bacteria animal extracts and vegetable substances. From time to time during the experiments small quantities of blood, peritoneal fluid or other exudation may be removed for examination." That is the range of work covered by that certificate.

12929. (*Sir Mackenzie Chalmers.*) Is that the original certificate given, or is it the certificate as confirmed by the Home Office?—It is a copy sent from the Home Office with the stamp of the Home Office on it.

12930. (*Sir John McFadyean.*) That is precisely the form of certificate you would approve of to be given to heads of departments?—This is the kind of certificate I would like to have, and will apply for, because it seems to me that it is the only kind of certificate that would cover the range of work I am asked officially to do.

12931. You mean that otherwise some case might turn up that you would be really unable to investigate owing to the circumstances not having been precisely set forth in your certificate?—Yes. Supposing, for example, I put in a certificate thinking that I would have a certain number of experiments to perform, and named my animals and omitted to mention a rat, I should not with that limiting certificate be able to do an experiment on a rat without a new certificate.

12932. I wanted to ask you another question with regard to the necessity for animal experimentation in supposed cases of meat poisoning. As you mentioned, Gaertner's bacillus is frequently the offending microbe in these cases. In your opinion is it often or generally quite necessary to resort to experimentation on animals in order to prove that the case is really one of infection with Gaertner's bacillus?—I should not think it needful in every case where Gaertner's bacillus was found in the offending meat where it could be proved.

12933. That is just the point I wanted to raise. Can you tell the Commission whether Gaertner's bacillus can be recognised for a certainty from its morphological characters and the appearance with which it grows in cultures?—Gaertner's bacillus can be differentiated from the other two groups in the Coli series by cultural tests; but there is a series of organisms in the Gaertner group, the enteritidis group, that we cannot differentiate culturally.

12934. So that the answer to my question would be "No"; that in order to be sure that you have got the Gaertner's bacillus you must prove its pathogenic power?—Precisely. I have a long list here of food epidemics caused by micro-organisms to which I could easily add.

12935. I would like to repeat the same question with regard to sausage poisoning, that is to say, the effects produced by the bacillus botulinus. Should I be correct in saying that the bacillus botulinus really cannot be with certainty recognised without animal experimentation?—I quite agree with you in saying that the bacillus botulinus cannot be recognised without animal experimentation.

12936. It operates simply by the poisons which it generates in the meat?—Yes, the toxin does.

12937. So that you cannot prove that a particular parcel of meat is really the cause of illness in this way except by showing that it is capable of producing toxin effects when it is injected into animals?—That is the kind of evidence that I would be inclined to offer if I were asked to give evidence at all with regard to the toxic effect of meat. May I add with regard to Gaertner's bacillus that it is an open question yet whether the toxins are destroyable by boiling temperature or not. The latest investigations show that they are not, and that you might easily get the toxins of this bacillus in meat, there being no organism at all, and still fatal results and a serious outbreak arising.

12938. (*Mr. Ram.*) Although boiled?—Although boiled.

12939. (*Sir John McFadyean.*) Do you see any way of settling whether the poison can be destroyed by heat without animal experimentation?—I think we might easily discover some laboratory method, but it is another thing in practice with regard to public health, because the people take the meat or the soup not knowing the thing is there. If they knew it they would not take it.

12940. Can you suggest some way in which it might be proved that the toxin has been destroyed, without having resort to animal experimentation?—I cannot at the moment suggest any method in the case of meat by which you could destroy its toxin.

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12941. It is not as to how you would destroy it, but as to how you would know it was destroyed?—The only way is by experimentation on animals.

12942. Is anything known whatever about its toxin apart from its effects?—Nothing, except that it is very poisonous, and there is no doubt about it.

12943. With regard to some experiments to which I referred earlier carried out by Dr. Stuart McDonald with regard to cerebro-spinal meningitis, I rather understood you to say that the results of his experiments on monkeys were of little value as showing the way in which the disease is contracted in human beings?—No, as showing the way in which the disease was spread, was my language.

12944. That is the same thing?—I think not. When you are thinking of spread, you are thinking of how the disease may be communicated from place A to place B. Physiologically between the anthropoid ape and the human being, so far as taking the disease goes, there is no difference.

12945. Would you admit that these experiments were of great value as bearing upon the question whether man is infected by ingestion?—Yes, certainly. I was not animadverting on that. I was regarding the spread solely.

12946. Coming to the question of what really led to the introduction of hydrated oxide of iron as the antidote to arsenic, although there were chemical grounds for believing that it would act as an antidote, do you think that the custom of administering it as the antidote in human beings would ever have become universal if we had not been provided with experimental evidence such as you have referred to?—I think it is hardly likely that any man would have ventured to apply this antidote for the first time to a human being before he had seen how the thing would act on animals.

12947. So that you feel confident that these experiments had much to do with making the administration of this particular substance practically universal in cases of arsenical poisoning?—I have no doubt of it at all.

12948. (*Sir Mackenzie Chalmers.*) About how many cases of suspected poisoning do you have in a year in Scotland?—We have not very many.

12949. Half a dozen or a dozen, would you say?—You see we never know until we investigate them whether they are cases of poisoning or not; in many instances we have to make an investigation in the first place before arriving at a conclusion.

12950. Have you ever known of cases where that investigation or conclusion was impeded or delayed from want of a certificate?—No, I do not put that forward.

12951. You are aware that in any case of difficulty, without going to the Home Office, a judge of the Court of Session has power to order any necessary investigation?—That is quite so; I am aware of that.

12952. I take it that your point is this, is it not, that you think that the head of every important laboratory ought to have the wide form of Certificate "A," instead of the restricted form confining him to a specific number of animals and a specific time?—My answer is "Yes," for this reason, that if you take, for example, food poisoning, in which there may be questions of culpability arising, before you can get a certificate from the Home Office, even supposing you follow your suspected meat, your evidence is gone.

12953. You mean that the investigations must be made at once, or they are futile?—They must be made at once if they are to be of any value.

12954. Are there not plenty of people in Scotland holding the necessary certificates to cover any case that has actually arisen?—But it would be no use to me if I had not got it, and I was asked to conduct the investigation; and it is because of the variety of problems one has to deal with as head of the Department that one would like such a certificate.

12955. (*Chairman.*) You said the head of a department and the head of a laboratory; do you mean the head of a laboratory in a public department?—Yes, I mean that. I should say the head of a department, because in my Department of Forensic Medicine and Public Health I have several laboratories under my care.

12956. (*Sir Mackenzie Chalmers.*) You are not aware really whether there has been any difficulty in the head of a department, properly certified, obtaining the necessary certificates?—I do not think so. I think that the Home Office is exceedingly generous. The only difficulty that I have is this, and I would like to put it here, because I may have to salve my conscience and get the Commission, as a Royal Commission can do, to give me an indemnity if I have been wrong. I want to say what I have done. There is great difficulty felt by many of us in being able to determine what really is an experiment under the Act, and there is great difficulty also in defining what is pain under the Act. According to Section 2 a person shall not perform on a living animal any experiment calculated to give pain, except subject to the restrictions imposed by the Act. I want to ask the attention of the Commission for a moment to feeding experiments. It seems to me a ridiculous thing for a man who proposes to feed a cat on cow's milk to have to go to the Home Office for a certificate. I have interpreted the Act differently, and I have acted accordingly, as I shall show immediately. The effect of that on the public mind is this. If that practice is to be followed the total number of experiments at the end of a year is so increased that the public sensitive to this thing think that vivisection is enormously on the increase. I would like to refer in corroboration of what I have said to Questions 205, 206, and 207, which indicate that even in Mr. Byrne's opinion certificates are asked for which, in the opinion of the Home Office, although they would not offer any opinion of that kind, seem to be unnecessary with regard to feeding. Now, three years ago I was asked to deal with the following problem. A series of cows were alleged to have been poisoned with draff, that is, distillers spent grains, on account of minute proportions of arsenic present in the draff and found on analysis. The amount of arsenic found per lb. of draff was between 1-500th and 1-600th of a grain. The people who had to deal with that action had had no opportunity of seeing the animals during the continuance of this so-called poisoning. The animals were dead; they had no opportunity of seeing their bodies at a *post-mortem* examination. I was consulted by the people and asked—What are we to do? I said—It is no good our going to court and simply swearing to a negative, that this amount of arsenic would do cows no harm. The only evidence of any value for the guidance of the court was to give arsenic to cows over a lengthened period, because we knew that the arsenic, to be so given, would do no harm. Accordingly the Principal of a Veterinary College, a Fellow of the Royal Veterinary College, and an examiner of that college, a colleague in the University, and myself agreed that we should feed a series of cows on arsenic doses much larger than these over a period of three weeks. We did not feel that we were under the Act. We knew that the arsenic would do the cows no harm, but would do them good. We selected a bull and two cows, each with sucking calves. We gave each adult animal a quarter of a grain of arsenic three times a day for the first week; we gave them half a grain of arsenic three times a day for the second week, and we gave them a grain of arsenic three times a day for the third week. Thus in three weeks we gave these three adult animals 36¾ grains of arsenic in addition, of course, to the draff as their food, which was blamed as being the cause of the mischief. We weighed the animals at the commencement of the investigation, and their temperature was noted during the currency of the investigation night and morning, and at the end of the experiment every animal had increased in weight. This evidence was given to the court, and the court gave its judgment on that ground; and I hold personally that we were without the Act making that investigation.

12957. (*Dr. Wilson.*) Arsenic is often given to horses?—Yes. Then I may take another case. I am many times consulted with regard to rivers pollution cases in Scotland, and, as the legal members of the Commission will know, Part III. of the Rivers Pollution Act deals with what is called noxious poisoning or polluting liquid. The County Council of Lanarkshire asked me to advise them with regard to an alleged occurrence of this kind, and I made an analysis of the river water above and below the point of effluence of this liquid, but it is impossible to convey to the mind of a court of law whether a thing is noxious or poisonous unless you get some evidence to show that it is noxious or poisonous. I could not ask the court to go and look at the plants in the river, and I did not expect that the court would be able to understand fully the chemical analysis, consequently I did the third best thing. I made an investigation on fish; I carried

through an investigation on fish in the different effluents, and I published the results in the Philosophical Society's Transactions in detail.

12958. (*Sir John McFadyean.*) Were the fish transported from healthy water and put into that water?—Yes, they were; they were got from a hatchery, as a matter of fact. The gold fish were bought at a fishmonger's, and the minnows I got caught in the river. I used gold fish and minnows, one-year-old trout, and trout of two years old.

12959. (*Sir Mackenzie Chalmers.*) Did they all increase in weight and happiness?—No.

12960. (*Mr. Ram.*) Did they all die?—Some of them died, certainly.

12961. (*Sir John McFadyean.*) On what ground do you conclude that this was not an experiment?—I thought that nobody would contend that fish, being cold-blooded animals, could at all be ranked with regard to pain on the same level as warm-blooded animals.

12962. Under the Act, do you mean?—No, it is a question of interpreting the word "pain." A definition of "pain" is what I want to get at. Nobody will define "pain" for us. The Home Office very properly decline to do so. The individual certifier to whom you apply has to do it for himself. I have never been able to look upon fish, from my knowledge of fish as an angler and in other ways, as on the same platform at all as warm-blooded vertebrate animals.

12963. (*Chairman.*) You will not expect us to offer an opinion as to whether this came within the Act or not?—No. I want to be quite frank, and I say that I am mentioning these cases in order to get the protection which the Commission, being a Royal Commission, can give me if I have gone outside the Act.

12964. (*Mr. Ram.*) As I gather, all these experiments of which you have told us as to cerebro-spinal meningitis and as to these noxious gases, all those which you decided to perform or did perform you could perform under the Act as it stands to-day?—Yes, they can all be performed under the Act as it stands to-day. My point is that these questions have relation to asking the Commission not to limit the powers which they presently give us, but in the case of heads of departments to somewhat increase them.

12965. I am coming to that. This evidence of yours, so far as I have alluded to it now, is in order to induce the Commission not to recommend narrowing the powers conferred by the Act to-day?—Precisely, that is the object of the evidence.

12966. The only suggestion that you make as to an alteration of the law is to extend, at any rate to heads of departments, such matters as are dealt with in what you call Certificate A 2?—Yes, that is, of course, where one has to deal with a variety of problems rapidly and very often for the guidance of Courts of Law.

12967. Whether that can be done under the Act as it stands to-day or not. If it cannot be done under the Act you are content if such matters as are described in that Certificate A 2 were given to heads of departments?—Yes, so far as I can see, I think the Act would then be quite excellent.

12968. (*Dr. Gaskell.*) I only just want to ask one further question with regard to that point, as to which I am not quite clear; that is, do you limit the word department to any special department in each place, in each University or town, or would you include in your term heads of departments the heads of all those places that are licensed?—My intention in using the word department was to convey this, and I can illustrate it by a concrete case. In every university there are departments in which experimentation is usually carried on; the Pathology Department, for example; the Forensic Medicine and Public Health Department, and the Pharmacology Department may be taken as types.

12969. And the Physiological Department?—I beg your pardon—the Physiological Department. There are four. There may be under each of these departments several laboratories. You may have assistants, you probably will have them in each of them; you may have research students in some of them. What I desire the Commission to understand by my position is that the head of the department, that is, the professor in charge, should have the fullest facilities, that the assistants should have limited facilities such as now exist under the Home Office, and in the same way research students would have perhaps, to a still more limited degree. The professor at the head of the department ought to be looked upon as the responsible person, who would naturally in his returns let the Home Office know exactly what he was doing.

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12970. That is what I wanted to get at. The head of the department, whom you would entrust with these further powers, might be the head of any one of those various departments, the head of the Physiological Department or the Pharmacological Department, or what not?—Surely.

12971. It is not for the particular Forensic Medicine Department that you ask it?—No. I may say that now the heads of departments are responsible for the conduct of the departments. They would be practically responsible, if, for instance, they had an assistant or a research student in them, because the animal house is under their control and under their charge.

12972. (*Sir Mackenzie Chalmers.*) You would make the head of a department responsible for any misconduct that went on in his laboratory by his assistants?—Surely, so far as he could control it.

12973. (*Mr. Tomkinson.*) In the investigation of poisoning, such as in Lamson's case, which you have to make and in which you say the discoverable amount of poison is so minute as to be hardly traceable when discovered, and used for experiments upon animals, I suppose you would be content with, you would necessarily have to use a very small animal, like a rat?—Yes. May I say that in the cases I have mentioned the difficulty does not commence at the experimentation on the animal? Before you can do that you have to isolate the substance which is to be injected into the animal from the whole mass of these organs, which is a very laborious business. You require to take the internal organs very largely, and get the poison extracted from those first, before you can use it upon animals.

12974. Where does the experimentation previously upon living animals help you to discover that very small and difficult amount to be discovered?—Assuming, for example, that I carry out a series of experiments on that poison, aconitin, which was used in the Lamson case, supposing I use it upon rats and mice I can see exactly the effect produced by the result. If I get in a rat or a mouse from something extracted from a human body which is dead, symptoms corresponding to those same symptoms in the animal, then it is fairly strong confirmatory evidence that I am dealing with the same poison.

12975. You judge by the symptoms?—Yes.

12976. But if the symptoms developed by the animal by a very minute portion of the poison discovered, tally exactly with the symptoms under which the human being died, that is naturally good evidence, but it does not meet your point which you put to me at first when you said that that was not the difficult point. A poison once discovered it is easy enough to test it upon a small animal?—I agree, but the trouble in these cases is getting the poison from the human body to test it upon the animal.

12977. But how are you helped to find that very minute portion?—You are not helped at all, so far as the animal is concerned, in the preliminary stages. You are only helped by the animal at the closing stages of the inquiry, namely, the detection of your poison, which is the important point.

12978. (*Sir John McFadyean.*) I think the question was, where are you to get the poison, which is not necessarily isolated. I suggest that you use as much of the corpse as you can?—I have just been explaining that. You use so much of the internal organs of the body to isolate the poison.

12979. (*Mr. Tomkinson.*) But are you helped to discover that poison by previous experiments on animals?—Surely; you could not make comparisons unless you had made previous experiments on animals with the same poison, and, as I said, assuming that you use aconitin upon rats and mice, knowing that it is aconitin, and you get the development of certain clear symptoms, then, if you get something extracted from a dead body which, when injected into a mouse or a rat, produces the same symptoms you have got strong confirmatory evidence of the fact that the same poison has been used.

12980. (*Dr. Wilson.*) You attach the greatest importance to animal experimentation, both in public health and in criminal cases?—Yes, in certain departments of each of those.

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12981. And you have been engaged in that sort of research for the last 25 years?—Yes, I have been engaged in researches, without experimentation on animals, for the last 25 years.

12982. And without, of course, as you say, having had any licence?—Yes.

12983. Should I be right then in inferring that you have been able to carry out all those researches and investigations, and I know that they are very difficult investigations and researches, without the aid of animal experimentation?—No, I have not been able to carry them all out.

12984. Could you name instances to the contrary?—Food poisoning cases, for example, would have been done either in the Corporation's Bacteriological Institution, which is licensed, or when one has had an investigation to be carried out, one has had to apply to somebody who had a licence and assume it as his own experiment.

12985. But have there been any cases of illness in which you have had to enlist this additional assistance as it were?—No, I have had very few cases. Fortunately, it is only the expert poisoner who would use the alkaloids for the purpose. An ignorant man has not a knowledge of them. He may use strychnia, as happened in a recent case, but then we can detect that by chemical means; we do not require experimentation on animals except with small quantities, and then we use a frog—Marshall Hall's well-known test.

12986. Has it not been proved by recent research that even sound food, during the process of healthy digestion is broken up in part, or in whole, into these poisons that you talk of—ptomaines and alkaloids?—You mean in the living stomach?

12987. Yes, in the living stomach; that that is the first process of healthy digestion and with wholesome food?—Before I could answer that I would require to know exactly what you mean by ptomaines, because the word ptomaine itself implies that it can only be formed in a dead body.

12988. Yes, I know; but as you know, ptomaines are formed, at least it is contended that they are, both ptomaines and leucomaines, by the decomposition of healthy food when it is attacked by micro-organisms. The food is dead, of course, when it gets into the stomach?—Yes.

12989. Then would you not expect on analysis to find a certain amount, at all events, of these deadly alkaloids and poisons in all food that has been kept for some time, whether flesh or fowl?—I do not think that the present stage of science would enable me to give a definite answer to that question. Brieger and others have isolated alkaloids from dead tissues which were poisonous to animals, and there can be no doubt that there is a difficulty sometimes in determining in the examination of a dead body, whether the poisonous principle that is found has been due to the substance swallowed or to the natural result of decomposition of the body. That is the difficulty, and it is in order to enable science to deal with such difficulties that experiments are carried on.

12990. Then if it is possible to find these ptomaines in animal food that has been kept for some time, you would expect to find them in still greater volume in, for example, venison or game, which is always somewhat high, and yet is edible?—I think that all those foods which are eaten high have their special risks.

12991. Because they contain ptomaines?—Yes.

12992. Then with regard to all these food outbreaks, such as from eating sausages, and so on, the poisoning called botulism, is it not possible to ascertain with certainty that all these outbreaks have been caused by the food?—No.

12993. Or by the condition of the food?—No, if you take, for example, Gaertner's bacillus, of which we have been speaking, you may get meat containing this organism which neither by smell nor appearance nor taste would give any indication of the presence of the organism. It is not a putrefactive organism in that sense; it does not cause a nasty smell, it does not make the meat look bad, consequently it may be taken quite innocently or purveyed quite innocently in the belief that it is good food.

12994. Do you say that Gaertner's bacillus is not a putrefactive organism?—Not in the bacteriological sense, as we speak of putrefactive organisms.

12995. I will put the question in this way then. We will suppose that you have isolated these ptomaines, some from the corpse, some from food about which I have been speaking, either game or meat that has been hung for some time, could you then distinguish between the ptomaines that have been obtained from the ordinary healthy butcher's meat or venison and the ptomaines which have been obtained from the decomposition of the dead body?—Do I understand you to mean healthy butcher's meat?

12996. Meat that has been kept for some time?—I would not call it healthy meat if it has been kept till it is high.

12997. I will say venison then, which can be eaten without doing any appreciable injury?—Venison is usually kept until it is fairly high before it is eaten.

12998. Will you assume then that ptomaines can be obtained from ordinary butcher's meat that has been hung and cannot be condemned as unwholesome, and can also be obtained from game or venison?—In the case of all flesh foods, no matter of what kind, if they are kept until they are high, it means simply that they are undergoing putrefaction, and there are risks attending their consumption because of the presence of these toxin or ptomaine substances.

12999. But you could not differentiate by experiments on animals between ptomaines derived, say, from butcher's meat that has been hung, and yet could not be pronounced to be unwholesome, and ptomaines derived from a dead body?—There is this source of differentiation with some of them. It would be correct, I think, to say that many of the toxins formed in the way we have been discussing would be destroyed by a temperature of boiling point, but that some others of them would not be so destroyed, and among these particularly that of Gaertner's bacillus. That is about the only way in which I can determine the difference between them up till now so far as science would enable me to distinguish them.

13000. Is not Gaertner's bacillus—that is to say the bacillus enteritidis—found almost anywhere where there is decaying vegetable or animal matter?—Not by any means always.

13001. But generally?—No, I would not say even generally; it is exceptional; that is the reason why meat poisoning outbreaks are so uncommon, considering the great use of meat in this country as compared with other countries. We do not eat our meat raw in this country; they do largely on the Continent, and if this Gaertner's bacillus be present in meat they are liable to have outbreaks on the Continent, as the evidence shows.

13002. Do you attach the poisoning of food then to the presence of the bacillus?—All the outbreaks which I have been speaking of were believed to be due to this Gaertner's bacillus or one of its allies in the same group.

13003. You have referred to the cerebro-spinal meningitis disease and to the passive carriers of the meningo-coccus. Has it not been proved lately, by Dr. Gordon, one of the experts of the Local Government Board, that very little reliance can be placed upon the examinations which have been hitherto made with regard to the presence of the bacillus in the excretions from the mouth?—The latest work that has been done on that is this: it is quite likely that the witness may be here, but I can anticipate his coming, if he does come, by stating the fact that within the present year an examination has been made individually of 370 contacts, that is to say, persons coming in contact with cases of cerebro-spinal meningitis in Glasgow and the results are as follows:—Of the 370 contacts 22 are to be deducted, for one reason, and in all, including these, 62, leaving for consideration 308 cases, occurring in 74 families. The meningo-coccus was found in 81 of these, or 26.3 per cent., that is to say that in 81 people out of 308 who came in contact with cerebro-spinal meningitis, but who themselves were not affected by it, in their upper air passages the meningo-coccus is proved by bacteriological examination.

13004. But has it not been demonstrated by Dr. Gordon, because I have just been reading his Report to the Local Government Board, that this meningo-coccus has been confused in these passive cases to which you are referring with the ordinary micro-coccus catarrhalis?—No, I tell you that this investigation was intended to bring out the difference between the micro-coccus catarrhalis and the meningo-coccus; there is a

characteristic difference between the micro-coccus catarrhalis, which makes it different at once from the meningo-coccus. The difference is this: if you take a particular medium, that is to say, a material containing neutral red, the meningo-coccus produces an acid reaction; the micro-coccus catarrhalis does not, and that is a very marked distinction. These investigations were based upon that difference, and other differences obtained culturally.

13005. But do not you think that the difficulties attending such research are so enormous that errors are liable to creep in?—Precisely, and it is because we want to eliminate these errors that we hope the Commission will not do anything to prevent our being able to do so, but will give us more facilities.

13006. You think experimentation on animals the final court of appeal on all these points?—Yes, in this respect, that I think we can hardly use human bodies. I think the medical profession and scientific men have not spared themselves in investigations, but there are certain others that we cannot ask them to perform upon themselves.

13007. I will put it in this way. You have been talking about scientific poisoning. Supposing in the Lamson case a needle had been used instead of the poison being given by the mouth, do you think the poison could have been discovered by any process?—It would have made no difference. In either case the aconitin would have been absorbed into the circulation, and have to be recovered from the organs. The mouth administration had nothing to do with it.

13008. Do you attach as much importance to the result of experiments by the mouth as Stevenson did on applying the extract to his lips?—I think that aconitin has such a peculiarly characteristic numbing effect upon the mucous membrane of the lips and the tongue, that nothing else could be mistaken for it.

13009. That of itself was conclusive—the mere testing on the mouse was at the very outside only a slightly confimatory test?—But may I put it in this way: before Dr. Stevenson could apply this test to his lip or his tongue he had to isolate this extract from the organs of that dead man; there were weeks of labour in that alone.

13010. But I mean in testing the extract?—The testing was the easiest part of it.

13011. It was not necessary really to test it on the mouse; was that of any value?—It was of this value that the infinitesimal amount injected into the mouse could produce the symptoms of poisoning by aconitin. A touch on his tongue could not produce them, but it could poison the mouse, and the mouse showed the toxic symptoms, and thus corroborated the test he originally carried out on his own tongue.

13012. But it would not have been considered a conclusive test of itself. By injecting aconitin into a mouse you could not say without any other experiments that the substance was aconitin?—I am afraid I do not follow your question or your reasoning. If I take the substance I extract from a plant in my garden called monkshood, and inject that principle into a mouse, and the mouse shows evidence of poisoning, surely there is no difficulty as to the cause and effect.

13013. But are the symptoms always the same?—Yes, the symptoms generally are the same.

13014. Have not experimenters differed greatly as regards the results that they have obtained by various testings?—Solely because different plants of monkshood contain different proportions of aconitin; that is the only thing. Every plant does not contain the same percentage proportion of toxic material; that is the only difference.

13015. I think you admitted that you do not perform experiments on animals to illustrate your lectures?—No, and I never should. I do not think in my work I should do it. I am speaking personally. I am not saying that other men may not require to do it. I am only saying that so far as I am concerned I should not use animals to illustrate my lectures.

13016. But in illustrating the action of poisons do you think it is necessary, in order to teach the students?—No, I do not think I should do it personally.

13017. Or to illustrate the physiological action of medicines?—I have nothing to do with the physiological action. I am beyond the physiological stage, the toxic stage.

13018. Then, of course, I need not ask you whether you support what is called seropathy right through—serum-therapy?—I am quite convinced that nothing has made such a vast alteration in the death rate from diphtheria as the serum treatment of diphtheria. I do not think that any medicinal treatment could have been devised which would ever have effected what this serum treatment has done for diphtheria.

13019. You think it can be regarded as a sort of sheet anchor now in the treatment of the disease?—I have not the remotest doubt of it.

13020. What about serum therapy in cerebro-spinal meningitis?—That has not been so uniformly successful; the anti-meningo-coccic serum has not been so successful.

13021. Has it been successful at all?—In some cases there has been a cure, but I do not know whether you can say it is cause and effect quite so positively as in the case of the serum anti-toxin in diphtheria.

13022. But the anti-diphtheritic serum is the only serum that has been attended with what you might call success?—If you are dealing with the preventive material used in the prevention of anthrax then that has proved an enormous benefit to the world.

13023. I am speaking of sera?—I claim that about anti-toxin.

13024. (*Dr. Gaskell.*) May I ask one question with respect to what you said just now. Do not you think it is advisable for students to see, for instance, the action of strychnia. When you say that you would not show students any experiments, would you not show them experiments in which the animal was completely under anæsthesia?—I must qualify my last answer by the fact that if the animal was under chloroform then the experiment would be of no use.

13025. Not necessarily, might it not be with certain alkaloids?—Chloroform would mask the action of alkaloids you see. The only treatment for strychnia poisoning is chloroform.

13026. Naturally. I was not speaking of strychnia then; I was speaking generally. As regards the action of digitalin, for instance, or many other poisons, would you not show experiments?—You could not show an experiment on digitalin with an animal under chloroform; it would obviate the effect. The only experiment in a criminal case where digitalin was suspected of having been the poison was a case occurring in France; the animals used there were frogs without any anæsthetic at all. That was Tardieu's case. That is the only case in which an experiment with digitalin could be conducted in order to show true results.

13027. What I want to get clear about is this, that the experiments showing the action of various alkaloids and so on you would rather leave to the Pharmacological Laboratory?—That is so.

13028. That is what I understand. It has not come into your department as teacher of forensic medicine? No, because the doses that I give are toxic—fatal.

13029. (*Dr. Wilson.*) I think I gather from what you have just said that anæsthetics interfere very considerably with the results which you would otherwise obtain from the administration of many of these drugs and poisons?—Poisons not drugs. If I want to see the true natural action of a poison I do not want to administer another substance which may act poisonously at the same time and confuse the results.

THIRTIETH DAY.

Tuesday, 29th October 1907.

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. STEPHEN FRANCIS SMITH, M.R.C.S., called in; and Examined.

Mr. S. F. Smith. M.R.C.S. 29 Oct. 1907.

13030. (*Chairman.*) You are a member of the Royal College of Surgeons?—Yes.

13031. And you are Surgeon to the Anti-Vivisection Hospital?—Yes.

13032. And author of two works on vivisection, "Scientific Research" and "Fruitless Experiments"?—Yes. I may mention also that I am a member of the International Medical Anti-Vivisection Association.

13033. I do not quite understand what the Anti-Vivisection Hospital is. I do not understand its title. Does it mean that no operation is performed there upon animals—that there is no laboratory or anything of that kind; or does it mean more than that?—Primarily it is a hospital; secondarily it has been founded by anti-vivisectors, who took that opportunity to make a kind of public protest against vivisection by the name which they gave to the hospital.

13034. That is to say, it is supported by anti-vivisectors. That is practically what it means?—Yes.

13035. In the treatment of patients you use the usual remedies, I suppose, used in other hospitals?—We do not bar anything that will do the patient any good.

13036. You do your best to cure your patient with whatever remedy is most suitable?—Yes.

13037. That, I suppose, applies both to surgery and to medicine?—Yes.

13038. You have formed an opinion as to the amount of sensibility of animals to pain as compared with man, I think?—I should like to mention first the evidence from special senses in the lower animals. We find that the sense of smell is stronger in many of the lower animals than in man; it is well known to be the case in dogs; and from travellers' accounts of hunting game the scent is stronger in them. I have read in many travellers' accounts of travels in South Africa that their oxen would smell water at a long distance, and very much to the surprise of the travellers. As regards sight, it is well known that vultures can see at an enormous distance, and although that may depend partly on the structure of the eye, yet without a highly developed sensitiveness in the retina, and in the brain centre to which the visual impulses are conducted, that structure of the eye would be comparatively useless. As regards hearing, I have found in my own dog that it has a very remarkable power of hearing, far greater than that of any human being. What I wish to point out is, and I am going to show presently, that there is a widespread belief that sensibility to pain depends upon intelligence; but here I simply wish to indicate that if that theory were correct it would prove that the lower animals could not smell, hear, and see as they do. Next I wish to say something about the teaching of biology and evolution. Wherever a sense or structure is necessary to the preservation of the animal, that sense or structure will be highly developed. Pain has a function. The greater the sensibility to pain, the more care will the animal take to avoid danger and preserve life. Pain is a more important factor to the lower animals than to man. Man, inferior in the special senses which warn him of danger, inferior in strength, speed, and natural weapons to many of the lower animals, makes up for these deficiencies by intelligence. Similarly, his intelligence guards him to some extent against danger, and there is less need for acute sense of pain. We have also the teaching of psychology. There is a popular belief that pain varies with, and depends on, intelligence. This view is contradicted by the evidence of the special senses I have mentioned, and lands us in the absurdity of having to believe that a man can smell more keenly than a dog. It is contradicted also by psychology, which tells us that the higher thoughts and feelings are due to the compounding and recompounding of the simple sensations, while the popular view, on the contrary, is that the simple comes from the complex. I wish to point out that the only right principles on which we can form a conclusion as to pain which the lower animals feel is by taking into consideration the surroundings of the animal; that is, if pain is necessary to the survival of the animal we may conclude by all analogy of evolution that the sense of pain will be highly developed.

13039. I should just like to follow what you mean by the evidence of the special senses. There are some savage races which have a much acuter sense of smell and sight than some civilised races, are there not?—Probably, yes.

13040. Do you think that those savages have necessarily a more acute sense of pain?—Not necessarily. But in the case of a savage there may be something in the surroundings of the savage which make it necessary that the sense of sight should be highly developed, and possibly, though I do not know, about the sense of smell. But it is said that Arabs have long sight, and it may be necessary, or was necessary in the ancestor of the Arab, that he should have long sight in order to see an enemy at a long distance; I would simply point out that his sight was modified to his conditions of life; but I was specially comparing these evidences of special senses with the belief that sensation is proportioned to the intelligence.

13041. Would you say that of different races of men? Would you say that the sensation of the negro in West Africa is more acute than that of a civilised man or as acute?—It might be very difficult to judge directly; but if pain has been necessary to the preservation of the negro I should say that it would be as well developed in him as in civilised man.

13042. You would not say that such a negro in his wilds was equal in intelligence to a cultivated white man in England?—No, I should think not.

13043. But you think his sense of pain would be the same?—I should think so.

13044. (*Sir Mackenzie Chalmers.*) Greatly increased, ought it not to be?—It ought to be greater.

13045. (*Chairman.*) You think it is greater?—I should think probably it is greater. But I should like to mention my own experience as regards different classes of people. In my occupation it has been my business to inflict a good deal of small pain without anæsthetics on patients, and I have never been able to observe any difference between my hospital patients and private patients. I spoke to a dentist—a dental surgeon to a hospital—and asked him his opinion, and he said he thought there was no difference—that the only difference he could see was that hospital patients gave greater manifestations of pain.

13046. I was interrupting you rather. Have you said all that you have to say on that question?—I was going on to say that it is obvious that if this untenable belief is held by physiologists it is a danger to animals under experiment. I therefore give evidence that such belief is held. Some years ago I received some literature from the Secretary of the Association for the Advancement of Medicine by Research. In it was a pamphlet by an anonymous writer, maintaining that the lower animals are less susceptible to pain by reason of their less intelligence. I was for some time at the Pasteur Institute in Paris. I observed laparotomy (opening of the abdomen) performed on rabbits without anæsthetics. I spoke to an American medical man who had been for some years in the Pasteur Institute, and asked him why an anæsthetic was not given, and he told me it was because the rabbit did not mind the operation. He was not making a joke; that was his genuine opinion. I give the opinions of some English physiologists, Professor Huxley and Dr. Anthony, who, at the last Royal Commission, maintained at Questions 2547 to 2560, that animals do not feel pain as we do, and that their struggles are mere automatic or reflex movements. Similar opinions at the same Commission were given by Dr. Pavy and Dr. Pye-Smith.

13047. Do I correctly understand you to say that Professor Huxley said that all struggles by animals, when painful operations were being inflicted upon them, were merely reflex movements?—I think it was left somewhat indefinite. I do not think he said all.

13048. I see he was asked: "So that an operation which is constantly performed on sows in order to get them to fatten a little faster is an operation decidedly more painful than that which you say is carried on for scientific purposes (that is spaying)," and the answer is, "Yes." That is Dr. Anthony giving evidence and Professor Huxley asking the questions?—Yes, he was asking leading questions.

13049. I will not read through the whole of the questions and answers that you refer to, but perhaps some other member of the Commission will have an opportunity of examining you upon them. That is what you say is the effect of his evidence?—Yes. I was going on to say that similar opinions at the same Commission were given by Dr. Pavy, Dr. Pye-Smith, Dr. Burden Sanderson, Dr. Crichton Browne, Mr. Schäfer, Mr. (now Lord) Lister, Professor Humphrey, Dr. McDonnell, Dr. Cleland, Dr. Sibson, Mr. George Henry Lewes, and, in the *Nineteenth Century* of December, 1881, by Sir James Paget.

13050. And all these gentlemen, you say, were of opinion that animals feel less?—Yes.

13051. (*Sir Mackenzie Chalmers.*) Do they give reasons?—No, I think not.*

13052. (*Chairman.*) I suppose they were asked questions about it, were they not?—I do not think they were. Dr. Cleland said that it was doubtful if frogs feel at all, and would therefore no more think of anæsthetising a frog for an experiment than a worm for fishing. (That is at Questions 4615 and 4616.) It was admitted by Dr. Sibson (Question 4752) that he had no ground for believing curare an anæsthetic, but had employed it as such in painful experiments.

13053. As to curare, I think everybody who has been before us has admitted that curare is not an anæsthetic?—That is not the point that I am referring to at present.

13054. It is what you were reading?—Yes, but it has reference to what I am going to say in a moment. There were similar admissions from Dr. Rutherford (Questions 2918 to 2935). This is all on the point of their belief. I am now going on to some fresh matter. Here is an experiment described by Dr. Rutherford at Questions 2918 to 2934, with his opinion and the opinion of Dr. Sibson: "A dog was starved for 18 hours; it was paralysed by curare through a cut in the throat, and artificial respiration was kept up. The abdomen was cut open and a tube inserted into the bile duct. The animal was kept in this condition eight hours, substances being frequently injected into the bowels. No anæsthetic was given because, in Dr. Rutherford's words, the "result of the experiment might be interfered with." He expresses his opinion of this operation that the pain was trivial.

13055. What was the date of it?—I do not know that any date was given. I have quoted the question in the last report. This opinion was endorsed by Dr. Sibson at Question 4760, because the animal was kept quiet by curare. Dr. Sibson's words are so remarkable that I give them: "There would be no struggling, the incisions would be made with great ease, the animal's mind would be withdrawn from what I would call the domain of attention, and when there is no attention there is no sensation, no sensitiveness, no pain." There are many more similar opinions in the Report of the last Royal Commission. At the present Commission the Inspector for Scotland (Question 750), expressed the opinion that pain varied as the intelligence, and when in Question 741 he was confronted with the evidence of the special senses, he made the curious and ambiguous statement that it is not every kind of dog that is good at smelling. He concluded that this evidence of special senses showed less susceptibility to pain. I give two more proofs of this wide-spread belief. In Mr. Keetley's "Index of Surgery," pages 180 and 181, second edition, is the operation for hare-lip in children. I quote, "Chloroform unnecessary. Secure his limbs by rolling him up tightly but firmly in a towel. If desired anæsthetic vapour may be pumped through a catheter." Then comes a description of the operation for which chloroform is unnecessary: "Begin by separating with the knife the two sides of the lip from the jaw subjacent. . . . Then pare the edges of the cleft. Remove enough. . . . In double hare-lip the intermaxillary nodule is pared"——

13056. I do not quite see how showing that certain operations were performed without anæsthetics, and were justified as being performed without anæsthetics, as I understand, before 1876, is material here, because we have now the Act of 1876, and what we have to inquire into is as to the desirability of amending it. Nobody has suggested to us that it should be recommended to dispense with anæsthetics?—That is not the point that I am trying to bring out at all. I am on one point only. That one point is that there is a wide-spread belief that pain varies as intelligence varies. That is the only point that at present I am trying to bring out.

13057. I think you have done that already by telling us the evidence of some eight or ten eminent medical men who all pronounce that opinion, and I think that opinion, no doubt, does prevail?—Then I will not pursue that subject further.

13058. That experiment is not on animals, it is on a child?—I know; but it has the same inference.

13059. (*Sir Mackenzie Chalmers.*) The inference being that the child feels as much as a grown-up being, although less intelligent?—Yes, but that this surgeon, at all events, did not believe it.

13060. Was he speaking solely with regard to children, or did he think the operation for hare-lip on anybody could be done without anæsthetics?—There is nothing in the book to reply to that question.

13061. (*Chairman.*) I really do not think you need labour that question about what the general opinion is, because, as I say, you have given already the evidence of a great many medical men thirty years ago;

* *Some of them gave reasons.—S. F. S.*

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and I think that is the general view now. I am not speaking of that as a scientific matter myself, but as a general opinion?—Then I will pass over it. I wish to point out, however, that there are certain reasons why this belief is stronger in physiologists than it is in other people. This belief in physiologists is strengthened by two circumstances: First, there is the blunting of the sensibilities. I wish to give my own experience about that. The first time I saw a brutal experiment on an unanæsthetised animal I wished to leave the room—I was sickened by it. The next time I was less affected, and with every experiment I was still less affected, until eventually I was able to look on at the most terrible things without my emotions being moved in any way, and I had left only my intelligence to tell me that the animal was being tortured. And I submit that what occurred in my own case probably occurs to everybody who witnesses an experiment. Now English vivisectors go abroad. These experiments to which I am referring are continental experiments. That they go abroad I know from my own personal experience. Professor Starling told me that he spent some months at the Pasteur Institute; Professor Heger, of Brussels, told me that Sir Michael Foster spent some months there; and I have heard one or two other Englishmen say that they have vivisected abroad. But besides the blunting of the feelings from often witnessing experiments, there is another circumstance which I think deceives the experimenter. Animals which do not often use their vocal chords frequently do not give the manifestations of pain when under severe suffering. Of course, everybody knows that a horse, which does not use its vocal chord often, will endure severe flogging without making a sound, and similar things occur with rabbits. There is one other circumstance, too, which I think deceives the experimenters, and that is that I believe when an animal is under very severe torture it may be to a certain extent paralysed. Mantegazza, in his work on pain, described how he kept animals quiet when he was subjecting them to torture. He said there were two ways; one was by nailing them down by their feet, so that he seriously increased their pain with movement. Another means was by making the pain so severe that the animal was paralysed.

13062. When was this book written?—It was written a long time ago.

13063. Fifty years ago?—No, less than that. I could not tell you the exact date.

13064. What country did the gentleman who wrote it belong to?—Italy. The only point I am trying to bring out is that physiologists may be deceived. In experiments on rabbits, for instance, the severity of the pain may possibly paralyse the rabbit and it may not give the manifestations of pain, and therefore the experimenter may be deceived by that. That is the only point I am trying to bring out. That is all I have to say on that point.

13065. Then the next point I think you want to speak about is the evidence of pain inflicted by English experimenters?—Yes. As to direct evidence, we have nothing but what experimenters disclose. From an analysis of every experiment published in the "Journal of Physiology" from 1896 to 1900, inclusive, I have found that 71 per cent. were cutting experiments.

13066. What was the gross number of experiments in those five years?—I could not tell you. It was not very great. I have simply mentioned that I referred to every experiment in the "Journal of Physiology" for five years. This percentage refers to the nature of the experiments only, for in many cases these were repeated on numerous animals, the precise number of which is not stated. Of these cutting experiments, the animals were not killed at the end of the experiment in 31 per cent.

13067. Is that 31 per cent. of the whole or of the 71?—Of the 71 per cent.

13068. 31 per cent. of the 71?—Yes. The period for which they were kept alive afterwards varied from days to months. In the great majority of these cases I do not think there is much suffering, judging by the analogy of patients recovering from operations, but I think there is some. I am well qualified to state what may be felt after an operation from a personal experience of last year. After an operation for varicose veins one of the wounds, about an inch long, inflamed; there were some pent-up discharges, and the pain I suffered from them was very violent, I might call it almost unbearable. I was obliged to drug myself heavily with morphia. I also wish to point out that it has been said that these operations are done with precautions as to surgical cleanliness, but in an animal there may be very great difficulties in doing it. We have first to assume that the experimenter will always take the trouble to shave away the fur from the animal—that he would not be tempted to omit that. Also an animal will remove its dressings; it tears them off or rubs them off. It is very doubtful whether the wound could be kept surgically clean for that reason. There is another disclosure in the "Journal of Physiology," that in the cutting experiments curare was used in 10.3 per cent. That refers to those in the "Journal of Physiology" for the five years that I have mentioned.

13069. Can you say what particular class of experiment it was in which the curare was used?—I cannot. It is some years since I made this analysis.

13070. Can you say who it was?—I cannot. All I have left is my notes.

13071. (*Dr. Gaskell.*) Instead of saying 10.3 per cent. could you say the actual number of cases in which curare was used?—I cannot, but one can easily find it out by going to the "Journal of Physiology" for those years.

13072. (*Mr. Ram.*) Do you mean used without anæsthetics?—No.

13073. In addition to anæsthetics?—In addition to anæsthetics.

13074. (*Sir Mackenzie Chalmers.*) How many hundred cases altogether would be included. Would it be something like 200 or 300?—No, there would be a less number of experiments made. I cannot tell you the number of animals experimented on, but I do not think there were 100 in the whole five years.

13075. (*Dr. Gaskell.*) Have you a list of the names of the experimenters you are referring to?—No.

13076. (*Sir Mackenzie Chalmers.*) So that these percentages are calculated on a single 100 or less?—They are calculated on as many experiments as are mentioned in the "Journal of Physiology" for the five years.

13077. (*Mr. Ram.*) It may be less than 100 in the whole time?—Yes, I think so.

13078. (*Chairman.*) Have you any observation to make about the use of curare with anæsthetics?—Yes. I think I may pass over the physiological evidence that curare is not an anæsthetic; it is admitted.

13079. Yes?—But that is denied on the strength of one experiment which is in Mr. Paget's book of a servant girl who was poisoned by an arrow in her master's hall and became unconscious and did not feel pain. I think I might refer to that, because it is mentioned in Mr. Paget's book, and it is stated as evidence that curare is an anæsthetic. What I wish to point out regarding that case is, firstly, that there was no positive evidence that the poison was curare; secondly, there is no evidence that artificial respiration was either prompt or efficient. If it was not prompt or if it was not efficient, the asphyxia would account for the unconsciousness. The only authentic account of experiment on man is given by Liouville in the "Bulletin Général Therapeutique," 1865, page 404. An overdose had been given and artificial respiration had to be kept up until the patient recovered. Liouville adds: "The patient then related all he had felt, the preservation of his intellect, the annihilation of all power of movement, of which he gave a clear account, witnessing all that went on around him without being able to take any part in it, the fears expressed by some assistants present being by no means reassuring." Claude Bernard believed that curare greatly intensified the susceptibility to pain, and in dramatic language has stated that under it he inflicted the most atrocious suffering that the mind of man can conceive. That he was correct there is every reason to believe. The afferent impulse has no escape along the motor channels. Herbert Spencer has in

several places emphasised the fact that an impulse is lessened by escape through surrounding channels.

13080. (*Dr. Gaskell.*) Have you got the quotation of Claude Bernard, where he says that it intensifies pain?—I have not. It is such a well-known quotation and has been so often made that I did not think it would be questioned.

13081. It is the well-known quotation that you are referring to?—Yes.

(*Sir John McFadyean.*) Would it not be easier to find the references if it is so well-known?

(*Sir William Collins.*) At page 282 of Mr. Paget's book I think a reference is given.

13082. (*Chairman.*) That is hardly a quotation, and very likely does not give the whole of what Claude Bernard said. Mr. Paget wrote, "Claude Bernard believed that it did not in any way affect the sensory nerves, and he described in theatrical terms the animal as being unable to stir, but suffering horrible torture." That is an extract from the "Edinburgh Review," for July, 1899. It does not profess to give the exact words beyond that. Perhaps you will go on with your evidence?—I assert that every time curare is given to an animal there is danger of most terrible suffering being inflicted. I think it is certain that in the majority of cases, possibly in every case, the most appalling torture is perpetrated. My reasons are as follows. In giving anæsthetics to human beings, great care is necessary to give enough to produce unconsciousness and to avoid giving more than is required, so endangering life. There are certain indications as a guide. After a certain time absence of the corneal reflex shows that the anæsthetic may be stopped. Reappearance of the corneal reflex shows that more may be given. A slight movement of the patient (which, however, rarely happens with a skilled anæsthetist) shows that more must be given at once. With these guides in human patients, anæsthesia is safe and efficient, although the quantity of anæsthetic required by one patient may differ considerably from what may be required by another similar patient. In a curarised animal these guides are totally absent. Moreover, there are no means of knowing the idiosyncrasy of the animal as in human patients. My experience is that one dog requires more anæsthetic than another of the same size. Further, my experience is that to give just the right amount to a dog, that is, to give it enough for anæsthesia, without killing it, is more difficult than in human beings. My belief is, therefore, that if ever a curarised dog is properly anæsthetised it is by a lucky chance. I do not think there is the consolation of believing that in these curare cases with incomplete anæsthesia (I am presuming there is incomplete anæsthesia) the ether or chloroform given lessens the pain, as would be the case with morphia. I believe, that is to say, that when such anæsthetics as ether, chloroform, and gas are given, the dividing line between complete anæsthesia and complete consciousness to pain is a very sharp one. I have only two proofs to give of this. One is an operation on myself. I was given gas by an experienced anæsthetist. A hole was bored in my jaw, an operation of a few seconds. I felt an inability to move, and pain which appeared to me almost as bad as any pain could be. The second case was that of an intelligent man whom I anæsthetised. I had given him a considerable amount of A.C.E. (alcohol, chloroform, and ether). I believed his corneal reflex was absent. I told the surgeon he might begin. An incision was made. The patient struggled, and the surgeon had to wait. I questioned the patient afterwards, and he gave as his opinion that he could not have felt the pain more if he had had no anæsthetic at all.

13083. (*Sir Mackenzie Chalmers.*) What operation was that?—It was an operation for the removal of suppurating glands.

13084. (*Chairman.*) But in the case of a skilled anæsthetist, has he not sufficient guide, although the subject may be curarised, for seeing whether he is still under the influence of the anæsthetic?—The indications are all absent; there is no indication.

13085. That is what I wanted to know. Are they all absent?—They are all absent.

13086. There is nothing by which he can tell whether pain is felt?—Absolutely nothing.

13087. Is that the view of all anæsthetists?—Anæsthetists have nothing to do with curarised animals.

13088. But under the Act the anæsthetist is obliged to keep the animal under the anæsthetic to prevent pain; then it is his business to see that the animal is kept free from pain. The operators tell us that when curare is administered it is not with the view of enabling them to be careless about the anæsthetic, but with the view of obtaining absolute stillness and absence of reflex movements. Supposing that that is their object, namely, to keep the animal under anæsthetics while at the same time it is rendered absolutely still by curare, can they not discover whether the animal is fully under anæsthetics?—I have just pointed out that the only guides which they have which are present in an animal without curare are absent in the case of a curarised animal.

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13089. Could the anæsthetist not tell by the eye or breathing, or anything else?—No.

13090. (*Sir John McFadyean.*) Are all these statements based on your own personal observation of attempts to anæsthetise animals that are curarised; have you ever seen this thing done?—Yes.

13091. (*Chairman.*) I am speaking with some doubt as to my memory, because it is some long time ago, but I think we have had skilled anæsthetists before us who have not come to the same opinion as yourself on that matter, and that is the reason why I asked you the question?—It seems to me obvious, because all you have got is the reflex of the cornea and the movement; that is all you have got in a human patient to guide you; in a human patient you have absolutely nothing else.

13092. However, that is clearly your opinion?—Yes.

13093. (*Sir William Collins.*) Does anyone maintain that the reflexes can be used as a guide in the case of a curarised animal?—No, nobody would maintain such a thing; it would be absurd.

13094. (*Chairman.*) I am not pledging myself as to what previous witnesses have said. I was rather under the impression that they had said that you could distinguish?—I am quite certain that no witness could have said that the corneal reflex is present.

13095. No; I used a very wide expression—whether there was anything that would give them the information?—Something was said about blood pressure, I believe, by one witness, but that is a point on which you want a great deal of corroboration of evidence.

13096. Or as to the quantity of anæsthetic that is being administered; would that be a guide?—I have already pointed out that the amount of anæsthetic that is required by one animal differs from the amount that is required by another animal of the same size. That has been my experience. If you only give enough to keep the animal alive you are in great danger of not having anæsthesia, and if you give sufficient to be more than certain that the animal is anæsthetised you will be in great danger of killing the animal.

13097. But that risk has to be run?—That risk has to be run; but it would depend upon the physiologists to say that they kill a great many animals when they are under curare.

13098. Is there anything more you wish to say upon that subject?—I think that is all I have to say about it.

13099. Then you wish to say something about the usefulness or not of vivisection?—I wish to state, first, that the reason why I am selecting certain claims is that when this book, "Experiments on Animals," was published by the Secretary of the Association for the Advancement of Medicine by Research, a number of medical men, including myself, were requested to inquire into the accuracy of the statements made therein. The work was split up, and what I am giving you now is what fell to my share. Firstly, on drugs. I am giving only a small part of the evidence which exists. I give certain facts first, and the inference to be drawn I shall place at the end. I may state that I am giving now only those drugs which begin with the letter A. I shall not go further than the letter A for the sake of brevity. I begin with alkalis. Referring to the experiments on the lower animals, Dr. Ringer says ("Therapeutics," page 127, fifth edition) that the results are contrary to experience of the action on the human body, and he indicates further that the animal experiments are in them-

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selves contradictory. As regards acetate of ammonia, Stillé and Maisch ("National Dispensatory," page 836) point out that, given to healthy men, there are no decided symptoms; but that in rabbits there are fatal tetanic spasms and dissolution of the gastric mucous membrane. Amyl nitrite was discovered by the French chemist Balard in 1844. Later, Professor Guthrie, when distilling it, found it producing flushing of the face. The natural deduction is that the blood pressure would be lowered if the small arteries are dilated. Still later, animal experiments demonstrated that the blood pressure is lowered, but this was only corroboration of clinical observation. Apomorphia: The clinical effect of this drug on the heart is flatly contradicted by the animal experiments of Siebert and Moerz ("Bartholow's Materia Medica," page 459), and Wood (page 437) points out further discrepancy between human and physiological investigation. I pass over as of less importance, though interesting, the various contradictions between physiologists themselves as to the action of this drug. Arsenic: Drs. Ringer and Murrell ("Journal of Physiology," I., page 217) obtained on frogs different results from those of Dr. Sklarck, of Berlin. They believed the discrepancy to depend on the time of year. I prefer to leave it to a physiologist to answer the problem. If arsenic has a certain effect on frogs in May, what effect would it have on man in March? The absurdity of these experiments is as obvious as the unconscious humour of the experimenters. Atropine (obtained from belladonna): Its well-known action of dilating the pupil in man is absent in birds (Stillé). Birds and herbivorous animals eat belladonna with impunity. "This is one of the many examples," remarks Stillé and Maisch (page 276), "which show the danger of concluding from the lower animals to man in regard to the uses of medicines." I pass over the disputes among physiologists as to the effects of this drug upon animals. I observed this morning that I have left out the drug aconite, which begins with "A," about which similar observations could be made; but for the sake of brevity I state briefly that similar observations can be made concerning the following:—Benzoic acid, bronchitis, caffein, Calebar bean, camphor, carbonic acid gas, chloral, chrysophanic acid, citric acid, coculus indicus.

13100. When you say similar observations, what do you mean precisely—observations that they have not the same effect on man as on animals?—Yes, and that there have been diverse views among physiologists themselves as to the effects of these drugs.

13101. You say that you cannot argue from experiments on animals as to the effect on man in the case of these drugs?—No. Colocynth, conium, sulphate of copper, mercury, croton chloral hydrate, croton oil, coriaria, elaterium, muscarine, gamboge, glycerine, gelseminum, ground ivy (which is glechoma), guarana, hamamelis, hyoscyamis, ipecacuahana, jaborandi, lead, milk (when injected into veins), kalmia latifolia, musk, nitrate of silver, nitropentane, opium, phosphoric acid, podophyllin, rhus toxicodendron, prussic acid, quinine, ergot, sanguinaria, sarsaparilla, senega, soda, sow-bread (cyclamen), cantharides.

13102. Take prussic acid, for instance, is not that fatal both to animals and man?—Yes. I have not said that all the properties of drugs differ. I have only said that there are differences.

13103. What is the difference in the operation on man and animals in the case of prussic acid?—I have not got my notes here at present. I have only given you the particulars as to certain drugs. I simply mention that there is evidence regarding all the remaining drugs for the sake of brevity. If the Commission wished to listen to it all I should be pleased to have spoken for an hour on all these drugs.

13104. We do not want to hear about them all. I would suggest that you might have taken half a dozen of the greatest cases; but prussic acid is a poison that is so commonly known, and is known to be used so often for killing animals, that I thought I would ask you about it, as it caught my ear?—A drug may have common properties; for example, it may kill both; but it may also have some properties which are different. Prussic acid is not used as a medicine in man for killing; it is used for other purposes. You may have a drug which may have some properties in common and yet will have properties which differ.

13105. But you are not at the present moment, without your notes, able to say what the particular difference is in the operation of prussic acid on man and on animals?—No, I am not. Then there are squill, stramonium, strychnine, tartar emetic, thein, coriaria sarmentosa, tobacco, trimethylamine, veratria, and solanum dulcamara. An account of one drug not mentioned—namely, digitalis—is so important that it should be given. I do so because it is so often stated that the action of this drug was discovered by experiments on animals. "

13106. Some effects of it. I do not think that anybody has come to say that its use as a drug was not known independently of animal experimentation; but I think what has been contended is that it was discovered to have properties and effects on particular parts of the body which were not understood without experiments on animals?—Then, in that case, if the Commission is of that opinion, I will pass over that. The evidence is overwhelming that whether with old drugs or new, any conclusion from experiments on animals are absolutely untrustworthy, and would be dangerous if relied on. Mr. Berdoe, in his brochure on drugs, puts this clearly. He says: "Let us imagine that a quantity of a new drug, called opium, is being examined for the first time by a special committee. Let us assume that nobody knows anything about its properties except some vague travellers' tales. The physiologists proceed to investigate its action by a long series of experiments on animals. They give it to frogs, and they find that small doses throw them into tetanic spasms. They give 20 grains to a pigeon, which is none the worse for it; 30 grains are given to a rabbit with no effect. They are beginning to think the travellers' tales exaggerations, especially as they discover that ducks and chickens, like the pigeons and rabbits, are never the worse for its administration. They resolve now to try it on a human patient, and proceeding with extreme caution, as they think, they decide not to venture at first beyond the dose they gave to the pigeon—namely, 20 grains. The patient is a powerful navvy. He is promptly killed. If physiological medicine were of any value, surely the method followed by these investigators was right and cautious. Yet how fatal their method when reduced to practice."

13107. Are those physiological facts, that opium, for example, does not operate on pigeons and rabbits and frogs?—Yes. I wish to compare these facts with the statement of Sir Lauder Brunton, at Questions 6929 and 6930, before the present Commission, that animal experiments are absolutely necessary to learn the effects on man, and to learn, in Sir Lauder Brunton's own language, "a safe dose." I would draw attention also to his statements at Questions 6893 to 6897. They are that Gamgee's animal experiments led to the observation that the pulse tension in man is lowered by nitrite of amyl, and that this observation pointed to the treatment of angina pectoris by nitrite of amyl. This may be, but it was previously known that nitrite of amyl caused flushing. The deduction that the pulse tension would be found lessened is so glaringly obvious that if it was really overlooked great want of commonsense was shown. Gamgee's experiments should have been unnecessary. That is all I have to say about drugs.

13108. Then I think you desire to make some observations upon the claims in favour of the value of vivisection, or rather, I should say, of experiments on animals, because some are not cutting experiments, made by Mr. Stephen Paget in his work "Experiments on Animals"?—Yes; the action of drugs, of course, is part of that.

13109. And I think you take first the case of inoculation in typhoid fever?—In what I am going to say now I am only stating what I have published. Dr. Washbourn, Physician to Guy's Hospital, writes ("British Medical Journal," April 20th, 1901): "With regard to the value of inoculation, I am satisfied, from clinical observation, that it does not modify the disease. Mild, severe and fatal cases occur among the inoculated and non-inoculated, and, as far as one can judge, with the same frequency. Whether inoculation diminishes the incidence of enteric fever can only be determined by extensive statistics. From my own personal experience I should not think that the incidence is diminished." Dr. Melville, late civil surgeon to the Natal Field Force, writes ("British

Medical Journal," April 20th, 1901): "Inoculation.—Of my 295 cases, 30 were inoculated, the remaining 265 being unprotected in this manner. A few statistics comparing the results obtained in the two classes of cases are not devoid of interest. From the following table it will be observed that the uninoculated cases compare very favourably with the inoculated. The complications were more numerous, the duration of the fever longer, and the death rate higher in the inoculated." The table is as follows:—

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	Complications.		Deaths.			
	Number of Cases.	Number of Cases.	Percentage.	Duration of Fever Days.	Number of Deaths.	Percentage.
Inoculated	30	12	40·00	29·57	2	6·67
Uninoculated	265	55	20·75	21·38	5	1·89
Total	295	67	22·71	22 21	7	2·38

That table shows that the percentage of deaths in inoculated cases was 6.67, and in the uninoculated, 1.89.

13110. (*Sir William Church.*) These figures which you are quoting are the experiences of those two gentlemen in South Africa?—Yes.

13111. They are not in any way a table of figures compiled to show the effect of inoculation or non-inoculation scientifically; they are merely a report of just such cases as happened to come before them during the South African War?—That is all. I have another table here which sets forth the complications met with in the uninoculated cases compared with those met with in the inoculated.

	Relapse.	Malaria.	Pneumonia.	Hæmorrhage.	Thrombosis.	Cerebral.	Peritonitis.	Erysipelas.	Total.
Inoculated	5	2	2	1	1	1	–	–	12
Uninoculated	18	3	9	16	4	1	3	1	55
Total	23	5	11	17	5	2	3	1	67

13112. (*Chairman.*) In Mr. Stephen Paget's book, in that part relating to typhoid fever, there are a good many tables of experience, where the inoculated have suffered less than the uninoculated, are there not?—Yes; all that I have said would show that there is a doubt about the matter.

13113. You have cited some in which the uninoculated came off better than the inoculated?—Yes.

13114. And he cites a good many others as well in which the reverse is the case?—Then, as I say, all that I have said simply shows that there is a doubt about the matter. I do not claim anything more than that. I next come to malaria. It is implied, though not definitely stated, by Mr. Paget that experiments on birds were necessary and essential in proving that mosquitoes convey malaria. Let us see how far he is justified in this. The following history is even admitted in Mr. Paget's book. It had long been suspected that mosquitoes caused the disease. The question was obscured by the circumstance that the common variety does not convey the poison, and that the kind responsible, the anopheles, does not buzz nor cause much irritation by its bite. In 1880, Laveran discovered the specific organism in the red blood corpuscles. In 1894 Manson suggested that this organism was removed from the blood by mosquitoes, and by a special variety. In 1895, Ross made observations on the organism found in mosquitoes after feeding them on malarial blood. The mosquito theory had now strong ground for credence. It remained to be proved that persons protected from mosquitoes in a malarial district would not get the disease, and that men bitten by the anopheles in a non-malarial district would get the disease. This was carried out and proved by a commission from the Tropical School of Medicine in 1900. Now, where was the necessity for experiments on birds. Could not a person of average intelligence have thought of the last experiment mentioned? It is true that Ross in 1898 showed that another variety of mosquito could convey an analogous disease to birds, and he also showed that this variety could not convey the malaria parasite to man. His experiment then was valueless, because it did not touch on malaria in man. That it was inconclusive was clearly the opinion of the Tropical School of Medicine, as evidenced by the fact that the final and convincing experiments on man were undertaken to settle the matter.

13115. What experiments on man are there that you refer to that were finally taken up?—One experiment was that they took a man who had had malaria previously, and therefore was very susceptible to it; they put him in a malarious district in Italy, where they kept him protected from mosquitoes, and they found that he did not get malaria. And the reverse experiment was that of I believe it was Dr. Manson's son, who allowed himself to be bitten in England, where there is no malaria, by malarial mosquitoes brought from a malarial district, and he contracted the disease.

13116. I understand that you do not doubt that mosquitoes do convey the disease?—I do not.

13117. But you say that experiments on animals were unnecessary to prove it?—They had nothing to

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do with the discovery, as is inferred, and as has been stated.

13118. (*Sir John McFadyean.*) Could you say whether that is Major Ronald Ross's own opinion as to the usefulness or uselessness of the researches which he had previously himself carried out on malaria in birds?—No, I have no information on the point.

13119. Would it affect your opinion if you knew that the discoverer himself did think so?—It would not affect my opinion unless he showed me the evidence. His mere opinion would not affect me.

13120. (*Chairman.*) Is that all you wish to say about malaria?—That is all.

13121. Then what is the next subject?—Suppuration and blood poisoning, I think, is the next. Mr. Paget writes: "The study of the micrococci has not only proved and made perfect the present methods of surgery; it has also discovered a serum for the cases of micrococcic infection. Take, for example, the disease called ulcerative endocarditis, the growth of micrococci on the valves of the heart, whence they are carried by the blood into different parts of the body. Several cases have lately been reported where recovery from this most hopeless disease has followed the use of the anti-streptococci serum. It has given excellent results in cases of puerperal fever and in cases of dissection wounds. Already, though it is a new thing, it has saved many lives, and is steadily gaining ground in practice." The truth of this statement can be estimated by comparing it with the following, which appeared in a leading article of the "Lancet" (November 18th, 1899): A valuable communication has been published ("American Journal of Obstetrics," September, 1899) by a committee appointed by the American Gynæcological Society to report upon and to investigate the value of the serum in the treatment of puerperal infection. A summary of the cases hitherto reported is given, showing that the results in 352 cases had been collected with a mortality of 20.74 per cent., and after a careful study of the literature of the subject the members of the committee formulate certain conclusions, amongst which they state that clinical observation has shown that the results obtained by employment of the serum leave a great deal to be desired, and apparently indicate that it has little, if any, effect upon the general course of streptococcic puerperal infection. The conclusions arrived at after careful consideration of the personal experiences of those forming the committee are even more emphatic, and include the following: "Experimental work has cast grave doubts upon the efficiency of anti-streptococcic serum in clinical work by showing that a serum which is obtained from a given streptococcus may protect an animal from that organism, but may be absolutely inefficient against another streptococcus, and that the number of sera which may be prepared is limited only by the number of varieties of streptococci which may exist." Again, they say: "The personal experience of your committee has shown that the mortality of streptococcus endometritis, if not interfered with, is something less than 5 per cent., and that such cases tend to recover if nature's work is not undone by too energetic local treatment." And, finally, the report ends with the words: "We find nothing in the clinical or experimental literature, or in our own experience, to indicate that its employment will materially improve the general results in the treatment of streptococcus puerperal infection." At the meeting of the British Medical Association held at Portsmouth this year, Dr. Herbert Spencer opened a discussion on the treatment of fever following delivery, with special reference to serum-therapy, in which the general opinion was expressed that although the whole question of serum treatment was too much in its infancy to justify any definite answer, the results hitherto obtained did not exhibit any material advantages over the methods generally adopted. In the "Lancet" of November 11th (pp. 1296, 1299), we published the reports of meetings of the Obstetrical Society of London, and of the Harveian Society of London, at which the subject was again discussed. At the first named Society, Dr. Herman read a paper on "Two cases in which life appeared to have been saved by anti-streptococci serum." The first was one in which a severe operation was followed by great prostration, from which the patient recovered, but the rally was followed by renewed prostration, such as in the speaker's former experience had ended in death. With anti-streptococcoc serum the symptoms quickly improved, and the patient got well. The second case was a puerperal one. Dr. Herman thought, however, that judgment as to the therapeutic value of the serum was difficult, because sufficient knowledge of the effects of the streptococcus was not yet forthcoming. The succeeding speakers all spoke in a doubtful tone as regards the therapeutic value of the serum. At the meeting of the Harveian Society, Dr. J. W. Washbourn stated that as the results of his experience, the serum was of extreme value in certain cases, but that it was valueless in other cases apparently identical. That is to say, there is a great deal of opinion that serum treatment in suppuration is useless.

13122. Did he mean by that, that in some cases the serum supplied had an effect and in others the serum had no effect, or that in some cases the same serum would have a different effect on different patients?—I do not know that that question is answered. I do not know that the report gives that information.

13123. In all these experiments on serum, so far as I understand, a great deal depends upon the purity and strength of the serum?—There is so much doubt about the whole matter that it must be a matter of opinion.

13124. (*Sir John McFadyean.*) Dr. Washbourn is dead, is he not?—I do not know. I am quoting now.

13125. (*Dr. Gaskell.*) Where is this statement in Mr. Paget's book; I cannot find it?—I think my edition of Mr. Paget's book is the first edition.

13126. It does not occur in the edition that I have; I cannot find it?—I have not seen any other edition but the first, I think.

13127. He does not seem to lay that same stress on it in the later edition which he did in what you quoted; at least, I cannot find it here?—I have seen nothing but the first edition of Mr. Paget's book. I did not know that it was altered.

13128. (*Chairman.*) I think you have read us what Dr. Washbourn said?—Yes.

13129. And you have read also what Dr. Herman said?—Yes.

13130. Then, I think, the next case you deal with is myxœdema?—Yes. It has been found that after removal of the thyroid gland—sometimes performed for hypertrophy—myxœdema follows. Now, since this disease does not come on when the patient possesses a thyroid gland, either healthy or enlarged, it is neither difficult nor brilliant to conclude that this gland produces something which prevents myxœdema. If we lost our watch and found we could not tell the time, it would not require much brain power to see that this was due to the absence of the watch. To buy another watch might occur to an unscientific person, without experiments on animals. Similarly, it is simple to see that to administer to a patient something from the loss of which he is suffering would probably give relief. And, as a fact, it is found that patients with myxœdema are benefited by taking thyroid gland from the lower animals, swallowed in tabloids. What is there wonderful in this? It is only common-sense applied. Mr. Stephen Paget tacitly implies that there is something difficult in this simple reasoning, and that it was necessary to ascertain, by removing the thyroid gland from monkeys, whether excision of a healthy gland would cause myxœdema. It might have been interesting; but why necessary? With a healthy gland—no myxœdema. With a hypertrophied gland—no myxœdema. With no gland at all—myxœdema. One who could not see the plain inference from this must be a person of diminished intelligence. If there had been any evidence that in goitre the function of the gland is destroyed by the hypertrophy, then something, though not much, might be said for the experiment on monkeys. But there was no such evidence. Goitres are only hypertrophy of one of the normal structures of the gland—the fibrous tissue, the vascular tissue, etc. There was no reason at all for assuming the function to be entirely lost. Hypertrophied muscle, or bone, or heart, does not cease to act. On the contrary, the fact that goitres usually cause no symptoms, but pressure, and that severe symptoms only come on when the gland is removed proves that the function is not lost. This is so plain that he who runs may read.

13131. Do I rightly understand your view to be that the cause of myxœdema was thoroughly and completely understood before any experiments were made on

animals?—Yes. That is all I have to say about myxœdema.

13132. Then we come to the anti-tetanus serum?—Mr. Paget remarks of anti-tetanus serum: "It is only ten years since Kitasato first obtained pure cultures of Nicolaier's bacillus; yet here is a good record already of lives saved or protected. Dr. Whitla, in his "Dictionary of Treatment," says: "Notwithstanding all the reported successes, there is little evidence of the value of injections of the anti-tetanic serum. Great numbers of cures are recorded in those cases where the period of incubation was prolonged much beyond the average noticed in severe cases, and it must be remembered that these are the class of cases where a spontaneous cure is the most likely to occur, and many reported successes would doubtless have been safe without the serum." Messrs. Rose and Carless state in their "Manual of Surgery": "At present the results of this treatment have proved disappointing, since few cases of acute tetanus have been cured by it, and the effect even in the more chronic cases is not at all certain." The "Lancet," May 20th, 1903, remarks: "Anti-tetanic serum has hitherto not fulfilled the expectations which were hoped from its use. . . . It is very uncertain."

13133. Do you express any opinion as to whether serum has been useful in any cases?—All I have been able to find out is that it is not of use. Are you speaking of serum generally or of anti-tetanus serum?

13134. I am speaking of the anti-tetanus serum?—I have no evidence except this evidence which I have already given.

13135. There have been a good many experiments, have there not, with it?—Yes, there must have been a great many.

13136. Then as to diphtheria, we have heard a good deal of evidence about the success of the anti-toxin in diphtheria?—May I take hydrophobia first?

13137. If you please?—There are certain minor considerations. Those are, that Pasteur has published misleading accounts of his experiments, that he equivocated and was compelled to admit it, that some features of the treatment are empirical guesswork and without foundation. I pass these over to come to the crucial point—the statistical evidence. This evidence is of two kinds. First, the gross mortality in France of French people from hydrophobia before and after Pasteur's treatment. Second, the case mortality. Please note that all the figures refer to French people. At the Pasteur Institute the figures of the foreigners were kept distinct and apart. My authorities for the following statements are the annals of the Pasteur Institute, Dr. Dolan's book on "Hydrophobia," "Etudes sur la Rage" by Dr. Luteaud, editor of the "Journal de Médecine," and Dr. Auguste Marie's work on "La Rage." I take the gross mortality first. There are three sets of figures: those of the Paris hospitals, those of the Department of the Seine, those of all France. Dr. Luteaud has shown that at the Paris hospitals the mortality before and after 1885 was the same. His figures give the names of the patients at each hospital for each year.

13138. (*Sir John McFadyean.*) Might I have a point made perfectly plain? What does this gross mortality mean—mortality among untreated persons?—The mortality among persons treated by Pasteur and not treated by him; but they refer to French people only, not foreigners.

13139. That is not the question. Are the bulk of them treated or untreated?—Presently I shall show you that about half were treated.

13140. You cannot discriminate between those who were treated and those who were not?—Are you referring now to what proportion these people were?

13141. I am referring to what you call the gross mortality?—By the gross mortality I mean the total number of deaths from hydrophobia in France of French people in a year.

13242. But what I meant was, that there did not seem to be any purpose in giving us the mortality of untreated people in judging of the effect of treatment, and I wondered whether you could leave out of account those not treated?—That would be no use. All I am trying to say now is that the total number of deaths did not go down after the Pasteurian treatment began

(*Chairman.*) What we would particularly like to know is, where the rabies anti-toxin has been tried, in how many cases it has been successful.

(*Sir William Collins.*) I also think it is important to know what was the gross mortality distributed among those treated and those not treated.

13143. (*Sir Mackenzie Chalmers.*) What was the gross mortality?—I am coming to that.

13144. In that last statement that you read of Dr. Luteaud, does he state whether they were treated or untreated cases?—Some treated and some untreated.

13145. (*Sir John McFadyean.*) What is the date of this? You say after 1885?—Yes.

13146. What is the date of the report you are quoting now?—I think it is for the following year.

13147. 1886?—Yes.

13148. (*Sir William Collins.*) Can you give the total deaths from hydrophobia in France during a series of years, either per million of the population or otherwise?—I am coming to that in a moment. Professor Michel Peter, another eminent Parisian clinician, showed in the "Journal de Médecine de Paris," July 5th, 1890, that if the figures for the Department of the Seine be compared for four years before and after 1885, they are 38 in one case and 37 in the other. That is to say, there were 38 for four years before 1885 and there were 37 afterwards.

13149. (*Chairman.*) Is 1885 fixed by you as the date when Pasteur began his treatment?—Yes.

13150. (*Sir Mackenzie Chalmers.*) How many years afterwards does this include?—Four years before and four years after.

13151. (*Sir William Collins.*) Is that a rate or total number?—It is simply the total mortality. There is nothing about case mortality at present.

13152. It is the total number of deaths?—Yes, the total number of deaths in the Department of the Seine of French people for four years before and four years after the introduction of Pasteur's treatment.

13153. (*Sir John McFadyean.*) Have you any statistics along with that as to the incidence of rabies among dogs in those years?—Yes, I am coming to that. If, however, the figures are taken for five years before and after 1885 the figures are 41 to 59. That is to say, that whereas in the five years previous 41 died, in the five subsequent years 59 died.

13154. (*Sir William Collins.*) Does that include the figures for Paris?—I do not know. It is simply given as the figures for the Department of the Seine.

(*Witness.*) Thirdly, the gross mortality for all France. The average annual mortality for a great many years, as given by the French Government, was 30. Dr. Auguste Marie gives also 30. Tardieu, in 1863, gave 25 as the average annual mortality. That these figures are correct is supported by the statistics of other countries, which show nearly the same mortality in proportion to population. The figures shown by the Dutch Government showed an average of less than 2, Belgium had an average of 2.6, Sweden, 4.2. In England, from 1864 to 1883 the average was 35. All these figures refer to pre-Pasteurian years. Pasteur's treatment began in July, 1885. In 1886 the deaths were of French people, 36, an increase above average, although Pasteur was treating immense numbers of patients. According to Dr. Dolan and Dr. Luteaud, the gross mortality continued higher than 30 in succeeding years. From the gross mortality there is an absence of any evidence that Pasteur has ever cured a patient of hydrophobia, and no such evidence has ever been claimed. If anything at all is proved, it is that the treatment killed. There is next the case mortality. I am coming now to the question that was asked about the number of dog bites, that is to say, the number of deaths in each 100 people bitten by dogs mad, or supposed to be mad. Pasteur's case mortality was about 1 per cent. Compared with the case mortality of any text book or any authority, this is an immense improvement. But the question necessary to face is this: Is even a single text book or authority right? There are two answers to the question. 1. There is strong presumptive evidence that every one is wrong. 2. There is direct and positive proof that every one is wrong, and that the real mortality from hydrophobia, without treatment, is the same as Pasteur obtained. I take the presump-

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tive evidence first, that is to say, the presumptive evidence that all the text books and authorities are wrong about the case mortality of hydrophobia. Suppose that the age of some one named John Smith were in question. Suppose one authority gave his age as 5 and another authority as 75, and that every other authority gave a different age, not more than one could be right. If there were 20 authorities, 19 must be wrong. And if 19 are mistaken it is probable that the twentieth would be mistaken also. Here are some authorities on the percentage mortality of hydrophobia: Brouardel gives 31 per cent, below the age of 20, and 62 per cent. above it. Tardieu, Bouley and the Comité d'Hygiène in France give a mortality of 30 per cent. for cauterised wounds and 80 per cent. for non-cauterised wounds. Bouley gives another estimate of 47 per cent. Faber (Wurtemburg), gives 20 per cent. Werner states 22 per cent.; Poland ("Holme's System of Surgery"), says it is 25 per cent.; Gamgee ("Reynold's System of Medicine"), says it varies from 5 to 50 per cent.; Gowers ("Quain's Dictionary of Medicine"), gives a mortality of from 50 to 75 per cent. Pasteur himself believed (or at least said he believed), it was 20 per cent. Mr. Paget, with unconscious humour, remarks, "What was the risk of a bite from a rabid animal in the days before 1885? It is a matter of guess work." The reasons for all this diversity of opinion are easy to see. There is the great rarity of the disease. Perhaps not one medical man in a hundred ever sees a case. This factor alone would prevent reliable statistics. There was the custom of killing the dog to prevent its biting more people. There is the fact that there are about half-a-dozen diseases simulating rabies. It is easy to see what has happened. A dog bites a number of people. Nobody dies. Natural conclusions, the dog was not mad. The figures are excluded from statistics. The text book figures are therefore worthless from their mutual contradiction, and from the fallacies which must have come in. I come now to the positive proof.

13155. (*Chairman.*) You are proposing to deal with the figures in the text books?—Yes. I shall show directly that the real mortality in the untreated people is about 1 per cent. Further, I shall show that even a belief in the lowest figure ever given—that is, in the text books—namely, 5 per cent., is a hopeless absurdity. The figures which condemn Pasteur's pretensions come from the Pasteur Institute. In 1886, the first whole year of Pasteur's treatment, 36 French people died of hydrophobia; 19 were treated by Pasteur, 17 were not. It is with these 17 who stayed away we are now concerned.

13156. Are these figures taken from some French statistics?—They are French statistics taken from Dr. Luteaud's work on "Etudes sur la Rage," and Dr. Dolan's book on Hydrophobia. The average total mortality in France was 30, as we have seen. I am going to give certain figures with certain deductions, and these are somewhat difficult to follow. I consider them very important, and I will give them slowly. Seventeen is rather more than the half of 30. We may therefore presume that rather more than half the dog-bitten people stayed away from the Pasteur Institute. Let us assume that at least half stayed away, which is conceding Pasteur a slight advantage; that is to say, apparently, by these figures more than half stayed away; but we will give Pasteur the advantage and concede that one-half stayed away. If half stayed away the other half went.

13157. (*Sir William Church.*) This is only supposition. You do not give us any figures?—I do give the figures.

13158. (*Chairman.*) What you told us was that 36 was the number who died in that year?—Thirty-six French people died in the year 1886.

13159. Then you must take your 19 who died as against the 36, and not against the 30?—The assumption is that Pasteur's treatment modified in some way the number of deaths. I am leaving that for a moment. I am taking 17 who died without going. I am only trying to show at present how many dog-bitten people stayed away from the Pasteur Institute. Do you follow?

13160. I cannot see, if you deal with the figures of 1886, how you can diminish those who actually stayed away to 30. You say that 19 were treated and 17 were not. You must deal with those figures?—The 17 people, then, who died did not go to the Pasteur Institute. Is that admitted?

13161. That I understand to be what you say?—Therefore, if 17 did not go, if 30 was the average, the presumption is that more than half the people untreated by Pasteur, bitten by dogs, mad or supposed to be mad, did not go.

13162. That I do not follow?—I think it is perfectly clear. It is a question of whether you understand me?

13163. No, it is my understanding that is lacking?—It is no good my going on unless that point is clear.

13164. (*Sir John McFadyean.*) Do you mean that a mortality of 30 would mean probably somewhere about 100 or 200 people bitten annually?—I am not talking about that.

13165. Then I am another who is unable to follow you?—Then let me begin again.

13166. (*Chairman.*) I think we understand, so far as you have gone, what you have said, but I was saying that you introduced the figure 30 as the average after having introduced the actual number of deaths and the actual number treated?—I have given a considerable number before that—30 was the average.

13167. I know that. Now, you introduce that 30 which does not seem to me to have anything to do with the question as to a year in which there were undoubtedly 36?—It seems to me it has very much to do with it. Seventeen French people in 1886 died in France who had not been to the Pasteur Institute. Supposing 34 people were the average, we might assume, then, that half the dog-bitten people stayed away. Is that clear?

13168. To my mind it is not of the slightest importance whether half or one more or one less than half stayed away. Say about half?—About half. Is that clear?

13169. You cannot either prove or disprove the usefulness of antitoxins by such distinctions as that?—These are the best figures we have got.

13170. Yes, I understand that?—Is it admitted that about half the dog-bitten people in France stayed away?

13171. (*Sir John McFadyean.*) That is what I want you to explain—how you know the number of people who stayed away from the number who died of hydrophobia without treatment?—This surely shows us.

13172. I tried to help you out a minute ago by suggesting that you might have estimated the number of people bitten annually from the number of people who died from hydrophobia, but you did not take it?—No, not at present.

13173. Then I do not know yet how you arrive at it?—I have not shown you yet. But what I want you to admit at present is that if 17 people in 1886 died of hydrophobia without going to Pasteur, it is probable that at least half the dog-bitten people stayed away.

13174. But we cannot see that?—Because the average is 30.

13175. (*Chairman.*) I think you had better present your argument without asking us to admit it?—Then I will state it, and I think at your leisure you will admit it. Thirty was the annual average; 17 is rather more than the half of 30. We may therefore presume that rather more than half the dog-bitten people stayed away from the Pasteur Institute. Let us assume that at least half stayed away, which is conceding Pasteur a slight advantage. If half stayed away the other half went. I think that will be admitted.

13176. (*Mr. Ram.*) The other half may be supposed to have gone, you mean?—Assuming that half stayed away, you must admit that the other half went. We know the number who went. On arriving they were counted; they were, in round numbers, 1,500.* I take these figures in another aspect. If half the dog-bitten people were 1,500, all the dog-bitten people were 3,000.

* *1,500 is a round number only. The figures in 1887 were 1,531 and in 1888, 1,404. My recollection is that in 1886 the figure was a little less.—S. F. S.*

13177. (*Chairman.*) When you say that half went, the other half of the 36 or the 30 went; but as to the other 1,460, I do not see why you say that they went to the Pasteur Institute?—They were counted.

13178. (*Mr. Ram.*) You know the number that went to Pasteur?—Yes.

13179. What indication does that give you of the number that did not go?—That can be arrived at by observing the number of deaths of untreated people, and comparing them with the previous average annual deaths.

13180. (*Chairman.*) Besides, the 30 are the number of people who died in France?—Yes, annually.

13181. But how many of these 1,500 people were foreigners who came to be treated by Pasteur?—The figures are kept distinct in the Annals of the Pasteur Institute.

13182. They are beyond the 1,500?—Yes, with foreigners there were about 2,000. I disregarded foreigners altogether. If half the dog-bitten people were 1,500, all the dog-bitten people were 3,000. Now, for the first time in history we have some reliable figures on a large scale as to the number of dog-bitten people in a country in a year. Now, compare the annual mortality in France—viz., 30—with the number of people—viz., 3,000—bitten by dogs mad and supposed to be mad; 30 to 3,000 gives 1 per cent. This is the real mortality of hydrophobia in dog-bitten people. If we accept any other percentage figure of the text books we are reduced to absurdity. If we accept even 10 per cent., then 10 per cent. of 3,000 is 300. To believe that 300 would have died in a year would be to assume that from some cause the mortality suddenly jumped from 30 to 300. If we accept the lowest figure ever given, 5 per cent. mortality, we are in inextricable difficulties with a gross mortality of 150. The pretensions of Pasteur's treatment rest upon statistics and by statistics they are condemned.

13183. All the 36 who died undoubtedly had hydrophobia?—Yes.

13184. Why do you reason from that that the 1,500 who went to Pasteur must undoubtedly represent half of the people who were bitten?—Bitten by dogs mad or supposed to be mad.

13185. I do not quite follow why it should be so?—It seemed to me so clear that it makes it appear to me as if you had not understood me.

13186. I desired to bring it to your mind that I did not understand you?—I think the figures undoubtedly are that in the first year, 1886, 17 people who did not go to Pasteur died of hydrophobia—that is a certainty. It is a certainty that the average annual mortality in France was 30.

13187. (*Sir Mackenzie Chalmers.*) In that year or in other years?—Extending over a number of years. I said that was an average. Then, as 17 is very nearly the half of 30, we may assume that half the people stayed away from the Pasteur Institute.

(*Mr. Ram.*) I cannot follow that; that is where my difficulty comes in.

13188. (*Sir Mackenzie Chalmers.*) There is a *non sequitor* there?—It is so absolutely clear—there is no *non sequitor*. Let us assume that in each year 30 people always died, never more and never less. Let us assume that in 1886 15 people died without going to Pasteur, surely that would show that half the dog-bitten people did not go to Pasteur.

13189. (*Mr. Ram.*) Why are we to assume that?—To make it clear.

13190. (*Chairman.*) It is not a question of assuming a thing to make it clear. The point is to make it clear that you have a right to assume it?—It seems to me already so clear.

13191. (*Sir William Collins.*) Are you suggesting that the ratio of cases treated and untreated must be regarded as the same as the ratio of deaths of treated and untreated?—No, I am not assuming that. That does occur, but I am not assuming it.

13192. (*Sir John McFadyean.*) Are you forming any estimate, then, as to the number of people annually bitten in France from the fact that 30 was the average mortality?—Not directly, but indirectly.

13193. Indirectly, how?—Indirectly, in this way: 30 people died annually. In the year 1886, 17 people, about half of the 30, died without being treated at the Pasteur Institute. Surely it follows—it is no *non sequitor*—that half the people stayed away from the Pasteur Institute. It seems to me to be absolutely clear.

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13194. (*Mr. Ram.*) Do you know how many went to Pasteur in that year?—Yes.

13195. How many?—Something over 1,500 in the year 1886.*

13196. Then do you mean that because 19 went——?—No, 19 did not go.

13197. How many went?—1,500.

13198. And how many died altogether?—In the whole of France 36.

13199. Of those you say that 17 were not treated at all?—Yes.

13200. Is your deduction that the rest of the number between 17 and 36 did go to Pasteur and did die?—Yes.

13201. That is your deduction?—No, it is not a deduction, it is a fact given by Pasteur.

13202. (*Sir Mackenzie Chalmers.*) He got 19 deaths out of 1,500 cases?—He got 19 deaths out of 1,500 cases.

13203. You want us to assume that there were another 1,500 cases with a similar number of deaths that were not treated?—With 17 deaths that were not treated.

13204. (*Sir John McFadyean.*) Might I ask you a question with regard to what you call the average mortality, which you said was 30? Can you tell us whether it fluctuated very much?—Yes.

13205. What were the extremes?—I believe there were in one year as many as 47. I think that was the extreme year.†

13206. Perhaps you could tell us if you have the statistics handy?—I have them all here.

13207. (*Mr. Ram.*) What you want to put comes to this, does it not? That out of the total number of 1,500 of those that went to Pasteur he got no better percentage of deaths, rather worse, than out of the number of 1,500 who did not go to him, and who died?—No, not quite like that. I have not worked it out; that is not the point I have been trying to make.

13208. (*Chairman.*) You have been giving these figures with the object of showing that the number of deaths in the natural course of events without Pasteur interfering is 17?—Yes, that is my object.

13209. It does not, to my mind, prove it. I see what you mean by it, but I am not at all satisfied that the same ratio exists between those who go there and die and those who go there and survive?—I would leave out of question at first all those thousands who went to Pasteur for the time being altogether, and I will take the 17 who died without going to Pasteur, and as 17 is about half the average, we may assume, certainly, that half stayed away.

13210. I think you have put your argument. I quite see what your argument is; but all I can say is that, from some deficiency in my mind very likely, it does not convey to me proof that 1 per cent. of the people died?—It seems to me that the proof is absolute so far as any proof of that kind goes; that these are the best figures we have on a large scale of the real mortality of hydrophobia.

13211. When you say that it is absolute, that is where I differ from you. It may be something to help you in forming a rough judgment, that is all I can say?—I think Sir John McFadyean asked for the figures.

13212. (*Sir John McFadyean.*) But do not let me interrupt your argument, it can stand over quite well; it was merely because it seemed to affect the argument whether they fluctuated?—That is all I have to say. My point was to prove from these figures that the real mortality from hydrophobia is 1 per cent., and that Pasteur, in comparing his mortality with the figures in the text books, was comparing his mortality with a fiction—with a figment. I may say that when

* *Less than 1,500.—S. F. S.*
† *The highest was 66 in 1864.—S. F. S.*

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Pasteur's treatment was first introduced there was a great deal of scepticism among Paris physicians as to the value of it, and both his figures and the circumstances of the case were subjected to very close examination. I give what Dr. Luteaud says about the evidence.

13213. (*Sir Mackenzie Chalmers.*) What is the date of that?—Dr. Luteaud's book was published in 1891. This is merely a summary of what is contained there; it is briefer than in the book. Hydrophobia shows itself under two forms—the convulsive form or the paralytic form. All authorities agree that the latter variety is extremely rare. In the year 1886 twenty-two of Pasteur's patients died of hydrophobia. I said 19 a moment ago. There was a dispute, that is why I say 22 here. Seven of these, practically one-third, died of the paralytic form. This is significant, because the virus which Pasteur injected on his patients, the "fixed virus" of the laboratory, invariably produces paralytic rabies in the rabbit. All authorities are likewise agreed that a common symptom at the onset of the malady is pricking at the place of the bite. Now, in many of Pasteur's fatal cases this sign appeared not at the bite, but at the point of inoculation. This occurred in the case of Bernard Sodini, who was treated on October 21st, 1886, and died November 24th. Such was the case also with Léopold Née, of Arras, who was treated November 17th, 1886, and died December 17th. Many more cases might be quoted. But all this is not absolutely conclusive, it may be objected. It is only presumptive. True. But what shall we say of the following? In July, 1887, Arthur Stoboi was bitten by a dog at Lubline, in Russia. On the chance that the dog might be mad he was sent to the Pasteur Institute at Warsaw, where he was treated by Dr. Boniville till August 11th. He died of hydrophobia on November 11th. The disease was preceded by pains at the seat of inoculation. The dog remained healthy, and lived for years afterwards. The same test—the survival of the dog—proved that in the cases of Née, of Arras, De Moens, of Antwerp, and of others, Pasteur was the cause of death and not the dog.

13214. (*Chairman.*) Is that all you propose to say about rabies?—Yes.

13215. Have you had any experience of treating rabies yourself?—No.

13216. What you have given us is from the books and opinions and statistics of others?—Yes.

13217. Do you think there is any antecedent improbability in an anti-toxin being discovered that would cure rabies?—It seems to me that that question is one of the widest questions that it is possible to put. It seems to open up everything.

13218. (*Mr. Ram.*) And what is the answer to it?—The question is too wide.

13219. (*Chairman.*) You mean, in fact, that you have never considered the subject sufficiently to say whether you do or do not see an antecedent improbability in treating rabies successfully by an anti-toxin?—The question seems so wide that I am not quite sure whether it has some less wide meaning than it appears to me to have.

13220. I assure you that I put the question in perfect good faith?—I have no doubt of that, but it seems so wide.

13221. You are not prepared to give an answer to it?—I am not.

13222. Very well. Supposing that in 1886, 24 out of Pasteur's 36 patients had died instead of 19 that would alter your figure altogether. It would not be at all an unlikely thing, would it, that out of 36 patients there might be a considerable variety between those who were cured and those who were not cured?—Do you say a considerable variety?

13223. A considerable variance?—A variance in what way?

13224. That in one year two-thirds of Pasteur's patients might die and in another year only half?—No two-thirds never died; only 1 per cent. died.

13225. But I mean that two-thirds of those who did die might be Pasteur's patients, and at another time only half?—Perhaps some variation of that kind might have occurred.

13226. Then your average of 1 per cent. would vary enormously in that case, would it not?—Not enormously, but it would vary to some extent.*

13227. A good many hundreds you would knock off or put on?—It is a question of mental arithmetic which is rather difficult for me.

13228. You speak of it as being such an absolute certainty that because they were 17 out of 30, or 19 out of 36, or whatever it was, of Pasteur's patients who died, there must exist exactly the same proportion always, and that that proves positively that 1 per cent. is the true average?—But it is just as likely that the figures might have fluctuated one way as the other.

13229. You do not know; you cannot take a single year and say it is just as likely the same thing might happen every year. However, I will not argue it further. I point out just why your reasoning does not seem to me satisfactory as to the 1 per cent. You passed over something that you wished to refer to, diphtheria?—The value of the anti-toxin for diphtheria has been disputed by a host of writers, especially continental and American. England is the exception; there has been little scepticism in England compared with what there has been in other countries. The objections to belief in anti-toxin fall into two classes; first, the theory underlying the treatment is not proved (we may call this speculative objections); second, the mortality statistics are a barrier (we may call this positive objections). Taking the speculative objections first, a patient attacked by such diseases as scarlatina, small pox or diphtheria, usually recovers. In some way Nature throws off the disease. So far we are merely stating facts. But when we try to explain how the "throwing off" is effected, we embark on speculation. We may assume, first, that the cells of which the body is composed become resistant to the disease; second, that the body manufactures an anti-toxin (anti-poison), which neutralises the toxin (poison) of the disease. These theories have two points in common—(a) They furnish us with a comfortable and seductive working hypothesis; (b) they may be true, but they lack proof. It is with the second theory that we are concerned, for on it is based the serum treatment of diphtheria. This therapeutic measure was adopted on the following grounds: In 1883 Klebs discovered a bacillus believed to be specific to diphtheria. (By "specific" is meant that it causes the disease.) From further research by Löffler it has been called the Klebs-Löffler bacillus. The reasons for believing it to be the cause of diphtheria are the following: (1) It is found in a large percentage of diphtheria cases; (2) Injected into the lower animals it kills; (3) It causes on mucous membranes an appearance resembling diphtheria. Whether this evidence is convincing we must subsequently consider. The anti-toxin treatment arose as follows: It is stated as a well-established fact that if an animal be inured to small doses of the bacterium it becomes immune to large doses. It is stated also that if the blood serum from an immunised animal be injected into a fresh animal, the latter is protected against an otherwise fatal dose of bacilli. Assuming that the Klebs-Löffler bacillus is the cause of diphtheria, we might expect the blood serum of immunised animals to protect man to some extent against the disease. The weak points in this speculation are the following: (1) There are reasons for believing that the Klebs-Löffler bacillus is not the cause of the disease. If this is true, the treatment cannot correspond with the theory. (2) Even with belief in the specificity of the bacillus, it would be necessary to prove that the results observed in animals occur also in man. The reasons for disbelieving that the Klebs-Löffler bacillus is the cause of diphtheria are as follows: Koch made the following demands before admitting the specificity of a micro-organism: (1) It must always be found in the disease it is supposed to produce; (2) It must not be found in healthy persons or in other diseases; (3) A pure cultivation injected into an animal must cause the disease and be found again in that animal. As the third test can be disposed of briefly, we may refer to it first. The most that can be said for the view that the bacillus causes diphtheria in animals is that (a) It kills; (b) It causes a white

* *Such a supposition would show a natural mortality of 1½ per cent., a slight variation only.—S. F. S.*

appearance on mucous membranes. But these two circumstances are not convincing. Many poisons kill. Many agents, including chemicals, render mucous membranes white. Hencke, after a series of experiments, arrived at the following declaration, in which not even a hope is expressed that the identity of human diphtheria and the artificial disease in animals is established. He says: "Diphtheria of guinea-pigs has, as is well known, not the least resemblance to human diphtheria, as the most enthusiastic adherents of the Klebs-Löffler bacillus have to admit." By Koch's third test, therefore, the claim of the bacillus to specificity breaks down. Here is a convenient place to notice an illogical position, in which the bacteriologists have placed themselves. They deny the value of naked eye appearances for diagnosis of diphtheria in man. They rely on naked eye appearances for diagnosis of diphtheria in animals. They beg the whole question. Koch's first test is that the bacterium must always be found in the disease. Löffler himself regarded the coincidence of clinical and bacteriological diagnosis as doubtful. This has been affirmed by others. He himself failed to find the bacillus in 25 per cent. of diphtheria cases. Hennig believed the bacterium to be absent in more than 25 per cent. The first test, therefore, fails. Bacteriologists have attempted to avoid this objection by denying that any cases are diphtheria unless the bacillus is found. This, again, begs the whole question. We have the position humorously described by Hanseman: "The Klebs- Löffler bacillus is to be found in all cases of diphtheria, provided we select only those cases in which it is present." Koch's second test is that the bacillus must not be found in health and other diseases. Ritter has found the bacterium (fully virulent to animals) in the mouths of healthy children (127 cases), of nurses of diphtheria patients, and in their noses and throats up to four weeks after disappearance of the disease. It is often found even months afterwards. Uthoff and Fränkel found it in the conjunctiva. Meyer noticed that of some children who were exposed to the same source of infection, one caught a harmless sore throat, another a malignant diphtheria, but both with the bacillus equally virulent to animals. The presence of the bacterium is common in harmless sore throats. I ask special attention to this. The importance will appear later. In catarrhal inflammation of the conjunctiva, the bacillus fully virulent to animals is often found. It is strange, therefore, that diphtheria of the conjunctiva (already very susceptible through inflammation) is so rare, Baginsky, Meyer, and others state the bacillus (highly virulent to animals) to be common in the mucous membrane of the nose, associated with rhinitis, a harmless disease. It is remarkable that in so many cases of rhinitis, with virulent diphtheria bacilli, so few contract diphtheria. Todd and Washbourn observed among convalescents from scarlatina, an endemic rhinitis (that is, inflammation of the mucous membrane of the nose) without the formation of a membrane, without swelling of the glands, without fever, etc., etc. In all cases (51) they found the Klebs-Löffler bacillus, which was fatal to guinea-pigs. They note a series of cases with the true Klebs-Löffler bacillus occurring in children strongly susceptible (through rhinitis) as well as susceptible in general, because they had had scarlatina. Among these was not found a single case in which diphtheria occurred. Soerensen points out that diphtheria, following measles and scarlatina, is very fatal. Yet he found the perfectly fatal (to animals) Klebs-Löffler bacillus in 326 patients convalescent from scarlatina. In 85 per cent. there was not a symptom of diphtheria. In 15 per cent. there was a slight membrane. There was not a single case of croup, and not a single case of death. Stoos gives observations about many nasal catarrhs from the Berne hospital, and from different parts of Switzerland, where no diphtheria has occurred for years. The Klebs-Löffler bacillus was found almost without exception. No diphtheria, yet a very susceptible mucous membrane. Kanthack says: "Mention must be made of the numerous cases of fibrinous rhinitis in which diphtheria bacilli have been found." "Further, bacilli, resembling diphtheria bacilli, are found with the greatest frequency in many ulcerations of the skin, gangrene, stomatitis, cancrum oris, noma." He continues: "Todd's observation is a further contribution gradually gaining ground that the diphtheria bacillus is found in many lesions which are not diphtheria." On each of Koch's three tests the evidence against the specificity of the Klebs-Löffler bacillus is crushing, and there are a great many disbelievers. Kassowitz states: "It is now generally admitted that the Löffler bacillus is not the exciting cause of diphtheria" (Medical Press and Circular, June 15th, 1898).

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13230. (*Sir John McFadyean.*) Is that an editorial article?—I do not remember; this is from notes made some time ago. It could be ascertained by looking up the Medical Press and Circular of 1898.

13231. I only wanted to know what importance to attach to the statement?—The statement is: "It is now generally admitted that the Löffler bacillus is not the exciting cause of diphtheria. That is the opinion of Kassowitz. I turn now to the positive objections. The antitoxin treatment depends on the assumption that the Klebs-Löffler bacillus is the cause of the disease. We have also seen that by all the recognised tests this assumption has been proved false. There remains the practical question: What have been the results? These results must be looked at in the form of statistics. These statistics may be examined in two ways. We may ascertain the total deaths from diphtheria in a country for each year. This is called the absolute mortality. We may also take the figures of patients in hospitals, and note the deaths in each hundred cases. This is called the case mortality. Before going further, we must look at several most important sources of fallacy which might vitiate any conclusion. They are the following:—(1) The fact that statistics have been made with selected cases. (2) The fact that the severity of diphtheria varies with time and place to an immense degree. When we have scrutinised these points we shall see that they are formidable enough to deceive even an impartial inquirer, and to enable a partisan who disregards them to prove either that antitoxin has saved or has killed an enormous number of diphtheria patients. Then statistics have been made from selected cases. We may get favourable but misleading results for a remedy by two methods of selecting cases. (*a*) We may eliminate severe instances. Roux, in the first statistics of antitoxin ever made, suppressed 7 per cent. of mortality by this means. Dr. Lennox Browne remarks in relation to this source of fallacy: "When this error is rectified the improvement due to antitoxin, as practised in the Metropolitan Infectious Fever hospitals for 1895 and 1896, is entirely dissipated." (*b*) We may include in our statistics an increased number of mild cases. Lennox Browne remarks on this: "The Klebs-Löffler bacillus being now recognised as an irrefragable test of diphtheria—the clinical criteria being often treated as subordinate—the numbers of cases of the disease are much greater than in former times." Tirard, in his book, "Diphtheria and Antitoxin," says: "Few diseases give rise to such frequent mistakes." He says also: "Some cases in which the bacillus is found to be present would otherwise give rise to no anxiety; while, on the other hand, in some cases of undoubted diphtheria, clinically, the bacillus is sought in vain."

13232. When you say that Dr. Lennox Browne says this and Tirard says that, are these your observations which you are making as the result of your own reading, or are you quoting it all from one article? It is difficult to follow what the evidence is?—I began to quote only here. I said Dr. Lennox Browne remarks in relation to this gross fallacy——

13233. Are you now reading from one of your own notes?—This is something I have already published.

13234. You are not going to read the whole of it?—I want to read the whole of it.

13235. It is quite unusual to read a long statement of many pages?—But I would like to point out that this question of antitoxin is one which has been so prominent in the controversy of vivisection—I have seen a good deal of vivisection controversy, and this point is always being brought up, and, so far as I can see, none of the witnesses who have been here before the Commission have any grasp whatever of the fallacies underlying these statistics.

13236. It is quite impossible to take in all these things mixed up with statistics and figures, and to follow them and to carry anything in one's mind. It can be printed as part of your evidence?—That is quite sufficient for my purpose. I ask this because of the very great importance of the question.

13237. We have been sitting here for many days, and we have heard, I think, almost all these points that

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you have been taking, and, therefore, when you say that it is of vital importance that you should read these thirty pages, you are speaking of it as a person who comes freshly into this room?—I have read Dr. Martin's evidence on diphtheria, and his evidence is simply honeycombed with fallacies, and I have been extremely anxious to point these fallacies out.

13238. (*Sir John McFadyean.*) Has this been written since Dr. Martin's evidence?—No, long before.

13239. Would it not be much more useful and more convincing to this Commission if you would show us the fallacies that are so abundant in Dr. Martin's statistics?—Yes, I will.

13240. (*Chairman.*) When you began reading, I did not understand what you were busy upon. It is very unusual to read whole pamphlets?—I ought, perhaps, to have stated before that I was going to read a lot, and to have told the Commission the reason why I wished to do so, as this is a point in the controversy which is so much more important than practically all other points.

13241. Several witnesses have handed in to us pamphlets which they have written which they did not expect to be printed in the evidence before the Commission. Others have handed in long statements with figures, but not amounting to thirty pages, which have been incorporated in the evidence of the witnesses. This is a very strong demand upon us?—May I, then, say that I have here about fourteen pages of what I consider the most important evidence, which I have not read?

13242. You would prefer reading them to throwing them aside and dealing with Dr. Martin's evidence separately? Because we cannot have it both ways. You say that you want this specially read, as being an answer to Dr. Martin?—Yes.

13243. Then you would prefer that it should be printed?—Yes, dealing with the positive objections.

13244. Then it shall be done?—I ask special attention to this point, for it relates to a fallacy of the greatest dimensions. We have already seen that the Klebs-Löffler bacillus is found not only in perfectly healthy people, but also in numerous lesions which are not diphtheria, and among these, simple sore throats. We must remember that the believers in antitoxin are all believers in the bacteriological diagnosis. Consequently the statistics of serum treatment are made on selected cases—selected by the inclusion of trifling sore throats that by the clinical diagnosis would be excluded. I bring further evidence on this point. The antitoxin treatment was first tried at Paris in February, 1894, and was in general use in 1895. The following figures show how the serum treatment and the bacteriological diagnosis at once swelled the numbers of cases diagnosed as diphtheria. Coakley gives these statistics:

BOSTON.

Year.		Notified Cases.
	1891	831
	1892	1,353
	1893	1,465
Serum years.	1894	3,019
	1895	4,059

NEW YORK.

Year.		Notified Cases.
	1891	4,874
	1892	4,654
	1893	6,463
Serum years.	1894	9,155
	1895	9,925

BROOKLYN.

Year.		Notified Cases.
	1891	1,850
	1892	1,829
	1893	1,672
Serum years.	1894	3,812
	1895	4,277

According to the "Medical News," 1896, No. 25, there occurred in the Boston City Hospital the following statistics:—

Year.		Admissions.	Deaths.
	1893	419	203
Serum years.	1894	598	266
	1895	1,566	207

We see here a great increase in numbers diagnosed as diphtheria, while the absolute mortality is not appreciably changed.

From the Hospital of St. Ladislaus we get the following figures:—

Year.		Admissions.
Without Serum.	1893	213
	1894	224
With Serum.	1895 / 1896	1,046

We see here the diagnosed cases doubled. According to Kassowitz, there were treated by Baginski in 1894.

In the first quarter, without serum 86
In the second quarter, with serum 239

Gottstein gives the following table:—

Year.	Admissions to the Hospital.
1892	453
1893	454
1894	712

In Basle, from 1885 to 1894, the average annual notifications were 245. In 1895 and 1896 we find the number increased to 740. We need not pursue this part of the subject further. The evidence is overwhelming that the bacteriological diagnosis and the serum treatment have brought an enormous number of simple sore throats into the figures of diphtheria. To compare such statistics with statistics from which mild inflammations of the throat have been excluded lands us at once in a fallacy of the greatest dimensions. Now this fallacy the champions of serum ask us to ignore, as they ignore it themselves. I here finish with the first source of erroneous conclusions, and come to the second. The severity of diphtheria varies widely in time and place. If we counted the shipwrecks after a moderate gale, and again after a violent hurricane, we should find a difference in the figures. If we tried

to explain the difference without taking into account the difference in the two storms, it is obvious that we should be leaving out a somewhat important factor. Now, one epidemic of diphtheria differs in severity from another, as much as one storm differs from another. I proceed to prove this. Dr. Feilchenfelder gives the following figures of diphtheria mortality in Charlottenburg:—

Year.	Deaths.
1887	56
1888	11
1889	18
1890	32
1891	37
1892	79
1893	129
1894	47
1895	45
1896	24
1897	53
1898	64

Hellström says that at Stockholm in 1894 the mortality of 33 per cent. in the first quarter of the year sank to 6 per cent. without change of treatment. In the South-Eastern Hospital of London, the mortality in July was 37 per cent. In the following August it fell to 18 per cent. without change of treatment (MacCombie). Aub has pointed out that at Munich the mortality during six years oscillated between 6 and 14 per cent. According to Hahn, the mortality of Friedrichshain in some years differed by 18 per cent. from the mortality of other years (with no change of treatment). Buning gives the diagram in Fig. 1 for the twelve largest parishes in Holland. The reader will scarcely ask for more evidence that the virulence of diphtheria at one period of time varies widely from its virulence at another.

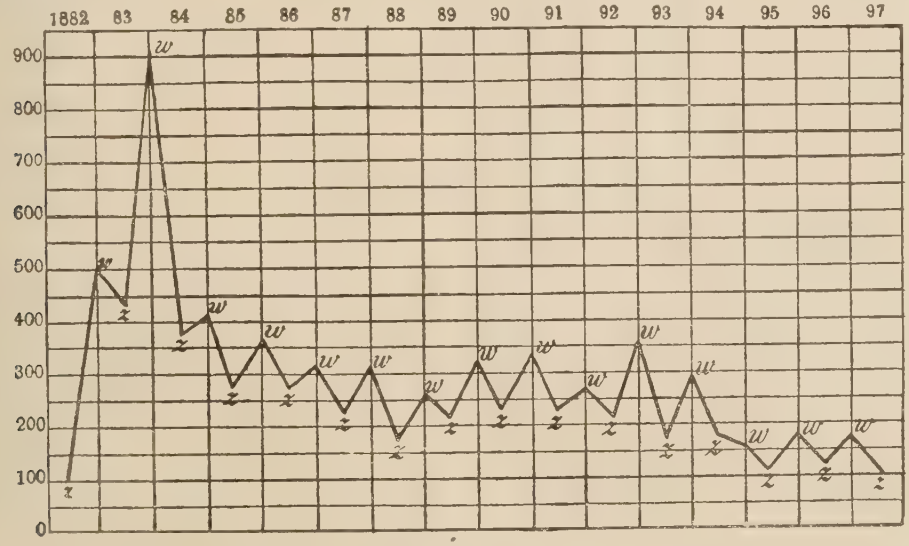

FIG. 1.

The causes of this variation are unknown. It is obvious that if we build up any argument on the assumption that the mortality of diphtheria would remain stationary, unless influenced by treatment, we are at once entangled in a huge fallacy. Yet this is exactly what the champions of serum ask us to do. And they ask it in face of all evidence to the contrary. Time is not the only disturbing factor. Place also has a mysterious influence. In 1888 Baginsky observed an epidemic in two villages side by side. Treatment and time were the same. In one place no patient died. In the other nearly all died. If the treatment had been different in each case a firm conviction in favour of one would have been established.

Rump, in Hamburg, gives the following table:—

The New Hospital.		The Old Hospital.	
Year.	Mortality.	Year.	Mortality.
1889	28 per cent.	1889	49 per cent.
1891	13 ,,	1891	52 ,,

We see to what a wide extent results will be affected by the selection of cases and the disturbing factors of time and place. Keeping these points in mind, we see how they throw light on the following remarks of Tirard: "Few diseases have been treated by such numerous remedies, and for every remedy a large percentage of successes has been claimed. It has been found that the drugs recommended were first greatly vaunted; that then they gradually fell into disuse. Perhaps they were completely forgotten, and a few years later they were introduced as novelties with the same extravagant praise and a similar list of successful cases." That is to say, when a new treatment was tried at a time when the mortality from natural causes was falling, the treatment got the credit. When the mortality again rose, the treatment was abandoned. The champions of various drugs were sincere in their belief, but they were deceived by ignoring the natural disturbing factors. I shall bring evidence that the believers in serum have been deceived in exactly the same way, and, moreover, have been led astray by one additional source of fallacy. Prior to the use of antitoxin, the advocates of different drugs were only misled by two factors—the disturbing elements of time and place. They could not be accused of introducing a new standard of diagnosis to flatter their figures. The antitoxin believers, on the contrary, based their statistics on the inclusion of an enormous number of mild cases brought in by the new bacteriological diagnosis. This we have already seen. There is another point of contrast. Other remedies, based on more favourable figures than those of serum, have never received a sudden world-wide attention. Roux, at the Hospital for Sick Children, Paris, made statistics on 300 cases from February to July, 1894. (I may note, *en passant*, that they were vitiated by every one of the sources of fallacy we have considered.) His report at once received a huge advertisement resounding throughout every civilized land. The treatment was soon adopted in most countries in 1894, and was in full swing in 1895. The explanation of why Roux's figures received such sudden fame whilst the better ones of others were neglected is obvious. Pasteur's

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treatment of hydrophobia was generally believed in. The treatment of diphtheria by Roux (of the Pasteur Institute), was based on somewhat similar lines. The prestige of Pasteur threw a glamour over the new antitoxin remedy. Before looking at serum statistics there is a point to be noticed. The new remedy was introduced at the most favourable moment—when a decrease in mortality was well on its way. Besides a more rapid up and down variation in the virulence of diphtheria, there is a slower movement extending over years. This is clearly exemplified in Figure 1. That the severity of diphtheria had been declining previous to 1894 there is abundant evidence. Göttstein says of Berlin that the virulence increased from 1876 to 1883, then remained nearly stationary till 1886, and after this declined more rapidly, especially in the years 1890, 1892 and 1893. Bernheim says of this: "It is clearly shown that for eleven years we have been on the descending curve of a severe epidemic, and that, apart from the decline of absolute mortality, the severity of the cases has diminished, and this has caused a decrease of the case mortality." This view is endorsed by Soerensen, Heubner, Bötticher, and others. From 1885 to 1890 the total deaths in the Netherlands were 9,656. From 1890 to 1895 the figures were 7,808, showing a spontaneous decrease. *Just previous* to the advent of anti-toxin there was a special fall observed. According to Buning this occurred:—In Paris at the beginning of 1893. In Holland at the end of 1893. In German towns at the end of 1893. In Vienna at the end of 1893. In Stockholm it occurred in 1894, but without the use of serum. Looking at the figures of the Metropolitan Asylums Board, we see a continuous fall from 1889 (see Table below). Buning gives the following diagram of the mortality at Paris (Fig. 2):—

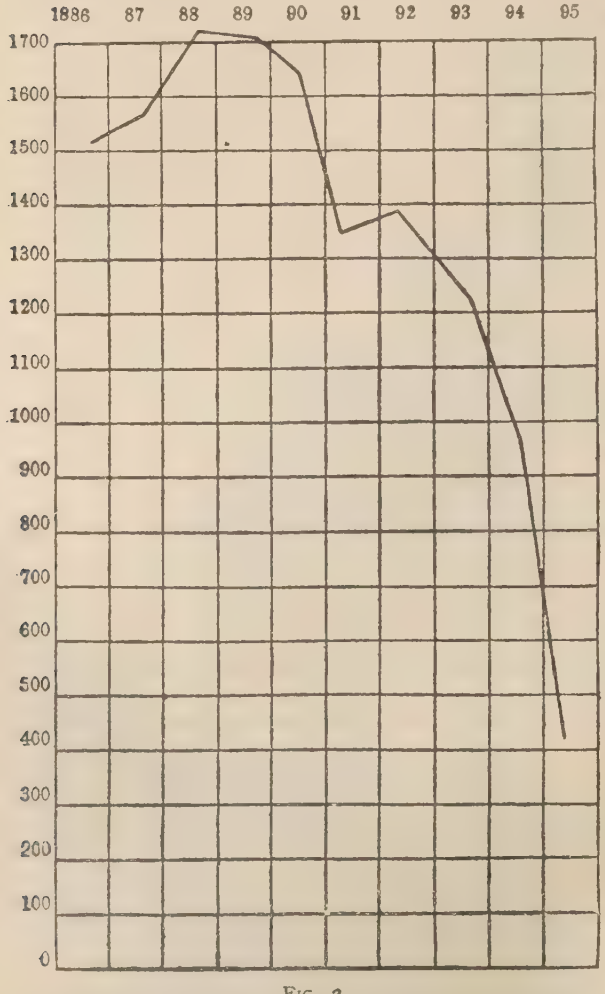

FIG. 2.

Looking at Fig. 2, we see that the spring of 1894 was a most favourable moment for assistance (from natural causes) in getting good figures. We have now considered the factors which, if disregarded, will vitiate any conclusions. We see the conditions necessary before a true comparison of different treatments can be obtained. They are:—(1) The place must be the same. (2) The time must be the same. (3) The statistics must not be aided by selecting the cases. We have seen that this has been done; sometimes by exclusion of severe instances, and, in all cases where serum statistics have been made, by the inclusion of an increased number of mild examples of diphtheria. (Roux's first figures were improved by exclusion of severe and inclusion of mild cases.) The only statistics that have been made under the proper precautions against error are those of Soerensen, of Copenhagen. They are as follows:—Of 51 cases treated with serum there died 17. Of 46 cases treated without serum there died 15; mortality in each case, 33 per cent. These are the only comparative figures existing on which the slightest reliance can be placed. To show the absurdity of making any comparison without regarding the sources of fallacy indicated above, I shall prove:—Firstly: That serum kills an enormous number of patients. Secondly: That serum saves an enormous number of patients. Taking the figures against antitoxin.—I shall examine first the absolute mortality—that is, the total annual deaths. It should be noted that absolute mortality involves one fallacy less than case mortality—viz., the variation due to place. The other two vitiating circumstances, however, operate in full force. *England and Wales.*—The figures of the Registrar-General show that for each million of the population the average annual deaths from diphtheria were, from 1881 to 1893 inclusive, 188. These were pre-serum years. From 1894 to 1900 the average was 269. These were serum years. *Trieste.*—Germonig gives the following figures of the mortality at the hospital:—

	Years.	Deaths.
Pre-serum years.	1892	59
	1893	87
	1895	297

In 1895 almost every case was treated with antitoxin. *New York, Brooklyn, and Boston.*—Coakley has shown that the absolute mortality in these places after serum treatment was as high as the worst years since 1832.

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	Year.	Deaths.
Pre-serum years	1892	333
	1893	378
	1894	1,027
Serum years.	1895	807
	1896	1,118
	1897	1,905

Basle.—Here there died on an average, from 1885 to 1894, 29 patients. There died in 1895, 65 patients; in 1896, 49 patients. No comment on the absolute mortality is needed. The figures speak. Taking the Case Mortality.—For the sake of comparison we take the mortality obtained by Roux at the Hospital for Sick Children, Paris, when serum was first used. This was 26·0 per cent. Against this we give the following results obtained without serum. Neumeyer treated 1,000 diphtheria patients in 15 years with only six deaths. Mortality, 0·6 per cent. Lueddeckens used cyanide of mercury, with the results shown by the following table:—

Year.	Number Treated.	Deaths.
1893	18	0
1894	14	0
1895	32	1
1896	17	0
Total	81	1

Mortality, 1·23 per cent.

Neumann, of Potsdam, gives the following figures, obtained without serum, and compares them with serum therapeutics:—

Year.	City Infirmary.	St. Joseph's.	His own Practice.
1894 - -	35·5 per cent.	36·9 per cent.	·6 per cent.
1895 - -	9·0 ,,	14·8 ,,	3·3 ,,
1896 - -	14·3 ,,	2·3 ,,	·0 ,,
1897 - -	12·0 ,,	7·3 ,,	·0 ,,
1898 - -	8·4 ,,	11·1 ,,	8·0 ,,
1894–98 -	Average, 15·4	Average, 13·6	Average, 1·6

Göttstein and Schleich quote the statistics of Feer, who in 4,000 cases from 1875 to 1891, had a mortality of 12·7 per cent. Aub has shown that at Munich the mortality for six years oscillated between 6 and 14 per cent., without serum. Hulol and Goubeau, employing perchloride of mercury, report a mortality of 4·7 per cent. Kastorsky, using an alcoholic solution of menthol, treated 37 successive cases without a death. This treatment yielded the same good results to Trutovsky. What shall we say of these figures? What shall we say of the enormously increased totality of deaths since the introduction of serum? What shall we say of the low mortality shown in many cases without serum? If we take up the utterly indefensible position of the antitoxin champions in ignoring the sources of fallacy, we must conclude that serum kills an enormous number of patients. The believers are hoist on their own petard. Having proved that antitoxin kills, we can just as easily prove that it saves. We do this in the same manner—by refusing to consider the factors which falsify our conclusions. Taking now the figures in favour of serum, I have mentioned previously that medical opinion in England has been moulded by one set of statistics. They are those of the Metropolitan Asylums Board. I give them:—

	Year.	Mortality.
Pre-serum Years.	1889 - - -	40·74 per cent.
	1890 - -	33·55 ,,
	1891 - - -	30·31 ,,
	1892 - - -	29·51 ,,
	1893 - - -	30.42 ,,
	1894 - - -	29·29 ,,
Serum Years.	1895 - - -	22·85 per cent.
	1896 - - -	21·20 ,,
	1897 - - -	17·79 ,,
	1898 - - -	15·37 ,,
	1899 - - - -	13·95 ,,
	1900 - - -	12·01 ,,

We observe here a continuous decline of mortality, with two falls more abrupt than the others, one in 1890 of 7·19 per cent. and the other of 6·44 per cent. in 1895. These figures have been triumphantly heralded as proof of the value of antitoxin. Let us ask the serum believers for an explanation of the decline, and note the reply. I put the matter in form of question and answer. *Question:* What was the cause of the continuous fall from 1889 to 1895? *Answer:* Natural causes—decreasing virulence or increased number of mild cases sent to hospitals. *Question:* What was the cause of the sudden decline of 7·19 per cent. in 1890? *Answer:* As before. *Question:* What was the cause of the continuous fall from 1894 to 1900? *Answer:* Serum. *Question:* What was the cause of the fall of 6·44 per cent. in 1895? *Answer:* Serum. *Question:* What evidence have you that the decline after 1894 was not due to the same causes as the decline in previous years? *Answer:* We offer you the testimony of our inner consciousness. In relation to the much-quoted figures of the Metropolitan Asylums Board for the years 1889 to 1900, let us look at the figures of the Metropolitan Asylums Board for 1903. I quote from the "Hospital" of August 20th, 1904: "It is to be observed that out of 530 cases of diphtheria admitted to the North-Western Hospital 172 were treated without antitoxin, and they all recovered, while of the remaining 358 treated with antitoxin 42 died, and of the entire 5,422 cases treated in the Board's hospitals 4,139 were treated with antitoxin, of whom 10·18 per cent. died, whereas of the 583 cases treated without antitoxin only 1·88 per cent. ended fatally." More figures can be given in favour of serum. In several places a fall in case mortality was noticed after the introduction of antitoxin. The most quoted is that observed at the Hospital for Sick Children, Paris. There the average mortality for four years had been 51·7 per cent. In the first five months of the use of antitoxin the figures fell to 26 per cent. During the same period at the Trousseau Hospital, Paris, the mortality was 63·2 per cent. without antitoxin.

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Hospital for Sick Children.		Hospital Trousseau.
Time.	Mortality.	Mortality.
1880–1893 - -	51·7 per cent.	—
Five months of 1894	26 per cent.	63·2 per cent.

At first sight this looks very convincing in favour of serum. But when we have looked at all the factors we shall see that the comparison is worth nothing. To compare the mortality of one period of time with that of another we have abundantly proved to be useless. I have already quoted examples where two places side by side showed differences of mortality greater than in this instance, without difference of treatment. These criticisms are far from being all. At the Hospital for Sick Children the statistics were under the care of Roux, the bacteriologist of the Pasteur Institute. The cases were selected. Roux admits that he suppressed twenty cases followed by death before treatment could be administered. If included, his mortality would have been 33 per cent., and this was not the only way in which his statistics were helped. All sore throats showing the Klebs-Löffler bacillus were included in the figures. All without it were excluded. We have already seen that this bacteriological diagnosis brings an adventitious number of mild cases into statistics, and excludes severer ones. Roux's figures are vitiated by all the fallacies we have considered. (1) The fallacy of time. (2) The fallacy of place. (3) The fallacy of excluding severe cases. (4) The fallacy of including an increased number of mild cases. The Commission have now the evidence before it, and must judge for itself whether antitoxin is of value, or whether it is to be classed with those remedies for diphtheria which "were first greatly vaunted, then fell into disuse . . . . and a few years later were introduced again as novelties, with the same extravagant praise and a similar list of successful cases." I conclude with an example of this praise: "The terror inspired by diphtheria has been banished, as that inspired by small-pox has been banished by vaccination."

13245. (*Mr. Ram.*) What is the date of this pamphlet?—The date is 1904, but it has been somewhat brought up to date recently. It has been recopied from the volume "Fruitless Experiments," published in 1904, with some few additions to it.

13246. (*Sir Mackenzie Chalmers.*) The first edition was in 1904?—Yes.

13247. It has been republished with corrections in 1905 or 1906?—I think in 1906. I should like though to just point out one fallacy at least, which is so constantly occurring in these statistics of diphtheria.

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13248. (*Chairman.*) Is this contained in the passage you have just put in?—Yes, but it applies to Dr. Martin's figures, and the fallacy of Dr. Martin's figures could only be seen after very careful examination of my statement, and might be overlooked.

13249. Will you just refer us to Dr. Martin's statement, and deal with that separately, because we cannot have it both ways?—It is on page 215 of the Third Report of this Commission. Firstly, taking the figures of the Metropolitan Asylums Board, he has put these figures previous to the introduction of the antitoxin in a misleading manner. He has lumped the years 1888 to 1893, and, as you will see, I have given the figures for each year instead of lumping them. By lumping them he leaves out of consideration the fact that there had been from year to year a decrease—that the mortality was already on the decline every year, which might be inferred but certainly is not apparent from the way in which Dr. Martin has given the figures.

(*Mr. Ram.*) There was not a universal decrease; there was a rise again in the year 1893.

13250. (*Chairman.*) But I think with some slight variations it steadily declined?—Yes, that is so.

13251. (*Sir Mackenzie Chalmers.*) Then, as soon as you get to the serum years it goes down with a jump?—I point out that there was just as big a jump from 1889 to 1890 as there was in the first year of the serum. But I wish to point out one fallacy for this reason, that it is a fallacy occurring over and over again, and always coming up, which nobody seems to have recognised, which fallacy is in a lot of the figures which Dr. Martin has given here. If you take the mortality in 1895 it is 22.8 per cent. If you take it in 1896 it is 21 per cent. That is in the figures of the Metropolitan Asylums Board. Everybody assumes, and we are always asked to take for granted, that this reduction of practically 2 per cent. has been due to the antitoxin, but everybody seems to overlook that you are comparing one antitoxin year with another antitoxin year, and if you keep on like that your error becomes greater and greater as you go along.

13252. I do not follow. Which is the first error?—The first error is that 1895 was an antitoxin year, and that 1896 was also an antitoxin year, and that by comparing an antitoxin year, 1896, with an antitoxin year, 1895, it is assumed that the improvement in the figures was due to the antitoxin. For the life of me I cannot see why it should be. You are comparing it with another antitoxin year.

13253. (*Mr. Ram.*) But you find a rise afterwards in 1901?—That shows that there must have been some other cause.

13254. They are both antitoxin years?——

13254A. (*Chairman.*) No doubt the figures do not absolutely prove one thing or the other, but the suggestion on the other side is that if you find antitoxin being taken up and used more and more in hospitals you find a greater decrease?—I do not think that has ever been shown. I do not think that it is because a larger percentage of patients have been treated by antitoxin. I do not think that has ever been alleged.

13255. I cannot say speaking from recollection. I am only pointing out that there may be an explanation on both sides; on the one that this is a natural decrease going on which would have gone on without the serum, and on the other that the serum, having been introduced for the first time in 1895, the probability is that it was not introduced so fully in the first year as it became afterwards?—I think it was pretty fully introduced in the year 1895.

13256. On the other hand these gentlemen will say that the decrease was not a natural one. I am not deciding which way it was; I am only pointing out that we have these figures before us and we have had these explanations on both sides before us. Is that your evidence upon the diphtheria antitoxin?—Yes, it is.

13257. Is there anything you wish to add?—I am sorry that the Commission do not wish to hear me fully on the question.

13258. You really must not say that. No witness has been invited to read a whole pamphlet to us?—Quite so. I quite admit that it is very long and it may be exceptional.

13259. We cannot hear it both ways. We cannot have the whole pamphlet printed and at the same time hear the substance of it given orally. We gave you the choice?—If I make certain criticisms now on Dr. Martin's evidence before I have pointed out certain general fallacies, I do not see how the Commission will understand it.

(*Sir John McFadyean.*) Might I suggest to the witness that he might keep his mind easy about Dr. Martin's statistics, because some of us are pretty certain to ask for his criticisms upon some of those statistics which do not seem to be touched, and he may have abundant opportunity to disclose the particular fallacies contained in Dr. Martin's statistics.

(*Witness.*) It is that opportunity that I want.

13260. (*Chairman.*) That you will have in questions put to you?—Then I shall be quite satisfied.

13261. (*Sir William Church.*) I think in your *précis* you have referred to this book that you have published, "Scientific Research"?—Yes, I am the author of it.

13262. It was, I suppose, written for the purpose of informing the public of what went on in physiological and pathological laboratories?—Yes.

13263. What is the meaning of this: "A View from Within"—that is on the cover and also on the title-page inside?—It means a view from a person who has been inside a laboratory, a view from inside the laboratories by a person who has been inside a laboratory.

13264. I presume that is what is meant; a person who is conversant with all the work that goes on in laboratories?—Yes.

13265. But then from your preface in this book it does not appear that you were very conversant with all the work that goes on in laboratories. Have you ever worked in a physiological laboratory?—Yes, I have.

13266. Where?—At Strasburg and Brussels.

13267. You were a month in Brussels. You said in your evidence, I think, you were a month in Brussels?—Perhaps I did; I was about a month in Brussels or more. I was several months in Brussels, as a matter of fact.

13268. I thought you said also that you worked a month at the Pasteur Institute?—Yes.

13269. And at Strasburg?—I was about three months at Strasburg.

13270. When working in these institutes were you regularly working in the laboratory at any special point?—At that time I had some idea of going up for the fellowship, and I was working on the muscles of dead frogs. I did no experiments on living animals myself. I did the common experiments on dead frogs.

13271. Have you ever worked in any English laboratory?—No, I have not.

13272. Are you conversant with English laboratories?—No, I am not.

13273. Then should not you have put on this book "A View from Within of the Strasburg and Brussels Laboratories," so that the public should have known what laboratories you had a knowledge of?—I presume that if the public see the outside of a book they will likewise read the inside of the book. I did not see any necessity to put it on the outside of the book.

13274. But it is in the inside also, on the title page?—I presume that, if people read the book, they would get a fair idea of what the book is from reading it. I did not attempt to put a full description of the book on the outside.

13275. I think you were a Licentiate of the Society of Apothecaries first of all?—Yes.

13276. About 1884?—About 1884.

13277. You were then in practice between 1884 and 1900?—Yes, I was, to 1899.

13278. Then you retired from practice?—Yes.

13279. And during those 16 years you were not working in either a foreign or an English laboratory?—I was in general practice from 1884 to 1899.

13280. Still, you had no practical experience then of what was going on in laboratories, either foreign or English?—No.

13281. And, in fact, the whole of your experience has been a month at the Pasteur Institute, a month at Brussels, and a month at Strasburg?—I was about three months at Strasburg.

13282. But there you were not working in labora-

tories; you were only just attending?—No, I was working in the laboratory.

13283. At some special subject?—On dead frogs.

13284. (*Mr. Ram.*) Did you ever experiment on any living animal?—I never operated on a living animal in my life.

13285. (*Dr. Gaskell.*) Was that in Goltz's laboratory?—Yes.

13286. (*Sir William Church.*) And on that experience of yours you think that you are entitled to say you were in a position to draw back the curtain disclosing the machine?—Yes, I think so.

13287. I am referring to what you say in the preface: "A celebrated lawyer addressed a jury at length to prove that the wheels of a certain machine could not possibly be more than a certain size. His opponent was expected to argue at length in reply. Instead of doing so he merely drew aside a curtain disclosing the machine and remarked—'Gentlemen, there are the wheels'" ?—Yes.

13288. Considering that this was for the English public, that is rather a strong thing to say by a person who has never worked in English laboratories, is it not?—It does not refer to English laboratories. A book speaks for itself. The preface does not speak for the whole book. The book itself must speak for itself.

13289. I am only alluding to that passage in the preface?—The book speaks for itself. I did not intend that a person should read the preface only without the book and form a wrong judgment from the preface.

13290. Then turning to the book, that being the case, I should like to ask you this: You were working only on frogs abroad. On page 47 of your book you definitely make these statements. You say, "Curare is used daily throughout England." Do you think that you were in a position to say that with your experience?—Yes. I did not say that it was from my personal experience.

13291. As you told us to-day in your evidence, in five years you have only been able to find about 100 instances of its use?——

(*Sir John McFadyean.*) No, about 10 per cent.

(*Witness.*) The reason is this: I have taken, as I have mentioned already, the "Journal of Physiology" for five years and I found that the proportion of cases of physiological experiments in which curare is used, and inasmuch as from that it appears that in cutting experiments it is used in 10 per cent., if it was used in 10 per cent. of these purely physiological experiments, it was not certainly an exaggeration to say that it was in daily use. That would practically amount to 7 per cent. of the physiological experiments in England for a year, and take 7 per cent. of those I think you would have more than one a day under curare.

13292. (*Sir Mackenzie Chalmers.*) That is the inference which you draw from the fact that in 10 per cent. of the cases reported in the "Physiological Journal" curare was used?—Yes.

13293. (*Sir John McFadyean.*) Along with anæsthetics?—Yes.

13294. (*Dr. Gaskell.*) Ten per cent. of the cutting cases?—Yes, 10 per cent. of 71 per cent.

(*Sir Mackenzie Chalmers.*) I do not think that follows myself.

13295. (*Sir William Church.*) Of course this book is for the public, and purports to be written by a person daily working in pathological and physiological laboratories, and as an authority. With regard to the use of curare, on page 32, you give us your opinion, the result, of course, of observation of your own—"That Bernard was right in believing,"—curare—"to greatly intensify pain"?—Yes.

13296. But have you ever worked on the subject?—Worked on what subject?

13297. On the subject of trying to find out whether curare greatly intensifies pain?—No, I have not tried to find that out.

13298. Therefore you are merely repeating what other people have said?—Yes.

13299. You give it, as you say, "That Bernard was right in believing it to greatly intensify pain is clear also"?—I do not know what the context is there. The word "also" refers to some context which you have not given me. If you give me the context I shall see the reason for it perhaps.

13300. The whole context is this: "We see clearly that the opinion of Bernard and all physiologists is correct—that curare is not an anæsthetic. That Bernard was right in believing it to greatly intensify pain is clear also." That is your statement on page 32, Chapter IV. Therefore you had no reason of your own for that definite statement?—Yes, I certainly had.

13301. It is only repeating what you thought was the opinion of others?—I am repeating the opinion of Claude Bernard; but I do not know whether I put in the context or not the reason for believing that it intensifies pain. "We see clearly that the opinion of Bernard and all physiologists is correct—that curare is not an anæsthetic. That Bernard was right in believing it to greatly intensify pain is clear also." Then what follows goes on this way: "For the motor tracts being shut off, the whole of the stimulus goes to the sensory centre." That is the reason for the statement.

13302. But that is not from any work of your own?—No, it is not. I do not profess it to be. I never said that it was.

13303. Then you go on to mention, if you will turn to page 36, that you saw an experiment at University College, London, on October 19th, 1900, by Professors Starling and Bayliss. This is also with regard to what you say about curare. You say: "When I first saw the animal (a small dog), he was motionless under curare. A hole had been cut in the windpipe, and the bellows kept up artificial respiration. At each stroke of the bellows a little ether was pumped in with the air—a much feebler blast than that used at Strasburg." How did you arrive at that knowledge?—What knowledge do you mean?

13304. That "a little ether was pumped in with the air—a much feebler blast than that used at Strasburg"?—By the sense of sight, and the sense of smell

13305. Nothing further than you thought that because you did not smell more ether?—No. The sense of smell was as to the nature of the anæsthetic.

13306. And the amount of the anæsthetic?—And the amount of the anæsthetic by the bellows and the bubbling. By watching the apparatus.

13307. You judged it then without either inquiring from Professor Starling or Professor Bayliss of the amount, or perhaps even seeing signs of Woulff's bottle or anything?—The bottle was in sight. At Strasburg they had the waterworks. There was a very powerful blast of air sent through the ether, and it was simply a matter of eyesight and observation that at University College the blast was much feebler.

13308. (*Sir Mackenzie Chalmers.*) Was it a lecturer's experiment or a scientific experiment?—After the lecture certain senior students were called in to see the experiment.

13309. (*Sir William Church.*) You really had no knowledge at all?—It was not a question of knowledge. It was a question of the eye.

13310. (*Chairman.*) I understand you to say that you measured it by your eyesight and your memory of what had taken place at Strasburg?—Yes.

13311. In neither case did you ascertain what the precise quantity or strength was?—No, not exactly. It was very easy to see. If I saw some person pouring water out of a glass and another out of a pail I should know that more water was poured out of the pail than out of the glass without measuring in either case.

13312. Do you wish to compare those two cases?—No, it is an exaggerated comparison.

13313. (*Mr. Ram.*) It is your suggestion that the animal was not sufficiently anæsthetised. Is that what you mean?—The suggestion is that it was very probably insufficiently anæsthetised.

13314. (*Sir Mackenzie Chalmers.*) Was the animal killed at the end of the experiment?—We were turned out of the room after being there about half an hour. I saw nothing more of the animal.

13315. (*Mr. Ram.*) How do you mean that you were turned out?—Well, it was time to go.

13316. (*Sir William Church.*) Here you say definitely (and this is for the information of the general public that this book is written) that morphia is not an

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anæsthetic. Surely as a surgeon you do not mean to tell me that you cannot render an animal unconscious with morphia?—When I wrote this book I made that statement on certain authorities. One was the authority of Dr. Mitchell Bruce in his work on "Materia Medica," and Sir Lauder Brunton, who classify morphia among the narcotics.

13317. That is quite a different point?—I was giving you my reason for making the statement. Another reason was the statement by Professor Rutherford before the last Royal Commission, quoting the opinion of Claude Bernard that in animal experiments morphia was not an anæsthetic. Those were my reasons for making that statement in this book. Since then I have had reason to believe that morphia given in a lethal dose may cause unconsciousness.*

13318. But you would not tell the Commission, as some witnesses have, that you cannot render an animal unconscious with morphia?—Since I have written this book I have come to the conclusion that morphia may be used as an anæsthetic in a lethal dose.

13319. There is one other question that I should like to ask you with regard to this book. You say on page 36: "It is clear that in curarised animals anæsthesia is a practical impossibility." This is a book for the general public?—Yes.

13320. What really did you mean by that?—I will repeat what I said a little while ago were the reasons, which I have already given you; but as you ask for it again I will give it again; that is to say that when curare is given the guides which exist in anæsthetising a human patient are absent.

13321. But surely has not the person who is giving an anæsthetic to a curarised animal a very much better guide by knowing the quantity of anæsthetic he is administering?—No, the quantity of the anæsthetic is an unsafe guide, because my observations have been——

13322. But you have never made any observations?—I have seen experiments.

13323. That is a different thing from making experiments yourself?—That is just as good. Looking on at an anæsthetised animal is just as good to me for observing as if I gave the anæsthetic myself.

13324. Unless you have really made inquiries you do not know what amount of anæsthetic the animal is receiving?—Inasmuch as I have seen animals anæsthetised and observed the effects of a fixed quantity of anæsthetics upon these animals, I was in a very good position to judge.

13325. I will not ask any further about that. I should just like to ask you one or two questions on your evidence which you have given to-day. Was anything known as the cause of myxœdema before experiments were made on animals?—Yes, it was known from clinical experience that after the removal of the thyroid, symptoms of myxœdema arose.

13326. Even granting that, does that tell us the cause?—It may not tell us everything, but it gives us a very good idea that something was elaborated in the thyroid which in healthy people prevents myxœdema. That is the inference.

13327. You say it is so simple that one who runs could read?—Yes.

13328. But simple as it was it had never been discovered?—What had never been discovered? You say "it." What do you mean by "it"?

13329. I mean that the importance of the thyroid gland in our economy had never been discovered before experiments on animals were made?—Excuse me. It had been discovered by these operations on the thyroid in which the thyroid was removed and myxœdema supervened. So that this much was discovered by those clinical observations that something was elaborated in the thyroid which prevented myxœdema. If nobody ever thought of that I can only say that everybody was very stupid; it seems to me so simple.

13330. Nothing was discovered. It was observed that in certain cases this condition did follow the removal of the thyroid, but it was not known at all why it did follow?—Excuse me, I think it was known from a reasonable inference, because it was the absence of the thyroid.

13331. (*Sir John MacFadyean.*) Was the healthy thyroid ever removed in any of these experiments?—I do not think so.

13332. How can you infer that it was the absence of the thyroid. It might have been an antecedent disease of the thyroid?—For this reason: that although there had been some hypertrophy of the thyroid or some of the tissues, part of the thyroid, at all events, there was no myxœdema, but when the thyroid was removed there was myxœdema.

13333. That only proved that hypertrophy would not cause myxœdema, but I do not see that excising a diseased gland could prove that the absence of normal thyroid would cause myxœdema?—It seems to me a very simple deduction, that if with thyroid of any kind, whether diseased or not, you have got first no myxœdema, and then, if you remove the thyroid and myxœdema supervenes something has gone. It seems to me a simple deduction.

13334. (*Sir William Church.*) Even that would not have given us the knowledge of the remedy how to treat it. It was the finding that removing healthy thyroids in animals produced symptoms similar to myxœdema in man that led us to think that it might be applied artificially by feeding with thyroids?—You evidently do not agree with me that it was a simple deduction from the removal of a hypertrophied thyroid that it was simply the absence of the thyroid which was the cause of myxœdema. I say that was a simple deduction; you say it was not. The only comment I have made upon that, is that if that was the case with those people at that time, they did not show much intelligence.

13335. It was a deduction that was not made, anyhow, until after experimentation on animals?—That may be, that is quite possible; but if the deduction was not made I maintain that it showed gross stupidity.

13336. I should like to ask you one or two questions on diphtheria. Do you believe that there is such a specific disease as diphtheria?—Yes, certainly.

13337. I do not gather from what you were reading about the different conditions whether you did or did not. You do think that there is a specific disease called diphtheria?—Well, I do not know whether there may not be some varieties. That is a very difficult question. I do not think that anybody could answer that question whether there may not be some varieties.

13338. But still, there is such a disease?—There is such a disease as diphtheria.

13339. Which can be communicated?—From one person to another.

13340. Which can be communicated to animals?—I think the evidence is not sufficient to show that the disease which is communicated to animals is really the same disease.

13341. You are not clear that what is communicated to animals is diphtheria?—No, I am not.

13342. But that also is a specific disease which kills the animal?—Undoubtedly.

13343. And that can be communicated to animals?—Yes.

13344. Do you know how antitoxin is standardized?—No, I do not.

13345. And yet you give an opinion about the value of the antitoxin?—Yes.

13346. Then you are not aware of how the antitoxin is standardized?—No, I do not know how it is standardized.

13347. And how you have to find out what quantity of the antitoxin protects an animal from this communicable disease. You were not aware of that?—I was aware, generally, that there was something of the kind, but the details of these animal experiments I am not acquainted with.

13348. But you were aware that there was something of the kind?—I was aware that there were experiments.

13349. How do you explain to yourself that an animal that is poisoned with toxin, if it receives the antitoxin in sufficient quantity takes no harm?—I am unable to give any explanation of it.

13350. You do not deny that it is the case?—No, I do not deny it.

13351. But you give no explanation?—No.

13352. Have you had any experience of diphtheria yourself?—Oh, yes, a certain amount as a general practitioner.

* *See Question 4133 of Evidence before Royal Commission on Vivisection, 1875.—S. F. S.*

13353. Before or after the introduction of the antitoxin?—It would be chiefly before.

13354. You have had none since?—No.

13355. Have you had much experience of diphtheria?—Well, after having been in general practice about 15 years I suppose I must have seen a good many cases. I could not say how many.

13356. Did you often have to do tracheotomy?—I never had to do it once.

13357. Then you were fortunate in not seeing very severe cases?—Yes.

13358. Could you mention to the Commission the name of any one who has had a large experience of diphtheria, before and after the introduction of the antitoxin, who thinks it is useless?—You mean any individual?

13359. Any individual who has had individual knowledge and large experience both before and after the introduction of the antitoxin, who thinks that the antitoxin is useless?—I would mention Soerensen of Copenhagen, who made some very interesting experiments at Copenhagen. He recognised all the fallacies which exist underlying these diphtheria statistics: the fallacy of time; the fact that the virulence varies from time to time; the fact that the virulence varies in two places at the same time; and also he recognised the factor that there has been a selection of cases. Soerensen guarded against these as far as possible. He treated certain patients in the same hospital at the same time, and he guarded against a selection of cases, and he got about the same percentage of mortality in each case.

13360. He was in charge of the diphtheria hospital?—I presume he was physician to the hospital.

(*Sir John McFadyean.*) Could you give us the number of cases and the date of his report?

13361. (*Sir William Church.*) I was unacquainted with his observations. I do not know his name. Where has he published it?—I have taken that apparently from "Die Diphtheriekrankeit" by Buning of Haarlem.

13362. (*Sir John McFadyean.*) What is the date?—I cannot give the date at the present moment, but it is about 1900, somewhere about there, approximately.

13363. Cannot you give us the number of cases on which Soerensen's conclusions were based?—Yes, I think I can. I am just trying to find it.*

13364. (*Sir William Church.*) You have not given the date in the Bibliography at the end of your pamphlet, so perhaps you have not got it?—I have the book at home somewhere.

13365. It does not matter in the least.

13366. (*Sir John McFadyean.*) In your evidence you offered some opinions with regard to the difficulty of keeping wounds inflicted on animals surgically clean. Is that opinion justified by your own experience?—It is only as regards my own dog.

13367. It will probably satisfy me if you will say that it is founded on observations made on your dog?—Yes.

13368. You have no extensive experience?—I have no extensive experience on that point.

13369. Would you abandon your present opinion if you were told on good authority that operations of most serious kinds on the lower animals are performed in thousands, including opening the abdomen, in veterinary practice, and that the wounds heal by first intention just as in human beings?—I think it is possible.

13370. Does not that dispose of your contention that in inoculating animals for physiological experiments it is difficult or impossible to ensure the wounds healing by first intention?—You said "veterinary practice" just now. I was speaking more particularly of dogs in physiological experiments. If a dog can get at the dressing or tear it off with his paws it will do so.

13371. You do not suggest that a dog behaves differently to a physiologist and to a veterinary surgeon assuming that the wound is the same?—No, I do not, of course.

13372. Will you take it from me that in a veterinary practice dogs frequently have extensive recent wounds inflicted upon them left uncovered, and that they display no tendency whatever to tear them open?—If a veterinary surgeon told me that from his experience I should accept it.

13373. You find nothing incredible in that?—No.

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13374. It is simply in agreement with the fact that aseptic wounds of human beings are comparatively painless?—Yes.

13375. You also offered us some opinions with regard to the difficulty of anæsthetising dogs. Is this your own dog again that has been anæsthetised?—No.

13376. Will you tell me how many dogs you have anæsthetised or seen anæsthetised?—About seven or eight.

13377. I notice that you said in a paragraph of your *précis* "*The uselessness of vivisection.*" Am I to understand that you contend that absolutely nothing useful for human or veterinary practice has ever been learned by vivisection?—I have not gone over the whole ground.

13378. Why did you head it "uselessness"?—The "uselessness" referred to what is contained in the rest of the *précis*.

13379. It is to be restricted to the special diseases that you deal with in your *précis*?—Yes, which I have examined.

13380. You do not offer any opinion with regard to other alleged instances of benefit to men and animals derived from vivisection?—No, because I have not examined them.

13381. We have had evidence given to us here, I may say, that a great deal of useful knowledge for the treatment of farm animals has been got from vivisection. Are you disposed to deny that?—I am disposed to regard it with very grave suspicion from my examination which I have made on these subjects. When I have seen the confident statements that have been made and have examined the experiments and found them so different from the confident statements, I gravely suspect the statements

13382. I would like to go in a little detail into any one of the cases that were mentioned, if you will tell me what are the grounds for your suspicion that the facts are misrepresented, or the conclusions not well founded. Take any animal disease that you like as to which you say you have gone into the evidence. Or perhaps I will suggest one or two to you; but I would rather you would name the animal disease yourself?—Are you speaking of animal diseases.

13383. Yes, certainly?—I beg your pardon; I misunderstood you. I thought you were referring to human diseases.

13384. No, to animal diseases?—I know nothing about animal diseases.

13385. You are not disposed to deny that it may be a fact that knowledge of great value for the treatment of animal diseases has been obtained by experiments upon animals?—I would rather not express an opinion upon that because I know absolutely nothing about it.

13386. But you do hold a very strong opinion that drug experiments on animals intended to throw light on the action of the drugs on men are useless. You have expressed that view?—Yes.

13387. You have cited some instances in which particular agents, poisons or medicines, have a different action on men and on some of the lower animals?—Yes.

13388. Do you want this Commission to believe that that is the rule or the exception?—I wish the Commission to believe that there are a very large number of drugs in which this does occur, and supposing it could be shown that there are 10 times as many cases in which the drug agrees as in which it disagrees, I say if in one-tenth it did not agree it would be very dangerous to argue from animals to man.

13389. But will you answer my question, please. Do these cases that you cited to us constitute the rule or the exception. My point is this. You contend that in a general way it is useless to make drug experiments on animals intended to throw light in advance on the action of these same drugs on man, and you cited to us some admitted instances in which the drug acts differently on the lower animals from what it does on man. I ask you whether you wish the Commission to believe that these cases represent the rule or constitute the exception?—I could not tell you that. It is quite possible that it may be in a great minority of cases.

13390. If you had answered otherwise I intended to ask you how you would explain the extraordinary fact that the veterinary pharmacopœa is practically the same as the human except with regard to doses. Are you aware of that?—No, I was not aware of that fact.

* *See Answer to Question 13344, p. 15. S.F.S.*

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13391. You would not for a moment deny that?—If you say so I would not.

13392. I am not giving evidence, but I do say so?—Then I do not deny it.

13393. Coming to these specific cases in which you allege that unfounded claims have been made as to the benefit derived from vivisection, would you kindly turn to pages 223 and 224 of the Report of the evidence already given to us. You have got, I think, the Third Report there?—I have the page here.

13394. I want to direct your attention to this, because you quoted to us to-day some statistics on, I think, a very limited scale, and already not at all recent, from a book published by Dr. Melville. I want you to notice that the statistics given on page 224, if you look at that in the first place, relate to a large number of persons uninoculated and inoculated, there being among the uninoculated 10,000 and among the inoculated 1,700, and that the percentage of incidence of cases among the uninoculated was 14.14, and among the inoculated 2.05. Would you advise the Commission to disbelieve these statistics or to give no weight to them when they are asked to form an opinion as to the value of inoculation against typhoid fever?—The figures, as they stand, are certainly strongly in favour of it, but judging by the analogy of statistics in general, I should not like to say, off hand, that there might not be some vitiating factor.

13395. But that is not the question. Would you suggest to us what the vitiating factor is?—No, I am not able to do that.

13396. Would you give the same answer with regard to those figures on the preceding page, where there is an equally great difference with regard to the deaths among the inoculated and the uninoculated?—On the preceding page there are three.

13397. At the bottom of the page, I mean. These were officers, non-commissioned officers and men in the same regiment at the same place. There were 317 inoculated and two cases, and 164 not inoculated and 14 cases?—Two cases of what?

13398. Typhoid?—I see.

13399. Assuming that you find that these figures are actually reliable, would you admit that this method of inoculation appears to be of value?—Yes, I am bound to admit that—that it does appear to be of value.

13400. Now, would you turn next, please, to page 214. I want to ask you a question about rabies. Would I be right in saying that your case—as against the contention that the Pasteurian treatment of rabies is successful—is founded on the contention that Pasteur's statistics as to the mortality among untreated bitten persons are entirely fallacious?—Yes.

13401. You did not quote to us, so far as I could see, any statistics at all, but you entered into somewhat elaborate reasoning, from which you inferred what was the number of bitten persons in France in a year?—Yes.

13402. And you said that these were the largest figures available?—I think so.

13403. Will you please look at page 214?—Yes.

13404. We had this evidence presented to us relating, as you will see, to 3,127 tabulated cases of persons who were bitten by dogs reported to be rabid, and showing that the average mortality in these 3,000 odd cases was 16 per cent.?—Yes.

13405. Will you point out to the Commission where there is any probable fallacy in these figures?—Yes, I will point it out. The probable fallacy is that owing to the habit of killing the dog for fear it should bite other people, it was not then ascertained whether the dog was mad or not. Now, assuming that a dog has bitten four or five people and nobody contracts hydrophobia, the probable conclusion would be that the dog did not have hydrophobia, and those cases would be excluded from the statistics.

13406. But why? I cannot follow. Why should they be excluded? They are bitten persons?—Yes.

13407. Why should they be excluded?—Owing to the comparative rareness, and the fact that these cases are scattered about, you would have selected cases there where there was a very strong belief that the dog was mad, or some reason of that kind.

13408. I do not follow?——

13409. (*Mr. Ram.*) Why should they be excluded?—To take a specific instance, supposing that a dog runs round and bites half-a-dozen people. Somebody suspects "That dog is probably mad. Let us kill him before he bites somebody else." The dog is killed. There is a doubt about it whether the dog is mad or not. Nobody gets hydrophobia. The conclusion is that it was not a mad dog. These figures would naturally be excluded from the statistics; they would not find their way in.

13410. (*Chairman.*) But means would be taken to ascertain whether any person had got hydrophobia, and a report would be made long before that?—But so far as I know there is no evidence to show that these were made on every dog-bitten person.

13411. They profess to be?——

13412. (*Sir John McFadyean.*) I do not see how that vitiates these particular statistics. It might be that they left out a considerable number of bitten persons, but the fact would remain that there, among 3,000 persons bitten by dogs, some of which would probably be rabid (but we will assume that they were all rabid), 16 per cent. died?—Yes.

13413. Where is the fallacy in that in assuming somewhere about 16 per cent. as being the average. Because it is not a small number; it is 3,000 cases?—Taken from a great many places all over the place.

13414. But still 3,000 cases. The fact of the locality does not make it objectionable to include them in the statistics?—Not at all. But on the other hand there is a strong probability here that these were selected cases of bites.

13415. How would you select a case? Assuming that they are dogs actually rabid, in what sense could they be selected cases?—That you would get all those cases selected where the patient had hydrophobia. You get a dog that runs round and bites somebody. One of the persons gets hydrophobia. That is now certain. Those bites would be included?

13416. Yes?—If you got another case where nothing happened you would have those excluded from your statistics.

13417. But you do not discriminate between the probable danger of the bite in those two cases so far as one could see. These, after all, you will admit, will probably represent 3,000 mad dogs or 3,000 persons bitten by mad dogs?—Dogs mad and supposed to be mad.

13418. I have not been able to follow your reason why we should adopt your estimate of less than 1 per cent. when you have no precise figures to offer us at all?—Are you referring now in connection with the text book statistics?

13419. I mean this: Do these statistics on page 214 appear to indicate that out of 3,000 persons bitten by mad dogs, selected or not selected, 16 per cent. may die from rabies if they are not treated?—16 per cent., yes.

13420. They do appear to show that?—Yes.

13421. I am quite content with that?—At the same time I hope it will be recorded that I have pointed out the great fallacy in that connection.

13422. I hope that everything you said will be reported. I have no reason to suppose that it will not Will you turn next, please, to page 218. I want to call your attention to some statistics which have been put before us with regard to the efficacy of the anti-diphtheritic serum. You entertain no confidence whatever in this anti-diphtheritic serum?—None at all.

13423. You actually believe that it is a bogus claim that the diphtheria bacillus has anything to do with it?—It is not on the theory; it is on the statistics.

13424. That is your belief?—Yes.

13425. Would you explain to the Commission the extraordinarily different effect which this serum appears to have according to the period after the onset of the disease at which it is administered?—Yes.

31426. You will observe that when it is administered on the first day the mortality among persons receiving the serum appears to be, as a rule, very slight?—Quite so.

13427. But it gradually rises with the lapse of time. Will you explain the reason why?—In the first place, as regards these statistics, there is no uniformity, you notice. There is a general increase in the mortality the later the antitoxin is given. But there is no uniformity in them, which, for one thing, indicates that there is some other factor.

13428. But might the other factor be one which on *a priori* grounds would be possible, that the serum

is not always absolutely of the same strength, and we know is not always given in the same dose?—There is no reason that that should not be suggested. But I have not given my reason why these figures vary so. I merely indicate that there is some other factor besides serum, from their want of uniformity.

13429. Might I interpolate a remark there and point out that the varying factor may be in the human system?—Certainly, that is possible. That is one factor.

13430. That does not affect the value of the serum?—No. But now I had intended to point out to the Commission (but the Commission preferred it to be printed without my doing so) that there is a great fallacy in these statistics from the inclusion of selected cases; and I have here a very large amount of evidence to show that since the introduction, immediately on the introduction of the antitoxin, owing to bacteriological diagnosis, a large number of cases of mild sore throats were included as diphtheria. I think that is fairly well admitted, even by believers in the antitoxin. Now in the early stage of diphtheria there is great doubt about the diagnosis. Then in the first few days there are many mild sore throats which resemble diphtheria.

13431. Yes, I quite follow that; that among the patients treated on the first day there would be a proportion of them actually not cases of diphtheria?—Yes.

13432. But observe in some of the instances in order to make this explanation acceptable you would have to assume that none of them were cases of diphtheria. There is no mortality at all in some of the statistics among those that get the serum on the first day?—In a large number of cases?

13433. Yes, a considerable number of cases?—How large a number with no deaths on the first day? Where have you got a case of a very large number?

13434. Perhaps I was wrong in saying that there was a large number, because as a matter of fact I do not think in the particular table on page 218, which shows no mortality on the first day the figures are given, unless it relates to those above. But at any rate in the table above that the number you will see was large in some instances; there were 1,189 cases in one series, and 2,428 in another series; and in that large series there were only 2.2 on the first day?—I give two reasons for explanation. Firstly, at the earlier stage of the disease the greater is the probability of error in diagnosis, and the greater the error in diagnosis the better are the statistics. If a child has a case of sore throat, apparently on the second day it is suspected of being diphtheria, and it is given the antitoxin. The child gets well very quickly, and the result is put down to the marvellous effect of the antitoxin. But assuming that some of these cases are hospital cases you have to look for the reason—what was the reason for delay in giving the antitoxin. There must have been some reason for the delay in giving the antitoxin. The delay may have been that some doctors before giving the antitoxin would wait for the symptoms to be more marked, so as to make the diagnosis sure, while other doctors would say, "I will be on the safe side and give the antitoxin at once"; so you have great fallacies coming in for that reason. If they were hospital patients we may assume that a certain amount of delay occurred before the patient was sent to hospital; therefore you get the worst cases sent in.

13435. And you suggest to the Commission that these two sources of fallacy would account for such a difference as 2.3 mortality on the first day, and 34.1 on the sixth day?—Yes, I suggest that, but I think the Commission will be more likely to believe it after they have read what I have printed in this pamphlet.

13436. In speaking about diphtheria you quoted somebody as having said a few years ago that it was generally admitted that the Klebs-Löffler bacillus is not the cause of diphtheria. Do you want the Commission to believe that among bacteriologists and physicians that is the opinion at the present day?—I think probably not in England.*

13437. In Germany?—I do not know about Germany particularly.†

13438. France. Would I be wrong in saying that among hospital physicians and bacteriologists throughout Europe and the United States of America it is the almost universally accepted opinion that the Klebs-Löffler bacillus is the cause of diphtheria?—I could not say. I think the facts speak for themselves. I do not know the opinions of these men.

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13439. You said there was something humorous about the suggestion that only those cases of disease in which the Klebs-Löffler bacillus was found should be called diphtheria?—I put it in this way: that the bacterium must always be found in the disease; that is Koch's first test. Löffler himself regarded the coincidence of clinical and bacteriological diagnosis as doubtful. This has been affirmed by others. He himself failed to find the bacillus in 25 per cent. of diphtheria cases. Hennig believed the bacterium to be absent in more than 25 per cent.

13440. I do not want to have all that over again, but what is there in the least humorous in insisting that only those cases shall now be called diphtheria in which the Klebs-Löffler bacillus is found?—I think perhaps you misunderstood me. What I said was this. We have the position humorously described by Hanseman: "The Klebs-Löffler bacillus is to be found in all cases of diphtheria, provided we select only those cases in which it is present." There is some humour in that.

13441. Nobody has ever made any such contention. Does this line of criticism apply to the present-day definition of tuberculosis, that it is the disease in which the bacillus of tuberculosis is found? Do you see anything wrong in that definition that tuberculosis is the disease in which the bacillus of tuberculosis is found?—In other words, where all the clinical symptoms exist the bacillus is found.

13442. No, on the contrary, we now know that there are a good many other disease conditions with lesions and symptoms simulating tuberculosis, but we agree to exclude them because Koch's bacillus is not found. Is it not perfectly logical in the same way to agree to exclude all cases which appear at first sight to be diphtheria, but in which the Klebs-Löffler bacillus is not found. Where is the humour in that?—The humour is in this: that bacteriologists have attempted to avoid, that is to say, deny, that these well-marked clinical cases of diphtheria are diphtheria because the bacillus is not found.

13443. In the same way that at present one contends that diseases following very closely and simulating tuberculosis are not tuberculosis if Koch's bacillus, the bacillus of tuberculosis, is not found in them. Is not that a precisely analogous case?—You are rather getting me out of my depth there, because I do not know much about tuberculosis.

13444. Surely not. I thought as you came here to criticise bacteriological work you would at least not lose your depth when I mention an elementary fact in bacteriology?—I am not criticising all bacteriology.

13445. Let us take that for granted?—I do not know anything about tuberculosis.

13446. What bacteriological disease do you know anything about. Do you believe in bacteria as the cause of disease at all?—I think one must believe it certainly, of course, in such cases as anthrax and so forth.

13447. That is good enough. We define cases of anthrax as those in which the anthrax bacillus is found, and it does not matter how like a case may be to anthrax clinically for it not to be anthrax unless the bacillus is found?—I may point out that it is a very sudden jump to suddenly exclude all these cases of clinical diphtheria because the bacillus of diphtheria is not found.

13448. (*Mr. Ram.*) Do you object to experiments upon animals if no pain results from the experiment. I am supposing the case of an experiment the object of which is to elucidate some fact which is for the benefit of humanity—an experiment which causes no pain to the animal?—I should like to say that if I thought all experiments are and would be of such a character that pain would be trivial (meaning by trivial what I call trivial and not what experimenters call trivial), I should cease to be an anti-vivisection agitator.

13449. All the evidence which you gave this morning, I think, of any examples you have seen affected only those animals which were allowed to recover?—You say I gave examples?

13450. You gave examples of animals which had

* *Lennox Browne, "Throat Diseases," 5th Edition, p. 546, says that a large number of practitioners have always denied the specificity of the bacillus, and that the number of them increases.—S.F.S.*

† *Buning in "Diptherickrankeit" quotes Lubarsch, Martins, Hennig, Hueppe, Baumgarten, Zupnik, and others.—S.F.S.*

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suffering inflicted upon them?—Did I? I do not remember.

13451. You gave some examples this morning in the books that you referred to. This book, for instance, which Sir William Church referred to, is full of experiments on animals which are painful?—Yes.

13452. Are you aware that under the Act with regard to cutting experiments the animals have to die under the anæsthetic except in the case of a special certificate being granted to admit of their recovery?—I am aware that there is a law to that effect—that such a law exists.

13453. Do you suggest that there are infringements of that law going on every day in England?—Yes, I do.

13454. Can you give any one instance which has come to your knowledge of an infringement of the law in England?—No, I cannot.

13455. Then on what do you justify your belief that there are many infringements of the law?—My belief is this. I do not believe that the present restrictive law has made any practical modification of vivisection at the present day although it may have modified what vivisectors have disclosed. There are two reasons for this disbelief. In the first place the inspection has been a farce. I consider that any statement to the contrary is an insult to common sense and intelligence. Secondly, obedience to a law merely because it is a law does not exist except in a small percentage of the people. Proof of that exists in the fact that nearly all people will smuggle when they can do so with safety.

13456. Do you put the case of smuggling when a man can do so with safety on precisely the same footing as that of a man of intelligence and education who is bound by law not to inflict pain upon animals or not to do so in an operation without a licence, and so forth?—I have already expressed the opinion that physiologists are mistaken about the pain which they inflict; that they are biassed in their opinion; they think the animals do not feel as they do; and that there is special reason for believing that physiologists hold an exaggerated belief of this insusceptibility of the lower animals to pain.

13457. (*Chairman.*) But would you say that the fact that a certain considerable proportion of the poor and the wretched will steal, and use the knife is a sufficient argument for you to satisfy your mind that a considerable proportion of the physiologists who are under this law break it habitually?—No, I said nothing about stealing and using the knife.

13458. You said the same sort of thing when you said that people would smuggle?—No, excuse me, there is a difference. Where a law exists concerning which the conscience is not exercised, people will not observe that law when they can break the law with impunity.

13459. (*Mr. Ram.*) Then your objections, as I understand them, are twofold, and they go to the root of all your evidence. First that pain is inflicted?—Yes.

13460. If there were no pain there would be no objection?—True.

13461. Secondly that that infliction of pain you believe goes on broadcast because the physiologist has a seared sense of sensibility?—Has blunted sensibilities.

13462. That will do. Whatever their sensibilities were the mischief could only occur in the case of animals not allowed to recover?—Or which were improperly anæsthetised during the operation.

13463. (*Chairman.*) And you say that, although these operations are never carried on in absolute solitude; there are always assistants, and in many cases spectators, and although there is no case that you know of, that you have any evidence about, yet you say it is done every day?—Yes.

13464. (*Mr. Ram.*) Have you read the evidence of the different people who have come here and said that they have operated?—I have not read all.

13465. Have you read some?—Yes.

13466. Do you observe that time after time they say that they have never witnessed any cruelty at all, or any insensibility to the sufferings of animals?—They may have done so.

13467. Do you believe their evidence?—I should disbelieve their judgment as to what pain was. Persons say there was no pain, but I have already given you examples.

13468. If it comes to be a question as many have said, that they have never known an operation performed on any animal that was inadequately anæsthetised, that is a question of fact. Do you disbelieve the assertion because the man is a vivisector?—I should say that it was not necessarily evidence, because I have already shown in evidence that there are people who believe that these movements of the animal are automatic, reflex, and that under curare they cannot tell whether the animal feels or not. I mean to say that they cannot tell whether the anæsthesia is complete.

13469. Do you deny that there are reflex actions under anæsthesia?—No, I think that does occur to a trifling extent.

13470. Then in the absence of the use of curare the animal may be fully anæsthetised and yet may make reflex motions?—I think it is very rare indeed, and I think reflex motions are very slight when they are purely reflex.

13471. (*Chairman.*) But are not these very gentlemen who perform these operations the most competent persons in England to observe reflex actions and to understand them. As to their capability you do not deny that?—I have known competent persons hold such extraordinary ideas about it.

13472. That is not answering my question. I say are not these gentlemen the most competent perhaps that can be found in England, so far as knowledge goes—the physiologists?—Yes.

13473. (*Mr. Ram.*) In regard to your own hospital do you use any antitoxins at all?—No.

13474. Any sera?—No.

13475. Have you any cases of diphtheria in your hospital?—I could not tell you about that. I have never had any cases.

13476. How long have you been connected with the hospital?—Four or five years.

13477. How long has the hospital gone on altogether?—I think I was appointed surgeon very soon after the hospital was opened.

13478. Four or five years ago?—Yes.

13479. Do you mean to say that you cannot say aye or no whether you have had any case of diphtheria or not?—No, because they would not come under my notice.

13480. What is the size of your staff?—Two surgeons, three physicians and a house surgeon.

13481. But holding the views that you do about the inutility of the diphtheritic antitoxin would it not be a matter of great interest to you to see a case treated?—With antitoxin?

13482. Either with or without?—No, I should not judge from one case for a moment.

13483. Could you find out for me whether there have been any cases of diphtheria, and what the rate of mortality has been in your hospital?—I think if they have been diagnosed at all they have not been taken in. We would not take them in if it could be helped.

13484. Why?—We have only one isolation ward. I do not think it is always furnished, and it is in a very inconvenient place.

13485. If the general view with regard to the diphtheritic antitoxin is correct it is lucky for the patients perhaps that you do not take them in?—Yes, if the view is correct that is so.

13486. With regard to your book, in this book which has been already referred to (Sir William Church has asked you about the preface) do you anywhere draw a distinction between the work done in England and the work done on the Continent?—I am afraid I could not answer that without reading the book again.

13487. Do you suggest that vivisection is carried on to-day in England just as it is carried on, or as it was carried on when you were in Strasburg?—No, because there is probably a difference in the nature of the experiments here and there to some extent. Strasburg is rather a special place for particular kinds of experiments.

13488. But you have been dealing in this book with all kinds of cruelties practised at Strasburg?—Not all kinds—some kinds.

13489. Some kinds, if you please, of cruelties practised on animals?—Yes.

13490. Were they operations of a cutting nature on animals not anæsthetised at all?—Yes.

13491. Do you suggest that that takes place in England to-day?—I do not suggest that all those experiments I mentioned there occur in England.

13492. Do you suggest that any of them do?—Yes, I suggest that it is probable, judging from human nature that the vivisector does not always take care to see that the animal is properly anæsthetised, believing as he does that the animal feels little pain.

13493. That is what you said just now. Have you any single fact upon which you can base that belief that you can bring before the Commission?—No, not a single fact.

13494. I want to read you one sentence from page 29 of your book: "My knowledge of what is done on the Continent, and my studies of the 'Journal of Physiology,' which details some of the experiments carried on in this country, prove to me absolutely that vivisection in Great Britain is not different in nature or scope to what it is on the Continent." Is that your opinion to-day?—Yes.

(*Chairman.*) Not if your last answer is correct, certainly.

(*Mr. Ram.*) I want to know whether the witness adheres to the previous or the last answer?

(*Witness.*) Would you mind reading it again?

13495. (*Mr. Ram.*) I was reading your words: "My knowledge of what is done on the Continent, and my studies of the 'Journal of Physiology,' which details some of the experiments carried on in this country, prove to me absolutely that vivisection in Great Britain is not different in nature or scope to what it is on the Continent"?—That refers largely to the nature of the experiments; that is what I had in my mind.

13496. I am asking you whether this represents your opinion of the facts to-day?—My opinion, and I am giving it simply as my opinion (I have already told you that I have no first-hand information to go upon), is that experiments of the present day do not differ from what they were at the time of the last Commission; that the present law has made no difference, and I do not believe that out of respect for the law, merely as the law and nothing more, experiments have altered.

13497. (*Chairman.*) Without having a single fact to go upon, speaking merely from what you call your knowledge of human nature, you mean to say that your opinion is that this Act has had no influence whatever upon the practice of vivisectors?—Yes.

13498. (*Mr. Ram.*) One or two questions about curare. You say, "An anæsthetised animal is immobilised by the anæsthetic. Curare in this case would be unnecessary. To speak of immobilising an anæsthetised animal would be equivalent to speaking of killing a dead animal." Is that your view now?—Yes.

13499. Have you seen the evidence given before this Commission of many cases in which, after the animal has been absolutely anæsthetised, curare has been given in order to prevent the possibility of any reflex action?—I think that the comment I put there is a fair one.

13500. You wish to stick to that, do you?—Yes.

13501. You say "it is clear that in curarised animals anæsthesia is a practical impossibility." Do you believe that?—Yes, certainly.

13502. You mean if the animal is given a lethal dose of anæsthesia?—No. If you say a lethal dose of anæsthesia, I do not say that.

13503. Then what do you mean by these words which I have just read to you "It is clear that in curarised animals anæsthesia is a practical impossibility." What does that mean?—That I am assuming, as in many of these cases, the experiment goes on for a certain length of time under chloroform and ether, and I am assuming that the experimenter does not wish the animal to die quickly, as it sometimes does when an overdose is given, and assuming that the experimenter will not give more anæsthetic than what he thinks is enough to keep the animal alive until the end of the experiment; and that inasmuch as under curare there are not the same guides which there are in giving anæsthetics on a human being, therefore it is highly probable that the animal will be conscious.

13504. Does that mean that anæsthesia is a practical impossibility in curarised animals?—Except by a lucky chance.

13505. Then you go on to speak of an animal which you saw operated on by Professors Starling and Bayliss in 1900 (I think this is an old friend), "When I first saw the animal (a small dog), he was motionless under curare." Then you go on to say what was done. Do you suggest that that dog had no anæsthetic given to it at all?—No, the account states that ether was given through a pump.

13506. Do you suggest that the dog was not anæsthetised?—I suggest that it is very highly improbable that the dog was properly anæsthetised.

13507. Then you go on to say, "This experiment is a fair example of what is going on daily throughout England"?—Yes.

13508. Do you believe that to-day?—Yes, certainly.

13509. Can you give me any fact on which you base that?—I have already shown you, I think, why it is so.

13510. I do not want deduction. I want you, if you can, to give me any case that has come to your knowledge on which you base that assertion?—I take it it is a question of statistics.

13511. Can you give me any case?—No, that is the only experiment I have seen on a living animal in England.

13512. Then why do you say it is a "fair example of what is going on daily throughout England," that being the only one that you have seen?—You have asked me the question and I think I have a right to answer it.

13513. I am trying to get you to answer it?—It is a fair example, because, firstly, I have seen, personally, cases in which curare is used, and, secondly, there are any amount of similar experiments to be found in the "Journal of Physiology."

13514. Is that the reason why you say that it is a fair example of what is going on daily?—Yes.

13515. You say on page 38, "Before the discovery of ether and chloroform, morphia was used in surgical operations. But it only diminished, it did not remove the torture." Do you still think that about morphia?—I have answered that question already.

13516. It is because you have answered it that I want to ask you a question upon it. You say now, I understand, that you have ascertained that there can be such a dose of morphia given as will produce complete anæsthesia?—Yes.

13517. Therefore you were mistaken in what you thought when you wrote this book?—I would rather say that the authorities upon which I made the statement were mistaken.

13518. At all events, you have now come to the superior knowledge that that is not correct?—Yes.

13519. Do you still believe that you are correct in saying, as you say here, that curare is used daily throughout England?—Yes.

13520. Do you believe that it is used daily throughout England on animals which are not absolutely anæsthetised?—Yes.

13521. Have you any facts on which you can base yourself for that?—Only you put in the word "daily" there.

13522. No, I have not; I am reading your words?—That is another statement, is it?

13523. Yes, it is another statement?—I have pointed out that the evidence of the "Journal of Physiology" is as to the proportion of physiological experiments in which curare is used, and I have already given you the reasons why, in my opinion, it is almost an impossibility, when you use curare, to have the animal properly anæsthetised.

13524. Do you know anybody who holds that opinion besides yourself?—I think all my colleagues do so.

13525. At the hospital?—Yes, and who belong to the Society.

13526. Eliminating those gentlemen, can you give me the name of any professional gentleman of eminence in England who holds that view?—I have not had an opportunity. My acquaintance is so limited that I could not give you any profitable information on that point.

13527. You give, in this pamphlet, certain statistics on page 67, called "Figures in favour of serum." You bring them down to 1900 only?—Yes.

13528. You have brought out another edition of this pamphlet, or brought it up to date, in which you deal with figures as late as 1904, in fact, later than that,

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because you quote the "Lancet" of 1905 on page 86?—Yes.

13529. Why was it that in those later editions, or in this bringing up to date, you did not bring these figures down to a later date than 1900?—Because I have not had the source to take them from.

13530. You have now got it before you in the evidence of Dr. Martin?—This pamphlet was printed before.

13531. But it was brought up to date later?—I have not said that I brought everything up to date. I said that some additions were made afterwards.

13532. I ask why you did not bring these figures up to date?—Because, firstly, I do not think it would have been of any interest whatever.

13533. Are you aware that if they had been brought up to date it would have shown a decrease in mortality as the use of serum was continued?—We will assume that it would have done so considerably; but I do not see the use of it.

13534. Will you answer my question. Are you aware that if you had brought your figures down from 1900 to 1905 (as you might have done), in this book, it would have shown a decreasing mortality since the use of serum became more common?—No, I am not aware of that fact.

13535. One question with regard to what you began your evidence to-day with. You said that animals probably are more sensitive to pain than other creatures because they have a keener sense of smell and hearing?—I did not say for that reason alone. I said for various reasons.

13536. You quoted the keen sense of smell?—I said that that was one part of the evidence.

13537. I see in your book that you quote an article about animals and their capacity for pain. You quote a case of a horse whose leg was broken, and you say the horse walked about on the broken bone and grazed and so forth, carrying the fractured limb dangling. You remember that case?—Yes, that is quoted.

13538. In your opinion does that show that the horse, at any rate, did not suffer very much?—I should want a good deal more evidence before believing that. It does not harmonise with all the evidence I have seen of pain in animals. Besides, even that is unauthentic.

THIRTY-FIRST DAY.

Wednesday, 30th October 1907.

PRESENT:

The Right Hon. the Viscount Selby (*Chairman*).

Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. J. Tomkinson, M.P.
Mr. G. Wilson, LL.D., M.D.
Captain C. Bigham, C.M.G., *Secretary*.

Mr. Francis Gotch, M.A., D.Sc., F.R.S., M.R.C.S., called in; and Examined.

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13539. (*Chairman.*) You are a Doctor of Science, a Fellow of the Royal Society, and a Member of the Royal College of Surgeons, I think?—Yes.

13540. You are Waynflete Professor of Physiology in the University of Oxford?—Yes.

13541. And you come here at the request of the University?—I come here at the request of the Faculties of Medicine and Science of the University of Oxford.

13542. You are a Fellow of Magdalen College?—Yes.

13543. You yourself hold a licence, I presume, under the Act?—Yes.

13544. Have you had large experience in experimenting yourself?—I have had experience in experimenting extending now over 20 years.

13545. In research of all kinds?—In research and such experiments as I considered necessary for teaching purposes.

13546. There was a correspondence which was shown to us relating to an alleged illtreatment of a dog. Of course, I shall not omit to ask you about it, but I would rather first take you through your *précis* before I come to the correspondence?—Very good.

13547. You desire, I believe, to speak in the first place of the importance of extending physiological knowledge?—Physiology and pathology are so intimately associated, that advance in the one leads to a corresponding advance in the other; hence the undue restriction of either branch retards the progress of the other one. Medical science is based upon the existing condition of knowledge in both these subjects, since it deals with tissue processes normal and abnormal, and these are the direct subject matter of physiology and pathology. The more extensive the knowledge of these processes becomes, the more helpful will these sciences be for the prevention and mitigation of suffering in regard to both human being and domestic animals. My object in bringing forward this particular point is to enforce the fact that physiology and pathology are practically two branches of the same science, and that you cannot interfere with the one without interfering with the progress of the other; that although there may be no obvious utilitarian results from some physiological work, you cannot judge physiology on that basis.

13548. You mean that it may throw light on questions of pathology?—It may throw light on questions of pathology. I contend that you can no more separate them, for instance, than you can separate the different parts of a tree, the fruit of which is the obviously useful thing, but the physiological stem cannot be unduly interfered with without doing away with the fruit which comes from pathology. These two sciences, physiology and pathology, are under an enormous debt to physics, chemistry, and biology; this debt they partially repay by the fact that physiological and pathological discoveries and methods have in their turn helped the advance of these other sciences. The discovery of electricity, and the formulation of the law of the conservation of energy are notable instances of this. Natural science as a whole is retarded in its progress if the free development of any one of her branches is unduly restricted.

13549. (*Mr. Ram.*) Do you quote the discovery of electricity as an assistance given by physiology and pathology to chemistry and biology?—I quote it as an instance. Certain phenomena observed in animals and animal tissues led to the discovery of electricity. I may perhaps refer also to the conservation of energy. It is a very remarkable fact that this law was practically discovered by Dr. Robert Mayer in consequence of his observations on the blood of patients in Java; from these he formulated the great law which has practically determined the advance of modern physics and chemistry.

13550. (*Sir William Collins.*) Had not Joule something to do with it?—Mayer was the first formulator. I can only refer to the well-known essay on this subject by Tyndall, in which he states the history of the events quite succinctly, and puts the whole credit to Mayer.

13551. My question was, had not Joule something to do with it?—Joule certainly elaborated the question in regard to heat and its relation to various other forms of force.

13552. I thought you said that Tyndall gave the whole credit to Mayer?—The whole credit of the first formulation of the law.

13553. (*Chairman.*) Was Joule an experimenter on animals?—No.

13554. And was Mayer?—Mayer was a medical man. I could name very many other points, but I do not know whether I should name them now. One of the peculiarities I should say of physiological science is the necessity for accurately measuring time since the events occur so quickly. The accurate measurement of

time started by physiologists has been employed by engineers and has been used for measuring the muzzle velocity of projectiles. The recording arrangements employed by physiologists, first brought forward by Helmholtz and Ludwig, have been employed by engineers for obtaining graphic records of the time and working of various parts of machines. This is only an illustration of the way in which one science helps another.

13555. You mean that physiologists find it a necessity to have some very accurate method of measurement?—Yes.

13556. Who discovered the methods of so doing?—They were chiefly invented by the physiologists Helmholtz and Ludwig. Besides those branches of knowledge generally designated under the term natural science, there are others which are of great importance for the social community. Such are the study of the effect of environment on men and animals, and the study of heredity; in addition there are such scientific branches as psychology and education, this last comprising the appropriate training of both mind and body. The extension of physiological knowledge is of the utmost importance for that of the above subjects. I do not think I need dwell on that.

13557. Not unless you think it necessary. Then your next point is the methods of extending physiological knowledge?—All methods resolve themselves into two—observations on uncontrolled phenomena and observations on controlled phenomena; the control is the determining factor in what is termed experiment. Owing to the extensive scope of physiology, which comprises chemical, physical, and anatomical points of view, many of the observations of the experimental type do not necessitate the investigation of the actual living processes, at any rate, in the first instance. Others are simplified by extensive introductory study upon the isolated tissues of technically dead animals, cold-blooded or warm-blooded.

13558. What do you mean by technically dead?—The animal as such is dead, but the muscular and peripheral tissues are not dead.

13559. (*Sir Mackenzie Chalmers.*) Is the conscious life dead? Yes, the conscious life is dead, the animal being killed, and the particular organ or tissue removed from the body. But in all these cases a point is reached sooner or later when it becomes necessary to study the changes in the whole animal—that is, in one still possessing its co-ordinating mechanism. I explain co-ordinating mechanism later. I do not think I will pause to explain it now.

13560. (*Chairman.*) As you please.—This necessity must obviously exist from the first as regards the study of the co-ordinating mechanisms themselves. The co-ordinating mechanisms bring about the further co-operation or the antagonistic restraint of the activity of any one set of organs through the presence of changes in another set. Such a co-ordinating mechanism is the transfer of chemical substances by means of the circulation, and the more subtle transfer effected by means of the nervous system. For their efficient study and continued investigation experiments upon higher living animals are necessary, since in these only do the particular co-ordinating mechanisms attain a sufficient degree of development. My object in bringing this forward is to show that the scope of physiology and experiments in physiology is very large, and does not necessarily involve any experiments on living animals until you reach a certain point; but that as regards two essential things—namely, the blood as a co-ordinating mechanism and the nervous system as a co-ordinating mechanism—the very fact of co-ordination, meaning the simultaneous value of all parts, does involve experiments on the whole animal.

13561. Then your next point is the co-ordinating mechanisms of the higher animals?—In regard to the first of those, our knowledge of the circulation of the blood, of the condition by which it is modified, and of the part which is played by the character of the conveyed blood, is all due to experiments made upon living animals, and in particular upon mammals. This knowledge is still in many respects incomplete, but has been greatly advanced during the last half-century, the advance being initiated by the introduction by Ludwig of accurate recording methods. As regards the character of the conveyed blood as a determining factor in this co-ordination, there has been during the last 20 years a remarkable development through the discovery of a number of internal secretions. This discovery is entirely due to experiments on higher mammals and by demonstrating an extensive co-ordinating mechanism, which was previously unsuspected, forms a new starting-point, whose value is of the same order as that of the earlier discovery of the reflex activities of the nervous system.

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13562. When you speak of the introduction of accurate recording methods, do you mean those methods that we have heard described in the evidence—I am afraid I am not competent to describe them accurately, but when you are testing the pressure on the heart, and so forth?—Yes, the methods of recording the blood pressure, and recording the effects upon the blood pressure of various circumstances. The co-ordinating mechanism, which is termed the central nervous system, is so elaborate in higher animals that its investigation has to be conducted along many different lines. The extension of our knowledge as to the structure and processes which constitute the two main aspects of neurology has a very special importance, inasmuch as psychology, and with it scientific education (I mean an education based upon scientific principles), must be more and more based upon neurology. Much has been learnt by dissection of dead animals and by microscopic improved technique, but such fundamental principles as the distribution of functions within the different parts of the whole nervous mass, the paths by which these parts intercommunicate, and those by which they are connected with other non-nervous body structures, are all the fruit of experiments carried out on higher mammals during the last quarter of a century.

13563. I have not been asking you questions myself, because there are gentlemen here more competent than I am to ask questions on many of these points, and if they desire to do so they will do so afterwards. You now desire to speak of the necessity of a free choice of mammals for physiological research?—This choice is necessary even for the purpose of acquiring a knowledge of the gross and minute structure of tissues and organs, for which animals are painlessly killed and the dead animal then utilised. The frog and rabbit are only suitable for more general tissues, muscle, nerve, and such connective tissues as bone, cartilage, etc.; the tissues of carnivorous animals (dog, cat) must supplement the above, particularly in the case of the digestive and excretory organs; finally, the monkey's tissues must supplement the foregoing, especially in connection with the nervous system. Similar freedom of choice is demanded for the purpose of acquiring a knowledge of the process which, during life, are displayed by these structures, but in this case the study may be carried on during the stage of complete anæsthesia, thus absolutely eliminating all pain. It is, however, occasionally necessary to keep the animal alive after some particular experimental procedure has been terminated, and this is especially the case in connection with the extension of our knowledge as to the working of the co-ordinating mechanisms previously referred to. A most striking example of the great increase in knowledge brought about by experiments of this kind is that of the structure and functions of the central nervous system, as displayed by the degenerative changes and functional losses following definite nervous ablations. Here the introduction of aseptic conditions eliminates subsequent pain.

13564. With regard to the necessity of a free choice of animals, have you experimented on all these animals—the frog, the rabbit, the monkey, the dog, and the cat?—Yes, some time or other.

13565. Can you give us an instance of what you speak of as the necessity of making some experiments upon the monkey, as opposed to the dog or the cat, for example?—The question of the localisation of different functions in the cerebral hemispheres is carried out in the monkey with far greater detail than in any such animals as the dog and rabbit.

13566. If you want to discover what particular part of the brain governs the action of a particular limb or muscle or nerve?—Yes, I should certainly not accept as convincing evidence for a higher mammal any experiment except one which was on a higher mammal—that is to say, the monkey, from that point of view.

13567. Would an experiment on a dog suffice for it?—No, because the distribution of these functions in a dog is different from that in a monkey, inasmuch as the cerebral hemisphere of the dog is framed on a

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different plan. It is framed on what is termed the longitudinal plan of fissures. The transverse plan of fissures, which allows for a large nervous mass without undue protrusion forwards, is the highest type; it begins in the monkey, and goes on, of course, to man.

13568. (*Sir Mackenzie Chalmers.*) Is there an ascending scale among monkeys?—Yes.

13569. (*Chairman.*) Are experiments upon the brains of dogs or cats ever useful?—Certainly. They give useful information from the point of view of determining the action of the higher cerebral centres upon what may be termed the lower centres. The lower centres in the dog and cat, I should say, have far more effective value than in the monkey and man, for the very reason that the cerebral hemispheres themselves have not replaced them to the same extent since they are less developed.

13570. By the lower centres you mean the nerve centres?—I mean the nerve centre, including those which are at the base of the brain in what are known as the basal ganglia, the corpora striata, and the optic thalami.

13571. Then your next subject is the value of physiological experiments for physiological teaching?—Teachers of physiology hold positions of almost exceptional responsibility. In all cases they deal with a subject which forms an essential part of every medical man's intellectual equipment. In many cases, as in universities, the subject has to be also treated as a branch of natural science, and, besides medical students, there are others who approach it chiefly from this point of view. Some of these scientific students enter the teaching profession, and may have, even at present, to conduct elementary courses of physiological instruction based upon sound principles, whilst it is probable that in the future the value of enforcing the physiological principles underlying national hygiene and the physiological improvement of the young will be more and more recognised by all branches of the social community. The demand for elementary physiological knowledge will thus increase. I should like to point out that I myself have always had some students of this type, and that in some local universities these students form a considerable part. They are students who are taking up physiology with the object later on of becoming teachers.

13572. Teachers of physiology, do you mean?—Not necessarily of physiology.

13573. Some of them are teachers of science?—Teachers of science.

13574. Treating physiology as one branch of it?—Yes.

13575. But desiring to learn all?—Yes. I do not know whether the Commission is aware of the very extensive work that is done in the North in this way. Professor Sherrington has a large number of students of this type, for instance.

13576. I do not think we have had any evidence upon that point. I think we have some gentlemen coming from the northern universities. Are you referring to Manchester and Liverpool?—Yes.

13577. I think we shall have some gentlemen from there?—In all scientific teaching it is now recognised that the great desideratum is to help the student in drawing his own conclusions; this is the only real virtue of personal teaching, apart from mere book-work. This help can be afforded by personal intercourse, by lectures, and finally by enabling students to observe for themselves certain phenomena, instructing them how to control these phenomena and ascertaining that they do draw from these the legitimate conclusions. In order to accentuate this last and most convincing (practical) educational method, it is essential that the teacher should himself go through such practical work with or before his students. It is for this reason that every lecturer thus feels the necessity of illustrating his lectures by experiment; he does this not merely to display a phenomenon but to show the conditions of its control, to draw the logical conclusions himself, and make the student realise the intellectual steps by which knowledge of scientific principles has been attained. There may, however, be phenomena which, though of fundamental importance from this point of view, are difficult to control and observe; and others which for other reasons it is undesirable, even impossible, for students themselves to control and observe. The teacher has then thrust upon him an additional responsibility, since he has to determine how far he should display these phenomena, not merely for drawing conclusions, but for giving students a first-hand knowledge of their occurrence. In physiology, as in all science, teaching methods have during the last thirty years undergone a very notable change in this direction. Practical work by students in microscopic study, in physiological chemistry, and in ascertaining such features of the muscular and nervous processes as can be readily observed on the tissues of the pithed and of the dead, decerebrated frog, now forms a large part of the student's work. The extent of this work varies in different teaching institutions, but the general character is everywhere the same. There are, however, certain fundamental aspects of physiology which these practical courses, however extensive, do not display, and, as already indicated, among these aspects are those connected with the co-ordinating mechanisms. The preservation of the circulation and of the chief co-ordinated reflex activities of the animal necessitates the animal being more or less intact. But the seat of consciousness may be absolutely paralysed by anæsthetics, or destroyed by decerebration without invalidating this preservation. On fully anæsthetised animals phenomena of this character can be displayed, and the questions for the physiological teacher to settle appear to me to be four.

13578. I gather from what you said that you attach great importance to demonstration experiments on living animals accompanying lectures?—Yes.

13579. And you have fully explained your reasons for that here?—Yes.

13580. Then you come now to matters of more detail. I see you tabulate four questions that you think are involved in this question of the value of demonstrations?—Yes. I would rather put this as my personal view. I do not know that others would agree to this.

13581. We understand that this is your evidence and your opinion?—The questions for the physiological teacher to settle appear to me to be the following: (1) Is it necessary for efficient teaching of the subject to display some of these phenomena, on mammals so fully anæsthetised that no pain is possible? (2) Is it certain that such an animal can be effectually and completely anæsthetised? (3) What particular groups of phenomena should be chosen out of the whole extensive series as the most essential for students to observe? (4) What circumstances should determine the choice of animal and of anæsthetic in such demonstrations?

13582. Then you proceed to give the answers?—In answer to the first question I, personally, have no doubt of this necessity; whilst as regards the second I am convinced by practical experience that complete insensibility can be easily produced in any mammal.

13583. You have, of course, told us that you have had an experience of twenty years. You have had a very large experience I suppose of anæsthetising?—I have anæsthetised mammals of different sorts in large numbers, though not in such a large number of individual cases as many of my colleagues have.

13584. And you are very familiar with all the different anæsthetics or mixtures of anæsthetics that are used?—Yes.

13585. And you have formed your own conclusions by experience as to which are the best for particular operations?—Yes.

13586. I ask you this because we have had some gentlemen before us who have doubted whether complete anæsthesia can in some cases be obtained, and have very much doubted whether it is obtained in most cases, or in many cases, so I should like to ask you particularly upon that, whether your experience leads you to speak confidently on this question as to whether complete anæsthesia can always be obtained in experiments?—I should say, so far as my belief goes, that it is a certainty that it can be obtained.

13587. (*Mr. Tomkinson.*) For an unlimited time almost, at least for a great length of time?—For a very great length of time.

13588. What do you mean by a very great length of time?—I have been under a severe operation myself which lasted two and a half hours. I mean it is a question of hours.

13589. (*Chairman.*) And in your case, of course, it was part of the surgeon's duty to see that you did not get a lethal dose?—Yes.

13590. Which is not a thing it is necessary to avoid

(in fact, it is almost desirable to produce it) in the end in many operations upon animals?—Yes.

13591. I suppose the necessity of avoiding a lethal dose increases the difficulty?—I should think it does. I should think the most difficult animal to maintain in a prolonged condition of anæsthesia would be man.

13592. For that reason?—Yes.

13593. We have had a good deal of evidence and opinion as to whether you can be sure that you have produced complete anæsthesia in cases where curare is given—curare not given as an anæsthetic but for the purpose of keeping the animal still. There have been some strong opinions expressed before us that there are no means by which you can be sure that you have produced anæsthesia. Have you met with that difficulty?—I have had very little experience of curare.

13594. Have you used it?—I have assisted in its being used, but I have never used it myself on mammals. I should, however, have thought that it was easy to see whether there were any of those violent and inco-ordinated reflexes which we take to be the ordinary indications of pain, even under curare, because ordinary voluntary muscles whilst with the dose of curare used are put out of court, the involuntary muscles remain. And with pain you get inco-ordinated activities of these involuntary muscles, which very readily displays itself—which displays itself for instance in extreme alterations of blood pressure in consequence of the varying contraction of the muscles and the arterials.

13595. What is the object of using curare if you cannot prevent involuntary movement?—The object is to prevent the gross movements of the voluntary muscles and to leave the involuntary muscles free to be affected by the central nervous system. That is the object of using curare.

13596. You mean that the object is not to produce perfect stillness?—Only to produce perfect stillness of such muscles as move the limbs voluntarily, but not of the involuntary muscles which move the intestines, for instance, and other things.

13597. It would prevent the animal from blinking its eyes, I suppose?—Yes, that would be a voluntary muscular movement. But you would still have the indication of uncontrollable and inco-ordinated reflexes through the altered activity of the involuntary muscles which are unaffected by the dose used.

13598. Do you consider, then, that there would be something visible to the eye of the operator?—Yes.

13599. Something to tell him whether or not the animal was feeling pain?—I should say so in the alterations of blood pressure.

13600. You say that you have never yourself operated under such circumstances?—In earlier days I have assisted at operations under those circumstances, as a demonstrator I mean.

13601. What you are saying is rather a matter, as I understand, of what you think is the scientific probability than of your own personal experience and observation?—Yes.

13602. (*Mr. Ram.*) Does curare prevent the utterance of cries?—Yes, those being voluntary movements.

13603. (*Dr. Gaskell.*) It is a rise of the blood pressure is it not?—Yes.

13604. Owing to the contraction of the involuntary muscles and the blood vessels which would not be abolished by curare?—Yes.

13605. But it would be diminished if the animal was absolutely anæsthetised?—Yes.

13606. (*Chairman.*) And supposing you had taken the pains which, of course, ought to be taken in these cases—that is to say, extreme pains—to ascertain that you got the proper anæsthetising dose administered to the animal, and you took note of the time at which it was administered, would that convey any certainty to you?—I should say that it would. But you have, I should have thought, the additional certainty that in all experiments of this type, the experiment would be vitiated if there were these sudden and uncontrollable rises of blood pressure which would be produced under curare if the animal had a consciousness of pain. In experiments of this nature they are not produced.

13607. (*Sir Mackenzie Chalmers.*) For what class of experiments, then, is curare used?—Blood pressure—for certain blood-pressure experiments.

13608. Outside blood pressure it is unnecessary to use curare?—I think so. I do not know of any other.

13609. (*Chairman.*) Supposing you were experimenting on a limb which would very likely move in case of pain, like a leg or an arm, you would take direct physical means to keep it still you mean, or would you trust to the anæsthetic alone?—You cannot trust to the anæsthetic alone because there are reflexes from the spinal cord. Moreover, it is necessary to fix any animal, whether alive or dead, in order to make a proper dissection.

13610. I mean that when you do not give curare to animals to prevent these involuntary movements you rely on physical means taken to fix their attitude?—Yes.

13611. Then what is your answer to your third question?—The answer to the third question requires more detailed treatment. In making his selection the teacher must, I think, be influenced by a number of considerations. First, the character of the physiological principles which underlie the displayed phenomena. They should be fundamental, and such as cannot be convincingly set forth by experiments on the pithed frog or on any isolated organ of the dead animal. Such I consider to be the character of such demonstrations as the following:—The blood pressure, the working of the heart, the intrathoracic pressure and its changes during respiration, the respiratory movements, the action of such substances as raise and lower the blood pressure, the reflex control exercised by the nervous system over the circulation and the respiration; the nervous control of secretion and the digestive organs, and, finally, the localisation of reflex motor centres in special parts of the central nervous system. A further consideration is whether all these can be so displayed that a considerable group of students can observe and appreciate the significance of the phenomena. This depends very largely upon the possession of appropriate apparatus and of skill in its use. Associated with this is the circumstance that physiology offers extensive fields for work, so that one teacher is better acquainted with one mode of physiological technique than with another; thus he naturally selects those phenomena whose display involves the technique in which he is most skilled.

13612. I think at this point I ought to ask you a question about the conduct of students at lectures where there are demonstrations. Some witnesses have spoken of students habitually and frequently making a jest of the operations on the animals, and have accused them of demonstrations of hard-heartedness or cruelty as if they treated the whole matter as a jest. Have you seen anything of that sort at your lectures?—I cannot say that I have ever seen it. On the contrary, I am always impressed by the circumstance that this has been the most serious part, and is regarded as the most serious part, of practical physiological teaching.

13613. How many students would pass through your hands in the course of a year now at Oxford?—I have this year altogether about 70 working in my laboratory. I have not demonstrated to more than 15 or 20.

13614. It is not desirable to have a greater number at a demonstration?—No.

13615. They cannot follow it?—No, I think not. But teachers differ about that.

13616. (*Sir Mackenzie Chalmers.*) Are they all advanced students?—No, of two years—about 35 of each year.

13617. Do you demonstrate before all students, or only before advanced students?—I demonstrate some of these things before the first-year students and others before the second-year students.

13618. All students at Oxford now see actual demonstrations?—They all can see demonstrations.

13619. On mammals?—On mammals.

13620. On anæsthetised mammals?—Yes; the choice of the animal I have not yet referred to.

13621. (*Chairman.*) When you say that there are something like 15 students at a demonstration, there would be more at a lecture?—Yes, the lectures are attended by a variable group of between 30 and 50 first and second year students.

13622. Do you describe beforehand what the demonstration is to be?—Yes.

13623. At the lecture?—At an informal lecture or at the actual lecture

13624. Do only those who are making a special study

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of physiology for medical purposes go?—No, I make no distinction.

13625. (*Mr. Tomkinson.*) Would that apply to any undergraduate who was reading for honours or for a degree in natural science?—Only those reading for a degree in physiology. At Oxford the schools are separate.

13626. I thought that you said in reply to the last question that they all went without reference to a particular line?—But all those who come to my laboratory are taking this one subject of physiology.

13627. (*Sir Mackenzie Chalmers.*) You have 70 physiological students?—Yes. Natural science is a very large subject, with about 10 different ways of taking honours in it. Only the physiological students come to me, and most of them are medical students. I do not think out of my number there are more than four or five who are not.

13628. (*Sir William Collins.*) Would all the students attending the physiological course within the two years of their course see all the experiments that you demonstrate, whether they are medical students or others?—They can see them, they all can. They have the choice of seeing them; I do not say that they do.

13629. (*Chairman.*) But in groups of 15, I understand?—Yes.

13630. (*Sir Mackenzie Chalmers.*) That is as many as can be comfortably accommodated?—Some experiments I show to a larger group in the lecture room, say 30, where there is no doubt that the essential thing can be seen without any difficulty.

13631. (*Sir William Church.*) But your course in physiology and your lectures on physiology are open to any student in the University, whether he intends to take a degree in arts or intends to take a degree in natural science, are they not?—I do not think that is the case. The laboratory is by special rule placed under my supervision.

13632. You can exclude?—I can if I wish.

13633. Do you exclude a man from your courses of lectures who does not pledge himself to go in for science?—If an Oxford student came to me and said he wished to attend either lectures or courses of laboratory instruction, I should inquire what his reason was. Nor do I allow anybody to attend either lectures or courses of practical instruction unless he has seen me and given his reason for doing so.

13634. Then would you exclude a man who wished to become acquainted with physiology, but yet who probably wished to take his degree in arts rather than in science?—No, I should not do so provided he satisfied me that he had a serious intention.

13635. (*Sir Mackenzie Chalmers.*) A serious intention of studying your subject?—Yes.

13636. Not coming merely for curiosity?—Yes.

13637. When he attends your lectures has he not to enter his name and pay a fee?—There is no fee for attending professional lectures in the University.*

13638. (*Chairman.*) I presume as a matter of course that you would have the right, and would probably exercise it, of excluding any student in the lecture room who did not behave himself properly?—Yes, I have that right. I may add that I do get students of a different type altogether—gentlemen who ask to come specially to the last course of lectures that I give on the central nervous system. These are students of philosophy and psychology.

13639. (*Sir William Collins.*) I was going to ask you this question: you have suggested that all education should be based on neurology, have you not?—I have. I should like to add that in a special annex to my department which is now being built, special rooms are reserved for this particular subject, psychophysics, which is taken up, not only by philosophers, but by those who are qualifying for the education scheme of the University.

13640. (*Chairman.*) Are you now prepared to give the answer to your fourth question. What circumstances should determine the choice of animals and anæsthetic in demonstration?—The answer to the fourth question depends very largely on the selection made—that is, what experiments you select out of that group; but may be influenced by other more special considerations. The size of the animal is an important element in some, such as blood pressure, nervous control of secretion, etc.; the character of the animal in others, such as the localisation of cerebral motor centres. A considerable number can be shown upon large rabbits, and whenever this is possible it is, in my opinion, to be preferred. As regards the choice of an anæsthetic, I hold that the demonstration of the anæsthetising influence of chloroform and ether, singly or mixed, itself presents phenomena of the greatest practical importance to medical students. Apart from this, I am inclined to use, in my own demonstrations, which are made upon rabbits, chloral-hydrate, or urethane, which introduced some time previously in the alimentary canal in sufficient quantity causes a profound and increasingly developed anæsthesia; the animal's condition gradually passes beyond anæsthesia into painless death.

13641. I should like to ask you this question: we have had some evidence given which rather suggested that the interval between giving a lethal dose and painless death was too short for any use in operation. How is that?—That is not my experience in chloral-hydrate.

13642. How long will the animal live supposing you wish it to live under anæsthetics?—An hour and a half or two hours.

13643. Without giving it a fresh dose?—Without giving it a fresh dose.

13644. With a dose that is lethal when given and secures complete anæsthesia the animal may live an hour and a half?—Yes, or two hours.

13645. What animal are you speaking of?—A rabbit.

13646. Would a dog or cat live as long?—I have not had much experience in chloral-hydrate and in urethane. I may say I have had no experience upon dogs at all. I have had rather limited experience upon the cat.

13647. I wanted to see whether it was the case or not that a lethal dose operated almost immediately?—No, it is a prolonged stage.

13648. (*Mr. Tomkinson.*) Under those two substances?—Yes, being absorbed slowly from the alimentary canal.

13649. (*Mr. Ram.*) In your last answer you said you had had little or no experience in dogs, and on cats it is limited as to the use of urethane?—Yes.

13650. You have had a large experience of other anæsthetics?—I have had some experience, not so large as those of some of my colleagues.

13651. You have had some?—Yes.

13652. (*Sir Mackenzie Chalmers.*) Is this hour and a half with or without artificial respiration?—Without artificial respiration.

13653. It is the mere action of the poison that takes an hour and a half?—It depends upon the amount of dose, but you can give it so that it would take, I should say, two hours.

13654. (*Mr. Ram.*) And yet with perfect anæsthesia?—Yes.

13655. (*Sir Mackenzie Chalmers.*) Perfect anæsthesia throughout?—Yes, perfect anæsthesia throughout, and gradually deepening.

13656. Gradually deepening into death?—Gradually deepening into death.

13657. (*Chairman.*) There is no doubt of the perfect anæsthesia?—I have no doubt; there are no evidences of these irregular activities.

13658. (*Mr. Tomkinson.*) Would a rabbit be fastened down as rule?—When it is in this stage finally, do you mean?

13659. Yes?—You can fasten it or not, as you like. It lies perfectly flaccid. If I produced a rabbit here under chloral hydrate it would lie so.

13660. It would not kick or struggle?—No.

13661. Therefore there is no necessity to strap it down?—There is no necessity to strap it down, except for the purpose of making the necessary dissection.

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* *The Professor of Physiology is authorised by a special statute to charge statutary fees on a given scale for Laboratory and is further authorised to charge a small University for students who commence attendance on lectures. This fee is to be applied towards meeting the expenses of the Department and do not go to the Professor; the charge is made at the beginning of the academic year and gives the Professor additional control over the admittance of students.—F. G.*

The necessity would practically be the same as in a dead animal.

13662. (*Sir Mackenzie Chalmers.*) To keep it in position, and to prevent your moving it with your own hand in the experiment?—Yes.

13663. (*Chairman.*) To keep the rabbit, for example, on its back?—Yes; it will not lie on this condition of absolute profound anæsthesia its back unless it is supported. The fact that in consequence of an ultimately lethal dose can be produced with such certainty in the rabbit, opens a further question. The demonstration method is not an ideal one as regards the students' acquisition of physiological principles. Viewed from this last standpoint, such acquisition would be greatly aided if it were possible for senior students under skilled and authorised supervision, and guarded by careful regulations, to themselves carry out on such profoundly and lethally anæsthetised animals some of the experimental procedures previously alluded to. The responsible character of medical, and particularly of surgical practice, renders it eminently desirable that this extension of existing possibilities should at least be seriously considered.

13664. Supposing you were consulted on the subject, what advice would you give as to the regulations that should limit the power of senior students to operate? Would you confine it to those who had taken a degree, and not begun to practise, or to those in the last year? —I should confine it to those who were in their last year in the university.

13665. But would you exercise any control, by examination or otherwise, to see whether they were qualified persons?—The control that I should exercise would be the control of a responsible person present.

13666. But in recommending them or in permitting them, if you had the power of permission, would you do it on the personal recommendation of the teacher? —I think so. I do not understand how one can make any other adequate selection. It is putting an extra responsibility, I confess, upon the teacher, but his burden already is a very heavy one, if he is a physiological teacher, and I do not think it is much extra burden.

13667. You would rely upon his observation and knowledge of the particular student?—Yes.

13668. I want now to ask you about this correspondence which took place. You are aware that Mr. Coleridge, in his evidence, called attention to a correspondence which he had with some undergraduate of Keble College; it is at Question 11455, and I will not read the letter, because you know it, and the Commission will remember it, but it was practically stating that that undergraduate had heard the yells of a dog at the University Museum, that he had satisfied himself that they proceeded from a dog under actual operation, and he spoke in strong language about the cruelty and the torture, "devilish practices," and so forth. That was signed by several other undergraduates, and then there was read a letter, I think, from yourself, denying that such a thing had taken place. Then Mr. Coleridge read a letter from Mr. Sharpe, the undergraduate in question, in which he in very strong language denies the truth of what you state, language probably which he did not, when he wrote it, intend to be read out to the Commission or anybody else, but thought he was writing to Mr. Coleridge only. However, it was read, and there the matter was left. Some questions were asked as to what Mr. Coleridge knew, and of course he said that he knew nothing about it personally, but that these gentlemen had given him this information, and he had no doubt that they were speaking the truth. I understand that since then this gentleman, Mr. Sharpe, has stated that he has inquired into the question, and has withdrawn the charge, and apologised for having made it?—Yes; and I should like in that connection to read two letters which I have been asked to read, in order that they may appear in the minutes. The first letter is a reprint of a published letter which appeared in the "Oxford Times" and the "Oxford Chronicle" of October 19th, 1907, from Dr. T. H. Warren, President of Magdalen College and Vice-Chancellor of the University of Oxford:—"Vivisection at the Oxford Museum. To the editor of the 'Oxford Times.'—Sir,—In your last issue you published an article with the above heading, taken from the 'Morning Leader,' which consisted largely of certain extracts taken from the evidence given by the Hon. Mr. Stephen Coleridge, before the Royal Commission on Vivisection. Mr. Coleridge quoted letters received by him from certain undergraduate members of Keble College, stating that it was within their knowledge that vivisection was being practised upon dogs in the Museum, and, further, that Dr. Francis Gotch, Professor of Physiology, when he stated that no vivisection on dogs had been carried on in the laboratory during the present year was not speaking the truth. I think it is just, alike to Professor Gotch and to the undergraduate members of Keble College who wrote or supported the letters, to say that I have had placed in my hands a complete apology by those undergraduates to Professor Gotch, a recognition by them of the correctness of Professor Gotch's statement, an explicit and unqualified withdrawal of the letters and statements sent and made by them to Mr. Coleridge, and an expression of their willingness that the fact of their apology and withdrawal should be published. I should be obliged, therefore, if you would now give the same publicity to the apology and withdrawal that you gave to the original statement.—I am, yours faithfully, T. Herbert Warren, Vice-Chancellor." The second letter is from the Rev. Walter Lock, D.D., Warden of Keble College, October 18th.—"Dear Professor Gotch,—I am very glad to find that you are satisfied with the apology which Mr. Sharpe and his friends have made to you, and with the action that the Vice-Chancellor is taking to make the apology public. And now that the matter is at an end, I should like to express my regret, and that of my colleagues, that you and the Museum authorities should have had this trouble, and that the relations between our college and the Museum, which have always been close and friendly, should have had even a momentary clouding. Mr. Sharpe and his friends are all excellent well-meaning fellows, who were actuated by a humane feeling, and honestly believed that the evidence which they were giving was true. But none of them is a science student or acquainted with the details of laboratory work, and I am afraid that they were not competent to estimate the real value of their evidence, the worst portion of which was based on a complete misunderstanding. They made some attempt to make inquiries, but the attempt was obviously inadequate, and I think that Mr. Coleridge treated them a little badly in accepting and making public the second letter without further investigations on his own part. As soon as further inquiries were made, and explanations given to them, they at once saw how their mistakes had arisen, and how impossible it was to substantiate their charges, and they have wholly withdrawn them. May I also thank you for the way in which your own patience and forbearance have contributed to reduce this matter to its true proportions, and to lead a friendly settlement of it. Pray make any use of this letter that you may think fit.—I am, yours sincerely, W. Lock."

13669. Have you Mr. Sharpe's letters that are referred to there?—They are in the hands of the Vice-Chancellor. I would rather not produce even a copy of them.

13670. I was going to say that it is entirely in your hands whether you wish them read or whether you wish simply to say that you are satisfied with his apology?—I am satisfied, but I should like to inform the Commission as to what the complete misunderstanding was.

13671. (*Mr. Tomkinson.*) Was one of those letters just read from Mr. Sharpe himself?—No.

13672. (*Chairman.*) Will you say how it arose?—The misunderstanding was one which may seem incredible, yet at the same time I know it does happen. These gentlemen imagined that dissection and vivisection were the same thing, and the evidence of the gentleman who was in for anatomy resolves itself into their having overheard a conversation between two students describing the work that they were doing for an examination in zoology involving the dissection of the rabbit; these students were not in my laboratory.

13673. (*Sir Mackenzie Chalmers.*) A dead rabbit?—Yes. That is their misunderstanding. The second point is that they heard dogs bark, and jumped to the conclusion, therefore, that it had some relation to the dogs. The third point is that they made inquiries of what they term an official. They made these inquiries by going in one Sunday morning, when they

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saw a man going round with the keys of the museum, and they asked him whether animals were cut up in that place, to which he said, "Yes." Those are the three facts upon which the whole statement is based.

13674. (*Chairman.*) Is your laboratory in the museum?—My laboratory is at the back of the museum.

13675. And there is a dissecting room also, is there, in the museum for purposes of zoology?—Yes, at the back of the museum, for human anatomy, and also a zoological laboratory where animals are dissected.

13676. (*Sir Mackenzie Chalmers.*) Dead animals?—Yes.

13677. (*Chairman.*) When Mr. Sharpe wrote his letter, of course he was not writing about the dissection of a dead animal; he was writing about what he believed to be the vivisection of a dog?—Yes.

13678. Can you say (I only ask it because Mr. Tomkinson appears to have some further questions about it), was there any dog vivisected at that time?—No, there was no dog vivisected.

13679. (*Mr. Tomkinson.*) There is one question I want to ask you. It seems rather strange that these young men, all of them, say that they are perfectly acquainted with dogs, and that it was ridiculous to say that they could have mistaken the case of a dog in agony for the mere yapping of puppies. Can you explain it to us? Was there any dog persistently howling there day after day?—The puppies made a great noise. I am bound to say that there was no concealment in the matter at all. These puppies played in the yard, and the students themselves going through the museum used to play with them, and certainly they made a noise, a very considerable noise.

13680. (*Sir Mackenzie Chalmers.*) Whose puppies were they?—They were mine.

13681. (*Mr. Tomkinson.*) They were not there for physiological purposes—for experiments?—Not for experiments; but I will explain in a moment what was done with them. The puppies themselves were in the yard, which lies 160 yards from Keble College, with two buildings in between. But the yard opens on the parks, through which anybody may walk. There is no concealment of the animals in the physiological department; and any of these gentlemen could have walked through a public way into this yard, where the puppies were barking, and have seen them. But they did not.

13682. (*Chairman.*) I did not ask you any of these questions that you are being asked now, because I understood that this gentleman had stated that the whole thing was absolutely unfounded?—He has.

13683. And apologised?—Exactly; that is the state of affairs.

13684. You thought that was enough?—Yes.

13685. (*Sir William Collins.*) I think you were going to add what the puppies were there for?—Yes, I thought the Commission would like to know, and I am glad to add it, because I wish to introduce the subject later of the supply of animals. The puppies were kept in order that some of them might be killed in the lethal chamber later and the tissues used for the histological class of the present term. They were so killed. They were not all wanted, and two of them were given away, at the request of two of the students who used to play with them.

13686. (*Mr. Ram.*) With regard to the apology, did it contain a complete withdrawal of the assertions they made?—Yes.

13687. You observe, with regard to what you have just told us as to their mistaking dissection for vivisection, that in one of their letters they say: "A man in Keble, who is in for anatomy, told Mr. Heald (one of those who signed my paper) that they did cut up rabbits, and that lately they had cut up a small dog, and this had been yelling for a week." You observe that apparently the information which they received, and upon which they based their assertion, was not merely that a dog was cut up, but that the dog cut up, whether alive or dead, had been yelling for a week. Was any of that withdrawn?—The whole thing was withdrawn. I can only say that it does not say whether the dog was yelling before or after. The puppies yapped for a great deal more than a week.

(*Chairman.*) If there is any doubt about the fulness of the apology, I think the apology had better be read. I think it would be better, because here there are several suggestions that the charge might not have been withdrawn. Nothing is clearer than what the opinion of the Vice-Chancellor and the Warden is.

(*Mr. Tomkinson.*) I have a letter direct from Mr. Sharpe, which I have authority to read. I wrote a fortnight ago to ask him whether he had anything further to say. He wrote to me on the 20th, and I again wrote to him, asking him whether he was prepared to have his letter read to the Commission; and his last letter says: "By all means read my letter at the Commission. I quite realise the publicity it will receive, and I am perfectly prepared to take the consequences. I have heard indirectly from Professor Gotch, through the Warden, and it is his intention to make my full apology as public as possible. I do not in the least mind that, as, through false information, I made unjust charges against him with reference to the points I mentioned to you. I have therefore no wish to make a simpler withdrawal." I asked him whether he would like this letter to be read, or to make a simpler or more formal withdrawal. Now he authorises me to read this: "Dear Sir,—In reply to your letter of the 19th inst., I am sorry to say I have no more evidence to give. I have found that on closer investigation the facts which were told me as true—viz., that a dog and rabbits had been cut up, and had been yelling—were entirely false. I felt therefore compelled to withdraw these statements, and to apologise to Professor Gotch for my language concerning him when I heard he denied this. I offered to see Professor Gotch when I knew this, but he refused to see me. I am sorry he did so, because I think he might have given me further proofs if I was wrong in *all* my charges. I cannot possibly withdraw my statements in which I said I frequently heard the cries of dogs or animals of some kind in pain, nor that when I asked an official if vivisection was carried on he replied in the affirmative. I did not ask him, it is true, whether it was done with or without anæsthetics. But at any rate I have not yet had a sufficient explanation of the cries I heard, although, as a gentleman, I am willing to believe Professor Gotch's statement that 'no vivisection experiments have been carried on in the past year.' I shall be very glad to answer any more questions you might like to put to me, and might I ask if I may have a copy of the report of the last sitting of the Royal Commission."

(*Sir William Collins.*) Could we not have put in the actual letter sent to the Vice-Chancellor by these students, as that has been read?

13688. (*Chairman to the Witness.*) I do not know whether, after that letter has been read, you think it desirable to read their letter?—I will read the letters if they are desired; at the same time, they are in the hands of the Vice-Chancellor.

13688a. You have a copy of them, have you not?—I have. The first letter was from Mr. Geoffrey Heald. Mr. Geoffrey Heald is the person quoted by Mr. Sharpe. You will see that he is quoted at the bottom of the second letter.

13689. (*Sir Mackenzie Chalmers.*) Is he a student of anatomy?—No, none of them were students of anatomy or science students at all. Most of them, the only ones I know of, are theological students. "Dear Sir,—I have just seen a statement from the book relating to evidence given before the Royal Commission, and I find therein a statement made as follows: 'Dr. Gotch is a liar.' I entirely disassociate myself from such a discourteous remark, and hereby beg to assure you that I never lent nor gave authority for my name to be joined to those who have made such an imputation. I did hear from a student in the museum that a dog had been vivisected, and I concluded that that was the noise that I heard. But since you have informed me to the contrary, then I accept your statement that no vivisection has been performed during the past year.—Yours truly, Geoffrey Heald." I am prepared to state what the student was. I have his evidence. I am prepared to state what the student said. It was a student who was discussing the question of his examination in zoology.

13690. That is what you told us; that it was dissection and not vivisection?—Yes.

13691. On that letter you made inquiries about it?—Yes, on that letter I made inquiries. Mr. Sharpe wrote to me first: "Dear Sir,—I wish to write and apologise to you for the rude way in which I have

spoken of you recently, and which I am sorry to say has been published, a thing I never dreamed of. In my hastiness last term I said many things which I have since regretted. I hope you will accept my sincere apology, and regret that this should have happened." I did not accept that. Then he wrote again: "Dear Sir,—I have seen the Warden, and find from him that you do not regard the apology in my letter of yesterday as adequate. As this is so, I should like to state quite explicitly now (1) I am quite prepared to withdraw my letter of June, and to express my regret for having written it. I find that these points—viz., a dog and rabbits having been cut up alive—were quite untrue, but they were told me as facts. I was misinformed, and I cannot substantiate the charges which I and those who supported me honestly then believed to be true at the time. (2) I heartily apologise for having spoken as I did of yourself in my second letter, and withdraw my statement about you. It never occurred to me for one moment that that second letter would ever become public property. I knew that it might go before the Royal Commission, but did not know their evidence would be published. I will do what I can to cancel the publicity given to it by the Press without my consent by writing to Mr. Coleridge withdrawing my charge. I leave you free to make the withdrawal public in any other way that may seem fit to you.—I am, Sir, yours truly, Harold S. Sharpe. We, the undersigned, beg to corroborate this.—Arthur H. M. Peate, Cyril A. Bonser, Alfred C. C. Harvey Evans, Basil W. Truman."

13692. Whether he did communicate it to Mr. Coleridge you do not know beyond that letter—I do not know.

(*Sir Mackenzie Chalmers.*) At any rate, we have had no communication from Mr. Coleridge on the subject.

(*Witness.*) I may say that I have had no communication with Mr. Coleridge except that first letter on this subject at all from first to last.

13693. (*Chairman.*) That clears up the matter entirely, and makes it probable that what you say you learnt from some student was correct, that it was a mistake made as regards vivisection, and that what really was spoken of was dissection?—I have explicit information that that is the source of it.

13694. (*Dr. Wilson.*) There was no vivisection going on?—Not on dogs. I have another letter on the subject.

13695. (*Chairman.*) We have had so many letters that perhaps you had better read it?—This is an inquiry made by the Warden of Keble and Dr. Jackson, the Radcliffe Librarian, on behalf of the museum.

13696. I do not know whether it is worth while having this letter. It seems to me we are getting into too many letters?—I do not wish to bring it forward. It is an absolutely clear case, and can be vouched for by all the students, of course, not only in my department, but in other parts of the museum.

13697. (*Sir Mackenzie Chalmers.*) We have it on the Notes that no experiments were made on dogs at that time?—No vivisection experiments.

13698. When was the last time that a dog was operated on under anæsthetics—how long before, do you know?—I should think four years ago.*

13699. No dog had been operated on either with anæsthetics or without for four years in your laboratory?—No, not operated on. I cannot say definitely, but I do not think there has been a vivisection experiment.

13700. (*Chairman.*) Not for some time before that?—No, not for the last year and a half.

(*Mr. Ram.*) We have an absolute withdrawal of the charges that were brought before us. Need we go further?

(*Witness.*) I should like to say one thing more. The undergraduates were all freshmen in their first year, who knew nothing about science, and I consider that you cannot view them quite in the same light as you would view other people. But I cannot in the same light, I am bound to say, acquit Mr. Stephen Coleridge. He received this second letter; he never communicated it to me; he took no pains to find out whether this was or was not a true statement, and he interpreted it to mean cruelty without any inquiry.

13701. (*Mr. Ram.*) Although he had received your denial?—Although he had received my denial. Moreover, I want to point out again that the second letter which came before him was written in the sort of language which ought on its own showing to have awakened his suspicions. I note, for instance, that Mr. Coleridge himself, commenting in the "Times" on a statement of Lord Justice Moulton, given before this Commission, uses this very excellent language—this is quoted from his letter to the "Times": "Violence of language is only indicative of shallowness of thought and inaccuracy of statement." This is from one of Mr. Coleridge's own letters. It appears to me therefore that Mr. Coleridge, on his own showing, in consequence of the violence of the language of the second letter, might have suspected inaccuracy of statement. I have nothing more to say. *Mr. F. Gotch M.A., D.SC., F.R.S., M.R.C.S.* 30 Oct. 1907

(*Chairman.*) I think that closes that particular incident.

13702. (*Sir William Church.*) What would be your definition for the Commission of the term "pithed," when you speak of a pithed frog? We have had varying evidence given to us upon the point?—I always use the term for the destruction of the whole central nervous system.

13703. The spinal cord as well as the brain?—Yes, I use it in that way.

13704. Therefore in this *précis* "pithed" and "decerebrated" are not the same?—No.

13705. Do you think that there are any reasons for believing that a decerebrated animal is capable of feeling pain?—No, I think there is no evidence for it.

13706. I suppose it may be taken as an accepted fact from human anatomy, that when an injury takes place to the spinal cord, so long as it is not sufficiently high to implicate the breathing apparatus, the body is insensible to pain—the lower part of the body and all below the seat of injury?—Yes, certainly.

13707. Supposing there is a sufficient amount of pressure on the spinal cord of a man, you might say that you can have an amount of mere pressure upon the spinal cord which entirely prevents a man having any sensation below the seat of injury?—I think it is a certain fact that if the spinal cord is cut off (functionally or structurally) from the higher cerebral centres, below that point there is no sensation of pain.

13708. And do you know anything which would lead you to think that in a frog whose cerebral centres were removed the sensation of pain would remain?—No, I know of nothing. I do not wish to put before the Commission the wide question of the evolution of consciousness and pain, but all scientific evidence appears to show that this is a late acquisition, and that pain is a specific form of consciousness having a specific value like such definite forms as visual consciousness or tactile consciousness, and is related to the presence of definite structures, those structures being the cerebral hemispheres.

13709. It has been definitely put before us by a witness, if not more than one, that the view is held by some physiologists that sensation of pain remains even after destruction of the cerebral hemispheres in the lower animals—I mean in cold-blooded animals?—What physiologists, may I ask?

13710. That I cannot tell you. The evidence before us has been: a person has come here and stated that it is a moot question of physiology?—The physiology of the central nervous system, I should say, is the growth of the last thirty years. I do not know a single physiologist of the last thirty years who would make such a statement.

13711. There is one more question that I should like to ask you, and that is this: You think the head of a laboratory, or of a department which might embrace more than one laboratory, should be entirely responsible for the work which goes on there?—Yes.

13712. And therefore that the decision should be trusted to his judgment as to the standing of the men who are working in that laboratory?—Yes.

13713. That, of course, should they in any way either transgress the existing law, or even do anything which in his judgment he thought was not a fit experiment to be made, it ought to be in his power to be able to, in the first instance, represent the fact that they had gone

* *The witness subsequently wrote that after careful inquiry, he could not find any evidence of any licence in Oxford for performing vivisection on a dog with or without anæsthetics during the last five years.*

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outside their licence, if they had a licence from the Home Office, and in the second case to refuse that they should work any longer in his laboratory?—Certainly, that is my view. I should like to make some suggestions later as to the Act.

13714. (*Sir William Collins.*) I was going to take you upon that point myself. I do not think you told us how long you have held a licence?—I am afraid I cannot recollect. I had one before I went to Liverpool. Twenty years, I should say.

13715. And I do not think you told us what certificates, if any, you hold?—I hold only Certificate C at present.

13716. What does that enable you to do?—To give demonstrations.

13717. Have you ever held a certificate enabling you to dispense with the use of anæsthetics?—Completely, do you mean?

13718. Certificate A?—No.

13719. Can you tell us how many animals a year you use for vivisection purposes at Oxford?—I personally only give a few demonstrations. I do not use more than half a dozen.

13720. Could you give us any idea of the number of animals you have used for vivisection during the twenty years for which you have held a licence?—At one period I did a large number of vivisection experiments, so that I should think I must altogether have performed experiments upon 150 mammals.

13721. Not more than 150?—No.

13722. And you have enumerated some animals that you have employed. Was that a complete list?—Monkey, cat, dog, rabbit, mice, guinea pig, rat, frog, and fish.

13723. You have used your licence both for purposes of research as well as for purposes of teaching, I understand?—Yes.

13724. Have you encountered any restriction in obtaining a licence or certificate?—Never. I am bound to say that although I have not done so, a recommendation made by me was not accepted.

13725. In your capacity as a lecturer on physiology?—A recommendation for a certificate signed by me in my capacity as Professor of Physiology in the University.

13726. That is what I intended to say. I thought it was as a lecturer or Professor of Physiology in the University that you are yourself entitled to sign an application for a certificate?—Yes.

13727. Would you tell us what that case was?—It was a case which I considered to be a very important piece of work on the feeding of animals, and the effects produced by deprivation of food in a dog, in carnivorous animal.

13728. (*Sir Mackenzie Chalmers.*) A starving experiment?—Yes.

13729. (*Sir William Collins.*) Is that a case in which a licence was refused?—No, a certificate.

13730. Was that a case, then, in which, although the certificate had been signed, the Home Office intervened to prevent its coming into effect?—Yes.

13731. (*Dr. Gaskell.*) How many days starvation was it asked for?—Either six or seven days. I forget the particulars at this moment. I do not know that it was very definitely stated.

13732. (*Sir William Collins.*) In your *précis* you speak of the importance of extending physiological knowledge, and of the value of physiology in matters of psychology and education, this last, as you say, comprising the appropriate training of both mind and body, and you say that the extension of physiological knowledge is of the utmost importance for the above subjects. I gathered in the course of your examination that you intimated that in your opinion the science of education should be more and more placed upon a neurological basis?—Yes.

13733. Should I be right in saying that you think the study of neurology is desirable for the training of teachers?—Yes, that is my point.

13734. That teachers should undergo a course of instruction in physiology and specially in neurology?—I think they ought to know the elements.

13735. And that they should see these demonstrations to which you have alluded?—That is not involved, is it?

13736. I am anxious to elicit your opinion?—I should have no objection to their seeing demonstrations if they were serious students.

13737. You laid importance on what is commonly known as the heuristic method of study?—Yes.

13738. Should that apply to these cases of teachers investigating neurology?—I think it does, but I think they would generally go through what we may term the first university course, and would not therefore probably occupy the position of senior students.

13739. Should they not become acquainted with the functions of the central nervous system?—They would get their general acquaintance in the first part of this course, which is divided over two academic years.

13740. But is it possible to understand the training of mind and body without understanding the working of the central nervous system?—Some of the main principles of the working of the central nervous system can be grasped in the first year's course.

13741. Does that mean without any demonstration?—No, nor should I advise anybody to do it without doing experiments himself; for instance, in the tissues of the frog. They ought, at any rate, once to see some experiments involving higher reflex actions.

13742. But I want to understand. You place importance upon training in psychology and neurology for the training of teachers. Would you think it necessary for them to undergo a course of demonstration exhibiting the functions of the central nervous system?—I think it is desirable that they should see some typical instance of this reflex co-ordinating mechanism. The other parts of the neurology do not necessarily involve vivisection experiments, not the experiments themselves. They involve the results of vivisection experiments. Even practical work can be done, and different parts examined microscopically without the students themselves having to do a vivisection experiment.

13743. Did I correctly understand you to say that you think it is desirable that such students should have, at any rate, one demonstration upon the co-ordinating mechanism of the central nervous system?—They should see co-ordinating reflexes.

13744. Should that be on the higher mammals, as I understand you to suggest?—I think it should. I suggest the rabbit.

13745. Would not the monkey be preferable from that point of view?—That is an advanced part of the subject. I do not find that the monkey is an available animal, owing to the expense, except for a few advanced students.

13746. I understood you to say that you would only accept convincing evidence for conclusions respecting the higher animals from experiments performed upon higher animals?—I was alluding then to the brain.

13747. I was alluding to the brain?—But the central nervous system is not merely the brain.

13748. I have not heard anyone suggest it in this room?—But I said just now that I think these people ought really to see an example on an animal of this nervous co-ordinating mechanism. That includes the brain, the medulla, and the spinal cord, of course. They must see a control reflex. That is my view; it is only my own view, of course.

13749. Would that include a demonstration showing the localisation of cerebral functions?—No, I should not necessarily show that to students of this type.

13750. Why?—I think it is rather an advanced experiment.

13751. Superfluous for those studying psychology and methods of teaching?—No, I do not think it is superfluous, but you must recollect that the last monkey I had for histological purposes cost £2.

13752. It is a matter of economy?—Largely a matter of economical cost, and many considerations come in in connection with what one would demonstrate, it being implied, of course, that the animal is under an anæsthetic, and killed under the anæsthetics in every case.

13753. I gathered that both for your research work and for your teaching you have never had occasion to perform experiments on living animals without resort to anæsthetics?—No, I have not.

13754. Do you think it is ever necessary to do so?—I think it is absolutely necessary to do so. And that allows me to bring forward the first point I have in connection with the working of the Act.

13755. If you please?—I draw attention to the fact that in the working of the Act there is practically only one certificate, Certificate A, for all experiments which are done in which the anæsthetic is dispensed with. I know it is realised, I may be wrong, that inoculation experiments coming under this certificate are now treated in a separate class, not from the point of view of the certificate, but in the returns that are made I think they are put separately.

13756. (*Sir Mackenzie Chalmers.*) Separately from feeding experiments?—Separately from experiments dispensing with anæsthetics. At any rate, whether that is so or not, I want to draw attention to the fact that there are a very large number of most important experiments which have to be done on animals without anæsthetics, for which I suggest that there ought to be some separate category. These are the experiments of feeding and experiments such as Dr. Haldane is carrying on at this moment on change of environment, alteration in the atmospheric environment.

13757. (*Sir William Collins.*) Experiments not involving a cutting operation?—They do not involve a cutting operation. As it goes out to the public the public is liable to consider that all experiments coming under this certificate involve cutting operations, whereas I think it is the rarest thing. I cannot remember myself a case of an experiment having ever been done without an anæsthetic which has involved a cutting operation.

13758. (*Sir Mackenzie Chalmers.*) Certificate A would not be granted if the experiment involved a cutting operation—anything more than the prick of a hypodermic needle?—Exactly. But that is not sufficiently realised by the public, who imagine that all the experiments which are done of this class under this certificate are what they call cutting operations. I draw attention in the next place to the fact that I consider two of the most important sets of experiments that have to be done under this certificate are the experiments as to food and the experiments as to altered environment. Experiments as to food are being largely done now in America, and I need not point out their importance. The matter has formed the basis of discussion in the British Association Physiological Section at the yearly meeting. The whole question of the minimum diet, whether of the Army, Navy, workhouse, of any diet of any type whatever, is bound up in it. The question of the alterations which occur in the absence of food is not only a very important one from the point of view of ordinary starvation, but involves some principles of great importance for medicine. In fever you have starvation—semi-starvation, as well as a temperature, and we are not yet certain, in consequence of not having experiments sufficiently numerous on this particular head, whether the changes which are produced are produced by the temperature or are produced by want of assimilation of the food. This is a very important point.

13759. (*Sir William Collins.*) What you speak of as feeding experiments would include the starving experiments?—Yes, including the starving experiments.

13760. And would the alteration of environment include such things as drowning?—I should personally have said that that is an alteration of environment, and that experiments must be made upon it. I am aware that that has been before the Commission.

13761-2. Is there anything you wish to add in regard to modifications of the Act or the certificates?—I should like to make a remark (I do not know whether it is worth making) about Certificate D.

13763. Is that the certificate about repetition of experiments?—Yes.

13764. I think it would be very valuable if you would tell us your views about that?—I would point out, about that, that no experiment is a repetition; that every experiment is a display of a phenomenon and its interpretation by the mind of the person who experiments, and this interpretation must be one thing in one generation and another thing in another generation; and it is impossible, therefore, for one to repeat an experiment so far as the whole result is concerned.

13765-6. You mean that the personal equation of the investigators must be brought in?—I mean that scientific knowledge is a group of things which are framed by the minds of men.

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13767. For instance, I think that the argument which you have been developing has specially been mentioned in connection with the experiments by Sir Charles Bell on the spinal cord. It has been suggested that it is unnecessary to repeat those experiments with a view to ascertain the functions of the spinal cord with regard to the motor and sensory fibres. What do you say in regard to that?—I say in regard to that that the introduction of the aseptic method has altered completely our whole knowledge of the way to do experiments so as to limit the structural alteration to the part which appears to be involved; that the experiments which were made before the introduction of the aseptic method ought to be repeated, because the introduction of the new method involves a limitation which could not otherwise be attained.

13768. At one time there was a sharp conflict, was there not, between the evidence produced by Brown-Sequard and that produced by Sir Charles Bell with reference to the function of the spinal cord?—By the Brown-Sequard conflict I presume you refer to the crossing of the sensory paths.

13769. I refer to the suggestion that was made by Brown-Sequard, I think, and supported by G. H. Lewes, that Sir Charles Bell's views were altogether erroneous?—I think that means the crossing of the sensory paths. Brown-Sequard supported a view that the sensory path crossed in the cord.

13770. It was also, I think, in reference to the relative functions of the grey matter and the white matter in the cord?—I am afraid I do not know this particular conflict.

13771. I call attention to the fact that Lewes stated that the doctrine of Sir Charles Bell having been devotedly followed for many years, was shown to be wrong in every particular on the experiments of Brown-Sequard?—What date was that?

13772. 1858?—I have already pointed out that in the last 30 years the whole knowledge of the paths in the spinal cord has had to be entirely modified, due to the circumstance that the introduction of the aseptic method has defined more correctly the obvious lesion. I may point out that the method used is that of ablation—removing the part, and seeing what happens; but you must be certain what part you have removed.

13773. That does not answer the question as to whether there was this conflict or not. But do I rightly understand you to suggest that both must be put aside because they were before the introduction of the aseptic method?—I should think that they both had some elements of right and a considerable amount of wrong, and I should suggest myself that the final details of those who did work upon the central nervous system at a date prior to 50 years ago are more or less fallible.

13774. Would the same description apply to the work at the present time?—Well, we are none of us infallible; but, at the same time, the introduction of that method I have just described does, by its limiting more definitely the lesion, make our knowledge far more precise.

13775. We started from the question of the need or otherwise for the repetition of experiments. It has been suggested to the Commission that in certain areas of research knowledge has been so definitely ascertained that it is unnecessary to repeat by way of further vivisection some of the experiments that have been previously made. I rather gather from you that, in your opinion, no experiment is to be regarded as a repetition of another?—That is my view.

13776. Therefore, this Certificate D is superfluous, and there is really no finality to such research?—That is my view. I am bound to maintain, as a scientific man, that there is no finality in science. I am bound to maintain that attitude.

13777. Do I correctly understand you to suggest, in regard to fallibility, that you thought the earlier researches of Sir Charles Bell and Brown-Sequard stood in a different category from those which are being conducted now?—Yes.

13778. Did I rightly understand you to say that in the course of your 20 years' experience you have not had occasion to use curare on your own responsibility?—I have not used curare. I do not use it for the blood-

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pressure experiments that I show. I have not used it for the research experiments that I have done upon the nervous system.

13779. Did I rightly understand from you that curare, in your opinion, does not paralyse the involuntary muscles?—The involuntary mechanisms.

13780. Is its action in the case of the voluntary muscles upon the muscular fibres or upon the ends of the nerves?—I should say it is upon the ends of the nerves.

13781. And does it not so act upon the termination of the nerves in involuntary movements?—Not with the customary dose. It would with a very large dose, I suppose, but I have no evidence of it.

13782. It is a question of doses, is it?—I have no evidence of its being able to paralyse really involuntary nerve endings, even in very large doses.

13783. Is a change in blood pressure the only mode by which you can objectively determine whether the animal is conscious or suffering pain if under the influence of curare?—I suggest that that is one obvious way; there may be others. I really cannot say. I should have thought the other involuntary mechanisms, in the urinary bladder, for instance, or the intestines, might show similar changes. The reason why I suggest the circulation is because curare is used in circulation experiments, and consequently this particular fact of the condition of the blood pressure is brought prominently before the experimenters.

13784. Is there any other objective sign that you could mention to us which could be relied upon in addition to the variations in blood pressure?—I am afraid that I have not got any evidence of another one, but I should imagine, as I say, that most of the involuntary mechanisms which are largely influenced by pain would give objective signs of this type in the alimentary canal or the urinary bladder, for example.

13785. But you have no evidence of it?—I have no evidence.

13786. (*Dr. Gaskell.*) Would you not modify what you said just now with regard to involuntary mechanisms with respect to the action of curare on the heart? It does abolish that?—The heart is rather in an intermediate position. I was not including that at the moment in what I call the involuntary movements.

13787. (*Sir William Collins.*) Would you say unstriated rather than involuntary?—Yes, I would say unstriated.

13788. That leaves the heart out?—Yes.

13789. Then, in order to ascertain whether any changes were taking place in the blood pressure in a curarised animal, would it be necessary to have a manometer?—Yes.

13790. Without apparatus, then, it would not be possible to determine whether consciousness or pain were present?—No, I do not think so, but I do not really know.

13791. (*Sir Mackenzie Chalmers.*) In your evidence do you speak simply as Wayntlete Professor, or do you in any way come as representative or any body in the University?—Three of us were appointed to represent the Faculties of Medicine and of Science—Professor Osler, myself, and Dr. Haldane.

13792. We may take it that your evidence is on behalf of those Faculties?—Yes.

13793. Is Professor Osler not coming to give evidence?—I have only heard this morning that he is not.

13794. Then, may I ask, does your evidence represent also the opinion of Professor Osler?—Yes, I communicated verbally the *précis* of my evidence to Professor Osler.

13795. He is Regius Professor of Medicine at Oxford?—Yes.

13796. And as regards Dr. Haldane, I forget what he is?—Reader in Physiology.

13797. Is he coming or not?—I think that he would rather allow me to represent the views of the three of us.

13798. Then we may take your views as representative of the school at Oxford?—Yes.

13799. I want to ask you now on one further point. You yourself have medical qualifications?—Yes.

13800. Are there many physiologists now, and teachers of physiology, who are not medical men?—There is a growing number, I should say.

13801. Who make physiology, so to speak, their profession?—Yes.

13802. Do you think that there is any difficulty arising out of that? For instance, when a licence is granted by the Home Office to a medical man, you have the guarantee of his profession and his position in a great profession; but what guarantee have you of competence and humanity in granting a licence to a man who says simply "I am a physiologist"?—You have certainly a guarantee when a man is in a responsible position in a great university or in any university in this country, have you not?

13803. I want you to tell us what your opinion is?—I should say that that is a better guarantee really than the guarantee of belonging even to such a great profession as the medical profession—the guarantee of your being a responsible teacher in a university.

13804. Appointed by a great university?—By a university.

13805. Would you go further in granting licences to physiologists who were not medical men, or would you confine the licences to people who hold university appointments?—No, I think I should require additional information, but I should not exclude them if that information was given by people who would themselves accept the responsibility of giving it—people I have mentioned.

13806. The people who sign the certificates?—You might have a further guarantee if you wished—of that type, I mean.

13807. Now, taking your evidence generally, have I understood you rightly that the main advances in physiology have been within the last 25 or 30 years?—During the last 50 years there have been extraordinary advances in physiology.

13808. In your opinion would those advances have been possible without animal experimentation?—I should say that they are absolutely based upon animal experimentation.

13809. And in your opinion further advances must equally be based on further animal experimentation?—Yes.

13810. As regards experiments before students, you have told us the necessity of them from an educational point of view. We have been told by some witnesses that they have a brutalising effect on character. Have you any opinion on that point?—I do not believe it.

13811. We have been told, for instance, that witnessing surgical operations has not a brutalising effect, but that witnessing a physiological operation would have a brutalising effect, for this reason—that the object in a surgical operation is to save life, but the object in a physiological operation is merely to obtain knowledge. Have you any comment to make upon that?—I make the comment that I do not consider that a serious pursuit for the attainment of knowledge is brutalising. That would be the case with these students.

13812. You have had a large number of students through your hands. What has been the effect, in your observation, upon your own students?—I think that witnessing demonstrations, and even doing experiments themselves upon the pithed frog, are always regarded as a very serious part of a student's work.

13813. Do you think it is necessary for students to go any further in the way of experiment than operating upon a pithed frog?—Yes, I do. I have indicated that.

13814. What sort of licence would you suggest that students should have?—I should suggest that they should have a licence for carrying out experiments belonging to a certain category, which I am not prepared at present to frame, upon an animal which is presented to them in a fully anæsthetised state.

13815. You mean that the professor, or whoever is in charge of the laboratory, should be responsible that the animal is anæsthetised and in a state of complete insensibility to pain?—Yes.

13816. That the animal in that condition should be handed over to advanced students to do certain operations?—Yes, and killed.

13817. Who would be responsible that the animal was kept under anæsthesia until death?—I should have thought the students, under the control of this re-

sponsible official. I do not think that students ought to be allowed to do the experiments without his being present. That is what I mean. They should be done in the presence of a responsible official.

13818. A responsible official who understands anæsthesia?—Yes.

13819. And who would be responsible that the animal was kept under anæsthesia until death?—Yes, that is my view. I should rather limit this control to university officials holding responsible teaching positions.

13820. You mentioned Professor Sherrington's school. What number of students has he?—I could not tell you at this moment.

13821. Are they medical students or training physiologists?—He has medical students and physiologists, and I know he happens to have a considerable class of those who are taking physiology because they are subsequently going to become teachers. May I be allowed to make one more reference to the Act?

13822. Certainly?—I want to bring to the attention of the Commission the extraordinary fact that the Act applies to such a wide group of animals.

13823. Would you kindly explain what you would suggest?—I think it ought to be limited to warm-blooded animals.

13824. The Act should be limited to warm-blooded animals; that is to say, frogs and fish you think have such small sensation that you think they ought not to be included?—I think the whole evidence on general grounds is that pain is a function of the higher animals and evolved as a potent element for certain specific purposes, for producing or restraining certain activities. But that is only a view. At any rate, I maintain that it is inconsistent to penalise work upon fish and amphibia for purposes of scientific research since procedures as regards these animals are quite unpenalised for other purposes—for instance, the whole fishing industry. Then I have a note with regard to the whole question of the supply of animals. I think that is the weakest part of the present position.

13825. That is not dealt with, of course, by the Act at all?—No.

13826. What is your suggestion?—My suggestion is that animals like cats and dogs might be handed over by responsible people, on certain payment, and the only responsible people that I know of are the police authorities.

13827. You mean stray animals that have to be killed?—Condemned animals.

13828. Stray animals that are, so to speak, under sentence of death?—Yes.

13829. That they should be handed over without payment?—Sold by the police authorities for the purpose. I think that there is not merely a sentimental but a possible practical difficulty in the way in which animals of this kind have to be at present obtained In my own case I make every inquiry possible, but I take very few animals of this type at Oxford, and even when one attempts to grow them for histological purposes you see the difficulties one is landed in.

13830. But on the other hand what you mean is that about 30,000 dogs a year have to be destroyed in London alone, and that as they have to be destroyed you would rather have one of those animals than breed a puppy for the purpose?—I would certainly. That is my view. I may say, of course, that when I spoke of growing these animals I did not grow them for experimental purposes, I grew them to use them as dead material, and even then I was subjected to criticism.

13831. You suggested an amendment of Certificate A. Certificate A is only granted when no cutting operation is involved?—It is granted for inoculations.

13832. It is granted for inoculations; it is granted wherever there is nothing more severe than the prick of a hypodermic needle. How would it benefit anybody to split those up?—I do not see why all these experiments should be grouped together under this one certificate. They are all very different. This is the one certificate I should have thought in which you could make categories.

13833. What would the practical object be?—That the public would receive more ample information from the publication.

13834. It is limited to the public?—Yes.

13835. You referred to one case when I think about three years ago a certificate was refused by the Home Office, by Mr. Akers-Douglas?—Yes.

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13836. If I remember aright, I think he said that he could not authorise starving experiments that would last longer than three days?—Yes.

13837. In your opinion would that have vitiated the particular experiment you required?—Yes, the experiment had to go on for longer than three days.

13838. Would it have involved very severe suffering?—Different views are held about that. I should maintain that it would not.

13839. Starving experiments, of course, would come under Certificate A?—Yes.

13840. In your proposed new classification such would be feeding experiments?—Yes.

13841. Modifying food?—Yes.

13842. Then of course Certificate A does include certain kinds of experiments with food—for instance, experiments with new drugs?—I should group those in an entirely different category, as pharmacological experiments.

13843. How would you group inoculations of disease by feeding? When once you get into grouping you get into difficulties. You may inoculate disease by feeding?—They are divided at present, I understand, into physiological, pharmacological or pathological.

13844. You think there ought to be three categories?—Yes. I think there ought to be three.

13845. A 1, A 2, and A 3?—Yes. That is a suggestion merely.

13846. (*Mr. Ram.*) In the third part of the *précis* which you have given to us you say that as regards the character of the conveyed blood as a determining factor in the co-ordinating mechanisms of the higher animals there has been during the last 20 years a remarkable development through the discovery of a number of internal secretions. Have those matters been discovered entirely by experiments on animals?—Not entirely, but the most important of them have been discovered entirely by experiments on animals.

13847. Has the discovery of those internal secretions been a direct benefit to humanity?—I do not know to what extent they have been a direct benefit to humanity, but I should hold, of course, that the next sentence is an answer to that—namely, that by demonstrating an extensive co-ordinating mechanism, which was previously unsuspected, the discovery forms a new starting point, for the whole question of co-ordinating mechanisms has been practically brought forward.

13848. That is exactly what I am upon. That is the good you think that has resulted from those experiments?—Yes.

13849. And may that discovery in its application be a direct benefit to mankind in your opinion?—In the future I should have thought it was likely to be as directly useful as the discovery of the nervous system control. I may point out that one of these new mechanisms is the discovery by Professor Starling of secretion, a means of co-ordinating the mechanism of the pancreatic secretion.

13850. With regard to the use of some of the higher animals, in your experience do animals of a higher class of development experience greater pain than those of a lower class; are they more liable, do you think, to pain?—I hold the view, of course, that pain is a late evolution, therefore I am bound to say that I think they do. I think man of course is likely to experience it in the highest degree of all.

13851. But save in so far as they are affected by the use of anæsthetics, cats or dogs might experience a greater degree of pain in an operation than a rabbit or a lower animal than a rabbit?—That would be my belief.

13852. (*Sir Mackenzie Chalmers.*) A witness told us yesterday that he concluded that animals which have very sensitive smell or sight are more sensitive to pain than animals with less sensitive sight or smell. Is there any connection whatever between special senses of sight and smell and the power of feeling pain?—No, I do not think so.

13853. (*Mr. Ram.*) Can you see any connection between those two conditions at all?—No.

13854. (*Sir William Collins.*) Are sight and smell an earlier or a later development?—Earlier than pain, I should have thought; the relation, if any, is between

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tactile sense and consciousness of pain; but even there the relation is not really close.

13855. (*Mr. Ram.*) With regard to students, Sir Mackenzie Chalmers has asked you some of the questions that I wanted to ask you, but I have one or two that I still want to ask. You would only allow students to deal with an animal under an anæsthetic, which anæsthetic had been given and was watched by a responsible person?—That is my view.

13856. And you would not advocate operations, I understand, by a student in any case in which the animal was allowed to recover from the anæsthesia?—No, I do not think I should allow a student to do it as part of the physiological curriculum or for the mere purposes of teaching.

13857. Of education?—Of instruction. At the same time I am bound to say that I represent only my own view, and that Professor Osler does not agree with me in that; but he treats the matter from the point of view of the use of such experiments for subsequent medical and surgical practice, and he thinks that in many cases it would be necessary that the animals should recover.

13858. (*Sir Mackenzie Chalmers.*) He goes further than you?—Yes.

13859. (*Dr. Gaskell.*) He would rather advocate the John Hopkins' method of treating an animal like a patient?—Yes.

13860. (*Mr. Ram.*) At any rate, the action of the student would only, in your opinion, be desirable when the animal was under anæsthesia?—Yes, for physiological purposes.

13861. And even if Professor Osler's view prevailed and a student was allowed to deal with an animal which ultimately came from under the anæsthesia, at the time that the student was operating on the animal, the animal would be under anæsthesia?—Yes. I should like to add that I think what Professor Osler means is that this should not be physiology, but a subsequent course, as it were—an introduction to the practice of surgical treatment. I only deal with it from the point of view of physiology.

13862. Would you have the responsible person who is to superintend the student, and be present all the time the student is operating, to be a licensed person; would you limit it to that?—Yes, I should limit it to a licensed person. I should further limit it to a person holding a responsible position in a university.

13863. There would be very few of those. There would be some two or three in the university, perhaps?—Yes.

13864. In one answer that you made just now as to amendment of the Act you stated that you thought that work done for scientific purposes should not be penalised, as the Act is at present, by its being limited to cold-blooded animals. Do you regard the Act as to any extent penalising scientific work?—Experiments on fish are extremely difficult to carry out.

13865. Before I come to the question of cold-blooded animals, taking the Act as it stands at present, both with regard to cold-blooded animals or warm-blooded animals, do you regard it as penalising scientific work?—Perhaps I used the wrong word when I said "penalising." May I say restricting?

13866. The Act does restrict it, you think?—It does restrict it.

13867. Leaving out the question for the moment of warm-blooded animals and cold-blooded animals, does the Act restrict scientific work to an extent which, in your opinion, damages scientific research at all?—I think it does by its scope, not by its detail. I think this one question is rather an important one—it is too wide in its scope. I suggest that it is rather absurd that you should not be able to make any experiments for scientific purposes under the Act, at any rate, on fish. It is a very difficult thing to anæsthetise a fish.

13868. Then you would say that all animals other than warm-blooded animals should be exempt from the protection of the Act?—Yes.

13869. (*Sir Mackenzie Chalmers.*) They should come under the ordinary law of cruelty to animals?—Yes.

13870. (*Mr. Ram.*) Now with regard to warm-blooded animals, do you regard the Act as restricting scientific research?—No, I do not think it restricts it. I think it has operated well.

13871. A good deal has been said by some people who have been before us as to the desirability of making inspection more effective. Have you ever in your own experience been visited by the Inspector?—Yes, I am visited by the Inspector two or three or four times a year.

13872. Do you find his visit any nuisance to you at all?—Not in the least. I often am not there; that is because I do not know when he is coming.

13873. If it was thought to be otherwise desirable and feasible, should you, so far as you are concerned, have any objection to the Inspectors being multiplied in numbers and their visits being multiplied?—Not at all.

13874. With regard to curare, you have told us the effect of curare, and we have had evidence as to the amount of the use of it. Would you tell me, in the small number of cases, for we are told it is only small, in which curare is used, is the effect of curare so essential to the success of the operation that to forbid totally the use of curare would be a serious impediment to scientific research?—I think there are lines of inquiry in which it would be a very serious impediment. For instance, supposing you wished to ascertain the working of the nervous system, the discharge of nervous impulses from the nervous system without these nervous impulses being able to produce the movement of a muscle, under those circumstances it would be absolutely necessary somehow to block the end of the passage into the muscle.

13875. And the absence of curare would make that experiment impossible?—I do not know how it would be done in any other way.

13876. Would it be possible to limit and define those cases?—I think they could be more or less defined, but I think it would be difficult. It is difficult at any one stage of scientific knowledge to make a definition. That is my difficulty.

13877. Do you see any objection to its being enacted that there should be a special certificate for the use of curare?—No, I see no objection to that at all.

13878. One question as to starving operations. The carnivorous animals, as we know, can go without food for a very long time without dying?—Yes.

13879. Would it be possible in your opinion to allow starving experiments to extend with regard to carnivorous animals for such time as might be useful for science, and yet not so long as to cause any material pain to the animal?—I do not know that there is any material pain (that is my difficulty) after the discomfort of the first few days. I do not think the later stages, so far as all our evidence of starvation goes, even in man, are associated with material pain.

13880. Then if the animal suffered any pain, it would be in the first two or three days?—Yes, the discomfort would be at the start.

13881. (*Sir Mackenzie Chalmers.*) Did those exhibitors, those men who starved for forty days, suffer pain?—I do not know.

(*Dr. Gaskell.*) Then is it necessary to have a licence at all? The Hon. Stephen Coleridge told us that it was not.

13882. (*Mr. Ram.*) Should you feel justified in performing a starvation experiment without a licence?—No.

13883. (*Dr. Gaskell.*) Although you think there is no pain?—I think there is discomfort. I may say that I represent also the view of Dr. Haldane about his own experiments, which are in very much the same category; they are experiments upon alteration of environment, especially in compressed air, and are performed at the Lister Institute. There he has a licence, and on my asking him why, he said, "I cannot say that at the start there is not discomfort."

13884. (*Sir Mackenzie Chalmers.*) He has tried it on himself?—He has tried it on himself, and he found that there is some discomfort.

13885. Some discomfort at first?—Yes.

13886. (*Dr. Gaskell.*) You said just a minute ago that you would have no objection to a multiplication of inspectors. Would you adhere to the Hon. Stephen Coleridge's desire that an inspector should be present every time?—No, I do not think it is possible that he should be present every time, because then you could not perform even a simple demonstration to your class without waiting for his arrival. I understand the question to mean that there would be more inspectors

than there are at present, who would pay, I presume, surprise visits.

13887. Not that they should be so much multiplied as to be always present?—No.

13888. (*Mr. Ram.*) I understand that your objection would not be to the presence of the inspector, but to the inconvenience of always waiting for him?—Yes.

13889. You would not mind his being there?—No.

13890. (*Dr. Gaskell.*) Then with respect to urethane, I want to ask you a question. The Hon. Stephen Coleridge in the Bill which he proposes to bring forward, proposes to limit the anæsthetics to respirable gases. Have you much experience of urethane?—I use in my demonstrations either chloral-hydrate or urethane almost entirely.

13891. How many years ago is it since you used it?—The last two years urethane, and chloral hydrate the last four or five years.

13892. Do you consider that that is as efficient an agent for producing anæsthesia as a respirable gas?—Yes, and I prefer it because the dose being one which is derived from the absorption of the substance in the alimentary canal must go on gradually increasing to a certain point. You are not dependent, in short, upon the working of an external apparatus for maintaining your anæsthesia.

13893. With respect to the question of cold-blooded animals, do you know Harrison's experiments on cutting out portions of tadpoles and joining them together again?—I know of them.

13894. I presume that under the present condition of things those experiments would require a licence in England?—Certainly. I suppose they would require Certificate A.

13895. Still an experiment on a crayfish would not require anything of the kind?—No.

13896. In your opinion is not a crayfish a much more sensitive animal than a tadpole?—I think it has got a more complex nervous system, though I much doubt whether it has the particular sort of nervous system which involves a consciousness of pain.

13897. (*Sir William Collins.*) Is the crayfish a later or an earlier evolution?—An earlier evolution I should say.

13898. (*Dr. Gaskell.*) Do you consider that analgesia, the absence of sensation of pain, is not necessarily coincident with anæsthesia?—I consider that it is not necessarily coincident with anæsthesia.

13899. Which would you consider would come first?—Analgesia, by which I mean the absence of pain as in a large number of cases of hypnotism.

13900. (*Chairman.*) What other perceptions would survive when the analgesia came on and not yet anæsthesia? What would the other perceptions be that were left?—I think sight and hearing and touch might still be left as in the case of the hypnotic condition in which it is evident that the person sees and hears and feels and is entirely insensible to pain. There are cases of that description.

13901. (*Dr. Gaskell.*) Quite so. Have you made use of Certificate B at any time—to keep the animal alive after the operation?—Yes.

13902. Has your attention been called to the evidence of the Hon. Stephen Coleridge on this matter?—No.

13903. (*Sir William Collins.*) Do you hold Certificate B now. I thought you told me that Certificate C was the only one you held?—It is the only one I hold now. But I have held Certificate B and done a large number of experiments under it.

13904. (*Dr. Gaskell.*) I want to know what in your opinion is the meaning of the term "initial operation." The reference is at page 202, Q. 11473. The Hon. Stephen Coleridge there said, in answer to a question of mine: "My point is that under certificate B, as now worded, anæsthesia must be applied during the initial operation of opening the animal and fixing the electrodes to different nerves, or whatever it may be." Then I say that "under Certificate B the animal can be kept in that condition on the board, and allowed to recover its consciousness, and the animal can be kept there disembowelled, or whatever it may be, as long as the operator chooses, without any anæsthesia to allay its sufferings"?—That is an entire misconception, I should say, on Mr. Coleridge's part of the meaning of Certificate B. I hold, and everybody who has got Certificate B always holds, that anæsthesia must be applied during the whole period of any cutting operation.

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13905. What is the meaning of that period, the initial operation? Is it until the wound is sewn up, or what is it?—It is until the wound is not only sewn up, but I should say dressed.

13906. Dressed too?—Yes.

13907. That is your meaning of the initial operation?—Yes.

13908. And, so far as you are aware, is that the meaning that all physiologists give to that term?—Every physiologist that I know of gives that interpretation to that term.

13909. The only person, then, that you know of who does not give it that interpretation is the Hon. Stephen Coleridge?—Yes. I may say that it strikes me as a most extraordinary statement. I had not had my attention directed to it.

13910. As we are on Certificate B, there was a suggestion made by Lord Justice Moulton with respect to experiments that may involve pain such as those under Certificate B. That would be one class. That is on page 261. He advises that "certain men of acknowledged capacity in research, who have shown that they are not only capable of serious research, but have already done it, should have general certificates entitling them to perform such experiments, subject only to one thing, and that is that they should report fully every experiment. They are immeasurably better able to judge what is worth doing than any body of men that you could put over them. They are the people who are working at the subject itself, and they are, as a rule, men of such high position and high character in England that there can be no ground for being afraid of trusting them with this power." I should like to know whether you think that is a good suggestion and whether you agree with it?—I think it is a good suggestion and I agree with it, but I do not think we are seriously hampered by the working of the present Act except in regard to its scope.

13911. Do not you think that the difficulty of having to get a new certificate if you want to change the animal you are working on is a bit of a hamper?—It involves writing some letters, but I have never had any difficulty.

13912. Does it not involve loss of time?—Yes, it involves a little loss of time. I think there would be no objection to this any more than an objection to the present system, so far as I can see, and that it would be, on the whole, a more satisfactory method of giving the power.

13913. Then there is one other suggested alteration in the Act that I should like to have your opinion upon, and that is, whether it would not be advisable, certainly in the case of animals operated on under Certificate B, to allow them to live in some other place than the laboratory where they were operated upon, to have an outside farm place in which such animals could be kept. The present Act insists that they should be kept in the same place as has been licensed?—Of course, I think it would be an immense advantage, not only for science, but for the animals themselves.

13914. Would you strongly advocate that the Commission should report in favour of such a change as that?—I should. At the same time I should not wish it to be made necessary that you should transfer the animals.

13915. No, but that they should have power?—That it should not be permitted to put them in better hygienic surroundings seems to me to be wrong.

13916. Only one other question. You know most of the physiologists, or I might say all the physiologists in England practically. Have you any evidence whether or no they are kind-hearted men, who are fond of animals?—I think it is rather a remarkable thing that all physiologists almost are fond of animals, and most of them, so far as dogs are concerned, are particularly attached to dogs and keep them as pets.

13917. And the dogs are particularly attached, naturally, to them?—Yes.

13918. (*Mr. Tomkinson.*) You hold very strongly that any limitation of the freedom of choice of mammals or the higher living animals from experiment would be a distinct blow to the researches of science?—Yes.

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13919. In what order do you place the highest animals? Do you put the monkey first as being nearest to man in structure?—Yes.

13920. And on the whole you think in—whatever you call it—instinct or intellect?—No, I put the monkey as being nearest to man because the peculiarity of man is the development of the nervous system, and in the monkey the nervous system is the most highly developed; but I do not consider the monkey to be as near to man from the point of view of the digestive and excretory organs.

13921. And the dog next, I suppose?—The dog is nearer to man from the point of view of the digestive and excretory organs.

13922. Do you think it possible to draw an exact and a very marked dividing line between the highest, the most developed and most intelligent animal, and the very lowest form of human being known to us?—Yes, I could draw that distinction.

13923. Quite enough, apart from other considerations, to make experiments upon the one unthinkable and upon the other quite permissible?—Yes.

13924. What is the object of these operations for the sake of the investigation of the circulation and blood pressure? May I ask whether in those are included experiments for watching the operation of shock?—I presume that that would be one. But the object, as I understand it, is that the only means by which one organ can influence another is either by sending a message to the nervous system and so reaching it, or by the direct plan of the circulation, which, of course, passes through them all and is common to them all, by which I mean the assemblage of the whole organs. Therefore it becomes an extraordinarily important feature in an animal to extend our knowledge so far as possible of this means of intercommunication—viz., the circulation—and that of course is the object, as I understand it, of experiments upon the heart and blood pressure.

13925. But have not experiments of that kind and with that object been made over and over and over again, and is it necessary that they should be repeated?—Yes, and I have no doubt that they will be made over and over again.

13926. Is there something new to be discovered?—Something new is to be discovered almost in every succeeding year.

13927. Have you read a description of 14 cases of operations upon dogs by Dr. Crile?—No, I have not read them. I know the general character of the work, but I have not read it.

13928. You told us that the preservation of circulation was possible, even in an animal when paralysed by anæsthesia, or with the circulation absolutely destroyed by decerebration—that is, the taking out of the brain?—Yes, of the cerebral hemispheres.

13929. Can this circulation go on after the brain is taken out—that is, in some animals, I suppose?—The cerebral hemispheres—not the whole brain.

13930. Does pithing mean the severing of the spinal column?—I do not use the word pithing in that sense.

13931. I mean a pithed frog?—A pithed frog is one in which the whole central nervous system has been destroyed, including the spinal cord.

13932. Is the head severed from the body?—No, the whole system is destroyed in the usual way.

13933. And you are satisfied then that the frog cannot feel?—There is no central nervous system left at all then.

13934. (*Chairman.*) Is the brain taken out and the spinal cord also?—A hole is rapidly made, and by means of a wire the brain and spinal cord are destroyed with great rapidity.

13935. (*Mr. Tomkinson.*) In the case of the experiment of starvation of a dog, would water be withheld as well as food, or allowed?—In the particular experiment for which the Home Office refused a certificate water was not to be withheld. I should say that the experiment could not go on if water was withheld.

13936. In these cases of fasting by men, whether they were actually *bona fide* or not (there was some question about that, particularly in the case of Succi, whether he did not have some food), unlimited water was allowed?—Yes.

13937. Would that, in your opinion, tend to mitigate what are called the pangs of hunger?—Yes, I think so. There might be an experiment made upon the effect of withholding water, but ordinary starvation experiments must be accompanied with allowing the animal to have large amounts of water.

13938. But you rather seemed to think that the pain of starvation is negative rather than positive?—I think it may be expressed by early discomfort better than any other word.

13939. Then you think that the general opinion of the pangs of hunger and the dreadful pain of starvation is somewhat exaggerated?—I do. I think, of course, that provided you take water or something into the cavity of the stomach you will not get these particular cramps.

13940. I want to ask you one question as to the ground of your belief in the insensibility of animals, whether there is no analogy to be drawn between the sensitiveness and the development of the senses in an animal like the dog, and its application to its feeling. You admit that out of its five senses three are extremely highly developed, its sense of sight, of hearing, and scent. Everyone must admit that who knows dogs?—Yes, certainly.

13941. And as regards the fourth sense, taste, we do not know about that, but taste and smell are very close to one another, therefore we may presume almost that the dog has that fourth sense?—Yes.

13942. Then on what possible grounds are we justified in saying as we do, knowing that three at least of the senses are most highly developed, that the fifth sense, that of sensation, is duller in the dog than in man?—Do you mean by the fifth sense cutaneous sensation?

13943. I mean the power of feeling?—You mean touch.

13944. I mean the power of suffering?—Do you mean the sense of pain or the sense of touch?

13945. I put the two together?—But they are quite distinct. As I have already pointed out, the sense of pain and the sense of touch are quite distinct.

13946. But surely the sense of touch is sensitiveness?—I do not wish to go into the details, but even in a man it can be shown quite plainly that there are certain points of the skin most susceptible to contact; there are quite other points most susceptible to heat, quite other points most susceptible to cold, and quite other points most susceptible to the particular pressure which produces pain.

13947. But is it wrong to say that the five senses are seeing, hearing, smelling, tasting, feeling?—They have been greatly extended.

13948. I am afraid that the man in the street does not think so, and I am the man in the street?—I am afraid that the cutaneous sensations, as they are termed now, really comprise groups as different from one another almost as sight from hearing.

13949. The sense of touch is most highly developed in the ends of the fingers?—Yes.

13950. And surely the ends of the fingers, or the ends of the toes, are the parts of the body most sensitive to pain?—I think there are spots in your arm which could be demonstrated which would give you more pain.

13951. I would rather be vivisected in any other part of my body than in the ends of my fingers or the ends of my toes?—Then may I point out that you can touch hot things with your fingers.

13952. Only for an instant?—Without your being able to bear that temperature in other parts of your skin.

13953. Only because the skin at the end of the fingers may be a little rough or hardened by work when they have not got a covering like a glove.

13954. (*Dr. Wilson.*) I observe that you have not used dogs for two years at Oxford?—Not for vivisection experiments but for histology.

13955. As dead animals merely?—Yes.

13956. You do not believe in the necessity of demonstrating these blood pressure experiments before your class?—Yes, I demonstrate them on large rabbits, and sometimes on a cat.

13957. So that according to you—at all events, according to your experience at Oxford—dogs can be dispensed with altogether in these experiments before students?—Some things cannot be done without dogs, but then I do not do those things. I do not say that

they are not advisable to be done. For instance, the influence of the nervous system on secretion would be very difficult to do upon any other animal except a dog or a cat—preferably a dog.

13958. You do not consider it necessary for your class?—I do not do it, but I have a difficulty in the supply of animals.

13959. That is the reason, is it?—That is one reason.

13960. You say that no experiments are final?—That is my belief.

13961. The results of each experiment must be translated in a different way by different experimenters, there is that possibility always?—Yes, I should say that.

13962. So that there can be no finality?—I do not think there can be finality.

13963. And, of course, you strongly advocate experimentation on animals purely from the scientific point of view, not as an aid to medicine or surgery, or the relief of pain. You think it is absolutely essential for the acquirement of useful knowledge as a part of science?—Yes, that is, if I may say so, the trunk of my tree of knowledge. I think that physiology is in the trunk, but you cannot get the fruits, of course, if you damage the trunk. Some of these fruits are the medical and surgical advancements which result in the relief of pain. *Mr. F. Gotch, M.A., D.Sc., F.R.S. M.R.C.S.* 30 Oct. 1907.

13964. But supposing that it were even possible to prove to your satisfaction that no further cutting experiments on animals are necessary for the advancement of either medicine or surgery, you would still advocate the performance of these experiments before students?—I cannot really conceive such a condition, but I should advocate the advance of scientific knowledge under any circumstances.

13965. Would you use urethane and chloral hydrate instead of chloroform if you were experimenting with dogs?—I have had no experience of the use of urethane and chloral hydrate with dogs.

13966. Your laboratory, of course, is not very far from houses?—It is in the Parks. It is some way from houses, but still it is very open to the public.

13967. But to allay the sensibilities of the public you would recommend that laboratories for experiments on living animals should be at all events kept away from houses?—I suppose it is necessary, or at any rate desirable.

Mr. M. S. Pembrey, M.D., called in; and Examined.

13968. (*Chairman.*) You are a Doctor of Medicine and Lecturer on Physiology at Guy's Hospital?—I am. *Mr. M. S. Pembrey, M.D.*

13969. And a member of the Army Medical Advisory Board?—Yes.

13970. As a lecturer on physiology, I presume that you have had experience in experiments on living animals?—Yes, I have had considerable experience, both from the point of view of research and of teaching. I have taught physiology at Oxford, Charing Cross and Guy's.

13971. I will ask you shortly first of all a question which has been asked of many witnesses. Do you consider that the demonstration of experiments to pupils is or is not a valuable or essential part of education?—I think it is absolutely necessary if they are to obtain a proper knowledge of physiology in its application to medicine. I may take one example—coal-gas poisoning. We can show coal-gas poisoning on a mammal such as a rat or a rabbit. It takes 30 seconds to kill a rat or a rabbit with coal-gas. I can then demonstrate to the men the fact that when an animal is poisoned by coal-gas, as is perfectly well known in man, it dies a painless death—it becomes gradually unconscious, then gives convulsive movements, due to asphyxia, and dies—from lack of oxygen. If when it is dead, one cuts open the animal, one can at once demonstrate the characteristic change in the colour of the blood. Of the application of that to coal-gas poisoning we have a case in hospital at the present time. One points out to the men that the colour is quite characteristic, and the same knowledge is seen in man. And one can also show them on animals the treatment of such a case that it is no good to transfuse, as the chief thing to do is to give oxygen. Further, one can point out to them that an animal, such as a rat or a mouse, can be used as a test for the presence of carbon monoxide in the air of a mine after an explosion, and show them that by sending down an animal such as a rat or a mouse it can be used as an indicator of the presence of a poisonous quantity of the gas. This method is used. That is only one instance. Would you like me to give some further instances?

13972. If you would give us shortly one or two?—I may take another instance from experiments on the nervous system. This is one of the cases which is objected to by the anti-vivisectors. One can show upon a frog that after destruction of the cerebral hemispheres most complex movements can be carried on. The practical application of that one can bring out by showing specimens of brainless monsters. These brainless monsters, as it well known, can breathe, suck, cry and perform complicated movements, but there is no evidence of course that they are conscious.

13973. I do not quite know what a brainless monster is?—I am using the term "brainless monster" in the medical sense, that is a child born without any cerebrum. It ought not strictly to be called a child, because it is not really an individual. Then following from that example one can lead up to all the complicated mechanisms which can still act in a man who is perfectly unconscious. I could give more instances if you like, they are so many, and we use them every day. These of course are experiments which do not come strictly under the Act. Perhaps it would be well if I were to give an experiment which comes strictly under the Act, an experiment in which it is necessary to give an anæsthetic. I think it is necessary to do some experiments upon an animal under anæsthetics in order to demonstrate fundamental points on the circulation and respiration—to show, for example, the influence of opening the pleural cavity. Medical students find it very difficult to understand the mechanism of the pulmonary ventilation unless they see these experiments; they cannot form a definite idea of the contraction of the heart and of the passage of the blood through an artery and vein unless they see these experiments upon a mammal. A frog is not big enough to show the difference in the circulation in an artery and a vein. I could give many instances, but I think it would only take up your time.

13974. These are instances of the kinds of experiments which you think are absolutely necessary?—Yes, I think they are absolutely necessary.

13975. I want now to ask you some questions with reference to a statement made by Miss Lind-af-Hageby in her book, which is referred to at Question 9354 and following questions. The statement was: "We once saw a marmot the spinal cord of which had previously been divided by the vivisector," and then the story continues. You, I believe, were acquainted with that marmot?—It was my marmot.

13976. Was it kept at Guy's Hospital?—Yes.

13977. Was this experiment at Guy's Hospital?—No, it was at the University of London, South Kensington.

13978. And you brought the marmot there on that occasion?—Yes.

13979. I ask you at once, because there seems to have been considerable dispute about it, was the spinal cord of that marmot ever divided?—No, that is absolutely untrue. I have the body of the marmot here, and if you like I can show it. Perhaps I had better do it at the end of my evidence, and then I can wash my hands.

13980. How long ago did it die?—It died in 1903, but I think this marmot ought to be thoroughly well examined.

13981. Has it been preserved?—Yes, it has been preserved.

13982. Then I think you had better show it?—Shall we leave it to the end or shall I bring it out now?

13983. I think you had better do it at once—it is rather a principal point of your evidence?—Of course it may be suggested that this is not the marmot, but my laboratory man is here, and can confirm my statement that this is the marmot. (*Exhibiting the marmot.*)

13984. (*Sir Mackenzie Chalmers.*) You know that it is

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the same marmot?—Yes, but if my word is not good enough my laboratory man can confirm it, because that may be a point of dispute.

13985. (*Chairman.*) No, that is not the point, I think?—This is the same marmot that Professor Thane examined. We did not try to mount the specimen after Professor Thane examined it. I thought of having it macerated so as to obtain its skeleton. I can cut down on to the spinal cord and show you that it has not been divided. It is as Professor Thane examined it, absolutely intact. There has never been any operation on this marmot at all.

13986. (*Sir William Collins.*) Has the spinal cord ever been exposed?—No, there has never been any cutting operation on this marmot at all. I am perfectly willing to cut through the skin and show you that the cord is undivided. I have got my laboratory man to bring a scalpel on purpose.

13987. (*Chairman.*) No, I think the Commission do not need that?—I think, at any rate, it ought to be on record that I offered to do so.

13988. Miss Hageby says that she never saw the marmot, except on that one occasion, and from what she saw on that occasion she spoke?—Well, that is the marmot.

13989. There was some question about whether the marmot had been used for experiments?—That statement is also completely false. No experiment under the Act has been performed upon this marmot by me, nor has any experiment under the Act been performed by me upon any marmot.

13990. When you say no experiment under the Act, what do you mean exactly?—I use the term in that sense simply for this reason, that confusion has arisen because Professor Thane said that no experiment was performed except taking the temperature. Taking an animal's temperature I do not consider to be an experiment, and that is the whole point upon which the apparent conflict about Professor Thane's evidence rests. It is simply the use of the term "experiment." I told Professor Thane that no experiment under the Act had been performed. The taking of an animal's temperature is not an experiment.

13991. Was the temperature taken with a view to ascertain what was the effect of hibernation upon temperature?—Yes. Simply an ordinary thermometer put into its bowel or into its mouth.

13992. Were there any experiments of starvation?—There were certain fasting experiments, and in reference to that I sent down to the Commission some letters which appeared in the "British Medical Journal."*

13993. In the experiments on fasting, was the animal hibernating at the time?—No. I have a copy of a paper here containing an account of them. They were experiments upon the marmot during the deposition of fat. These animals can put on an enormous quantity of fat, as was shown by this paper, and in order to study it on some occasions the animal was kept in a fasting condition, and exception was taken to the one in which it fasted for 52 hours. The confusion is simply due to ignorance that hibernating animals, as is notorious, often in the summer will go into a sleepy condition, and take no food for days, because they have a large storage of fat—they can almost double their weight by putting on fat—so that it does not matter to them in the least if they go without food for a week or more. In the winter, for example, they may go without food for six months.

13994. I do not know that this question of fasting is very material. What I want to know is whether the experiment—testing the effect upon the marmot of fasting 52 hours—was a natural hibernation in the summer time, or was it that while it was lively and willing to feed it was not fed?—All the conditions are stated in the paper. The rectal temperature, the weight, and all the conditions are given in this paper† (*handing in the same*). The animal was then, of course, awake; it was not hibernating. But even when they are awake they often for days take no food. In support of my word I have references to the most recent monographs upon hibernation, and I can give you them to confirm my view.

13995. Perhaps you will just give us the reference?—As a general point, it is well known that in warm climates animals often hibernate in the summer; I will give the reference to that. *Ergebnisse der Physiologie*, third year, second part, pages 220 and 221. That is the most recent work in Germany on hibernation.

13996. The only experiment in the way of feeding, I understand, was leaving the animal without food for 52 hours on one occasion?—Yes.

13997. And something less on another?—Yes; on one occasion 52 hours and on another 42 hours. They are all given in this paper.

13998. Did the animal survive that?—Yes. They would survive, as I said just now, weeks of it in the summer, if that were done. That has been done, but not by myself.

13999. What was the object of it?—The object was to see the contrast between fasting and feeding.

14000. The contrast in what way do you mean—in the appearance of the animal or in the temperature?—In the animal's temperature and respiratory exchange. The experiment was on the gases that it gives out from its lungs and bear upon the question of the deposition of fat.

14001. (*Mr. Tomkinson.*) And the weight?—And the weight.

14002. (*Sir Mackenzie Chalmers.*) So far as you could tell, did the animal suffer or not?—It suffered absolutely no pain. I have two marmots now, and I should like, in support of my evidence, to bring up one of these marmots, or to let you have the two marmots so that someone can keep one, without your knowing which, without food for 52 hours, and then you could not tell the difference. There is an opportunity for you to make the experiment. I will provide the two marmots, and you will not be able to tell the difference unless by delicate weighing.

14003. (*Chairman.*) I think we must be content with your evidence. This is very much a by-point which happens to have cropped up?—If I may say so, I think it is frivolous, because I am willing any day to fast for 52 hours without suffering pain.

14004. You are not a marmot?—But a marmot can do it all the better.

14005. Yes, I know the marmot has the advantage of you?—Yes.

14006. You say that those are the only experiments that were performed upon this particular marmot?—No experiments under the Act were performed upon it. Those were only observations, and they are not under the Act.

14007. (*Sir Mackenzie Chalmers.*) They are not under any certificate or licence?—No.

14008. You did not consider them painful?—I did not consider that they came under the Act as painful, and therefore I did not apply for a certificate. The responsibility is thrown upon me of determining whether experiments come under the Act or not, and I think it would have been frivolous if I had applied for a certificate. I could have applied for a certificate, and have protected myself from bother, but I think it would have been frivolous to do so.

14009. (*Chairman.*) We know now that the spinal cord of this marmot has never been divided; you have produced the marmot. What was the condition of the marmot on that day of which Miss Hageby spoke?—It was in a condition of hibernation, and it was waking up. I can quite easily explain why the mistake arose as to the paraplegia. The condition of the animal was that the hind legs were quite paralysed and drawn out behind the body. Instead of the animal walking like that (*describing*), it would be drawn out thus in a condition of paraplegia. This is mentioned in one of my papers, and there is no excuse for the statement that the animal's spinal cord was cut, because I can give you references to the actual papers, in which I say that no operation had been done, and in which I also give the explanation of the paraplegia. On page 80 of this paper of mine I actually explain the paraplegia (*handing in the same*).‡

14010. (*Sir William Collins.*) When was that paper published?—In 1901, two years before that statement in "The Shambles of Science" was published.

* *May 23rd and 30th, 1903.*

† *Journal of Physiology*, 1901, *vol. xxvii., p.* 407.

‡ *Journal of Physiology*, 1901, *vol. xxvii. p.* 80.

14011. (*Chairman.*) It does not follow that it had been brought to the author's notice?—No; but she refers to these papers in the Blue-book and in the evidence too. Further, I can show by reference to the *Ergebnisse* again that the condition of paralysis of the hind legs is perfectly well known to all people who have worked at the question. I can give the reference to that if the Commission wish it.

14012. I do not think it is necessary?—It is a thing that should be known. I can give the pages—227 and 228 of the *Ergebnisse.*

14013. (*Sir William Collins.*) Are the symptoms just the same as from paralysis after division of the spinal cord?—Yes. A very interesting thing also is that as the animal wakes up this paralysis passes off, and on that very day, at the end of the demonstration, by the time that the marmot got back to Guy's there was no paraplegia at all. It is a condition that comes on each winter.

14014. (*Chairman.*) Did the marmot live for some time after that?—I gave the lecture in 1902. It lived for a year, and you see that marmot now upon which, if the Commission wish to see it, I could demonstrate in one moment that the spinal cord is uncut.

14015. (*Sir Mackenzie Chalmers.*) What did this particular marmot die of a year afterwards?—It died of an abscess in its throat. There was no experiment done. The animal got an abscess in its throat which extended to the eye, and it died. It was kept practically as a pet from 1898 to 1903. During this winter I am willing, if the Commission wish, to demonstrate the paraplegia upon two marmots which I now have at Guy's.

14016. (*Chairman.*) If our functions are extended to examining into the habits of the marmot we will avail ourselves of your offer, but at the present time I do not think it is necessary.

14017. (*Sir William Collins.*) Is that the only marmot that you had at that time?—That is the only marmot I had at that time, and I believe it was the only one that any physiologist had in England.

14018. (*Chairman.*) I think in answering those questions you have answered also some attack that was made upon you in a newspaper to which you wish to refer. I think that is sufficiently answered by what you have told me?—Yes. I would like simply to show the way in which we physiologists are misrepresented. Complaints were made that I was freezing hibernating animals by exposing them to a temperature of 10°. That complaint was made and inquiry was made about it. I was asked to supply details about it. It was an absolutely fallacious statement on the part of the anti-vivisector. It was 10° Centigrade, which is the ordinary temperature of my room in the winter. They thought it was 10° below freezing point upon the Fahrenheit scale.

14019. (*Sir William Collins.*) Has that statement been made in the Blue Book?—No, it has not. This is a copy from a letter to the "British Medical Journal."

14020. (*Chairman.*) I think we are getting rather beyond the question. We are not looking up all the statements which have been made by anti-vivisectors and then proceeding to contradict them. We are inquiring into the working of the Act?—The reason why I put it in was because they did consider these to be painful experiments.

14021. It was somebody writing in the papers that they did not consider this or that, but it is not really material; it is not an attack that has been made specially upon you with reference to any evidence that you have given or anything of the kind. Those I think are the points to which you wished to draw attention, especially as regards the marmot?—There are other points, my Lord. I wish to refer to the answer to Question 7410. That is with reference to a joint book published by me and the late Dr. Phillips: "The Physiological Action of Drugs."

14022-3. This was in MissLind-af-Hageby's evidence?—Yes. I wish to bring to the notice of the Commission that it is carefully pointed out in the preface to that book that the law relating to experiments on animals renders necessary the destruction of the animal's cerebral hemispheres. I say that to me the statement of the witness is absolutely misleading, as I carefully pointed out in the preface what was the law, that these experiments must not be performed without destruction of the cerebral hemispheres, and I contend that, so far as I can understand Miss Hageby's evidence, it maintains that we did the opposite.

14024. The statement there is that you said that (*after referring to the passage*) physiologists looked upon experiments on frogs as legitimate for students to perform?—Yes, and I point out in the preface of this book that on account of the law it is necessary in all these cases that the cerebrum should be destroyed.

14025. In frogs?—In frogs; so that Miss Hageby's evidence absolutely gives an entire misrepresentation of what we say in the preface.

14026. I do not think that is quite so. Surely an experiment on a frog with its brain taken out would come under the Act?—No, it is not under the Act. A frog with its brain removed is not under the Act, and that is carefully stated in the preface.

14027. You do not mean that the Act excepts frogs altogether?—No.

14028. Do you say (the Act does not say so in so many words) that if you perform an experiment on an animal which kills it it is not an experiment?—No; it is not an experiment under the Act, if the brain has been destroyed.

14029. If part of the experiment is killing it?—Then you cannot kill the animal in any possible way without doing an experiment.

14030. You kill the frog by pithing the brain?—By destroying the function of the brain.

14031. (*Sir William Collins.*) Is that pithing that you are speaking of?—I carefully say destruction of the cerebral hemispheres.

14032. Is that pithing?—I say that pithing is the destruction of the total central nervous system. That is the definition which I use of pithing throughout the book.

14033. Destruction of the cerebral hemispheres would not, in your opinion, be pithing?—Not in the sense in which I use the word, and not in the sense in which I teach my students. I carefully draw attention to that—that pithing is used in different senses; by some observers it is used for the complete destruction of the central nervous system, and that is the sense in which I use it.

14034. (*Chairman.*) You complain that Miss Hageby says that you have spoken of frogs as animals upon which students might operate without first stating that you had said that they should only operate after the frogs have been pithed?—Yes, after pithing the cerebrum.

14035. (*Dr. Wilson.*) After decerebration?—Yes; thank you.

14036. That is the difference?—Yes. Then there is a chapter to which I would like to refer in the "Shambles of Science" on "painless experiments." It refers to a lecture of mine, and seeing that it is full of false statements, I should like to have an opportunity of being taken right through them. It begins at page 11 of the book.

14037. (*Sir Mackenzie Chalmers.*) Which edition?—I have the first—I did not know that there was another. If there is a second edition it seems to me that it is a libel on me.

14038. We have "the fourth and revised edition"?—I have only the first one, published by Bell.

14039. (*Chairman.*) Will you refer to the passage?—I should like to be taken right through. First of all it is maintained that I inserted a pair of forceps under the skin behind the skull for a moment. That is absolutely untrue.

14040. You were the lecturer on that occasion, were you?—I was the lecturer at London University. I believe the date is wrong.

14041. That is a little material as to identifying you as the lecturer?—Yes, I raised that in connection with the question whether I could bring an action for libel. I believe the date is wrong by one day. I have not looked that point up in my pocket-book just now, and I could not swear to it.

14042. (*Dr. Wilson.*) It is immaterial?—Yes, it is immaterial here. The experiments were done by me, and Dr. Waller can support my statement. I did not insert a pair of forceps under the skin. I destroyed the cerebrum, and this is also shown later on towards the end of the chapter on page 14.

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14043. (*Chairman.*) It does not seem to me very material whether you put a pair of forceps under the skin behind the skull for a moment. You did destroy the cerebrum?—Yes; but the inference from the statement that I inserted one point of a pair of forceps under the skin behind the skull for a moment is, to any one who knows the subject, that I made a farce of destroying the cerebrum, which I did not. Then the next point is on page 13.

14044. (*Sir Mackenzie Chalmers.*) Will you read the words first to which you object?—This is a very minor point, but it is suggested there that I thought it would be better to cover over the frog after the destruction of its brain, with a cloth. I did nothing of the kind. It is often noticed in lectures that if the animal is not covered over, whether anything has been done to the animal or not, it distracts the attention of the audience, and for that reason it is constantly covered over. When an animal is brought into the theatre, it is absolutely imperative to cover up the animal before anything is done with it, or it distracts the attention of the audience in the same way as a butterfly in church will distract the attention of the congregation. It was not thought that it would "produce disagreeable reflections in some of the audience," as is stated. Then, again, on page 13, it is said that the rabbit was placed in a freezing machine. There was no freezing machine there at all. There was only an ice chest, and a piece of ice was put into that ice chest in order to lower the temperature of the ice chest.

14045. (*Chairman.*) Do you mean by an ice chest a chest with double walls for ice?—Yes. The temperature could never be down to zero, so that the rabbit was never exposed to the cold to which it is constantly exposed in winter. As a matter of fact, the temperature was four or five degrees above freezing point. The reason for that will appear later. Then it is further stated on page 13 that after fifty-five minutes the rabbit was taken out of this freezing machine, and found to be "beyond the stage for observation." Now the actual facts about it are these; and upon this I could bring Dr. Waller and Dr. Alcock, I believe they were both there. Dr. Waller I know was there. I showed before the audience that the animal's temperature had not gone down, that I had waited too long. I took the animal's temperature before the audience, and showed that the temperature was about 37 degrees Centigrade—that it had not gone down one degree.

14046. What is that Fahrenheit?—About 99 degrees—between 98 degrees and 99 degrees.

14047. What is the normal temperature of a rabbit?—About 99 degrees. Then these references must be taken together, because it is maintained on page 14 that the rabbit was "quite conscious but frozen stiff like a piece of wood." That is absolutely false, and it is shown on the face of it. An animal frozen stiff could not have a temperature of 37 degrees; it could not jump. It is absolutely absurd; it is entirely false. The animal's temperature taken before the audience was not one degree below the normal temperature, it could not therefore be even suffering from cold. The statement is absolutely false, and is shown to be false by the statement that the animal tried to get away.

14048. (*Sir Mackenzie Chalmers.*) If I understand you aright, what you meant when you said that the animal was beyond the stage of observation was that the temperature had fallen, and had very nearly gone back to normal?—Yes.

14049. That was all that you meant?—Yes, that was the only point of it, because the reason was to show the audience the importance of paying attention to the question of cold during an operation. It is well known to surgeons that unless the temperature of the air can be kept up during an operation, the patient's temperature goes down from the influence of the anæsthetic, and he is in a worse condition for recovery. I anæsthetised the rabbit. It was, therefore, not under the Act. I exposed it to a temperature of only two or three degrees above freezing point, and constantly in winter it is exposed to 10 degrees of frost under natural conditions, or even inside a laboratory it may be exposed to actual freezing point. I forgot, owing to the lecture, to take its temperature before the anæsthesia passed off. It had gone down slightly. I did not prolong the action of cold; it was beyond the stage of observation, because I did not observe it early enough, and, of course, the observation of the temperature showing that it was 37 degrees Centigrade, showed conclusively to the audience that the animal was not even suffering from cold, and that was the important thing. It is absolutely false to suggest that that animal was even suffering from cold, much less that it was frozen stiff as a board—stiff as a piece of wood.

14050. Miss Hageby states then that you again inserted an instrument into the skull cavity of the frog, moved it round several times, after which you said, "Now the brain is thoroughly destroyed," and she says, "What, then, about the first destroying"?—It is said on the first page that I only put a pair of forceps under the skin. She contradicts herself when she says that I put it into the skull in the first case. Those two points have to be taken together.

14051. She says that you informed them that you had destroyed the brain, and that the frog could not feel pain any longer?—Yes.

14052. (*Chairman.*) What had you done?—I had destroyed the cerebrum first; it was a decerebrate animal in the first case.

14053. And in the second case?—In the second case I did the complete pithing.

14054. You mean that you destroyed the rest of the brain?—Yes. I was perfectly fair at those experiments. If you read page 15, you will see evidence of my fairness, and what I complain of is that outspoken remarks are absolutely turned against me. I said I was not licensed for that laboratory, and therefore that I could not perform vivisection experiments. I said that I did not think the laboratory was licensed for vivisection experiments, and anything, therefore, that I did would be experiments which were not under the Act, and were painless experiments. And to freeze an animal, even partly, without anæsthetics, even to let its temperature go down 10 degrees, apart from any question of freezing it as stiff as a piece of wood, would be a cruel experiment. The suggestion, I maintain, is absolutely monstrous. I think it is contradicted by the statement: "The animal springs back." The only other point is about the same animal, only enforcing the same remark. The animal was not half paralysed by the cold, its eyes were not glazed in the least, and there were no signs of terror in the animal at all.

14055. Had anything been done to the rabbit at all, except putting it into this box?—Only giving it the anæsthetic.

14056. (*Sir Mackenzie Chalmers.*) Was the animal any the worse for it afterwards?—None the worse. Can I bring out a few other points that I have notes of in reference to some of the statements given in evidence before the Commission?

14057. Certainly?—One is in reference to sulphur dioxide experiments in relation to the destruction of rats on plague ships. The reference to that is in your last volume of evidence.

14058. (*Chairman.*) I have not had any notice of any other statements, except those with regard to the marmot. I did not, in fact, know that you were going to speak about these things?—When I was asked to come, I was not told at the time that I should be expected to give a *précis* of all the points I wished to bring before the Commission, but I thought, seeing that the Commissioners did touch upon the point of the sulphur dioxide experiments in connection with the destruction of rats on plague ships, I might state that I did some of those experiments within the last year or two in conjunction with some work done by Dr. Wade, who did the chemical part. It was work done for the Local Government Board. I want to draw the Commission's attention to the different position in which I was placed as a physiologist, compared with an ordinary man. A rat catcher on the Thames could use sulphur dioxide, and could be paid for using sulphur dioxide, and he could not be interfered with for the destruction of rats in that way. It was necessary for me, a physiologist, to get a special licence, which was limited to so many rats, in order that I might do experiments on rats, with the object of finding out for the Local Government Board what was the minimum dose of sulphur dioxide necessary—that is to say, I, as a physiologist, am in a worse position than a rat-catcher. These experiments are painful experiments. I returned them as painful experiments.

14059. How does the poison operate on a rat—at once?—No, it takes some time to produce fatal symptoms.

Further, one cannot do these experiments with anæsthetics. If one does them with anæsthetics one cannot arrive at the minimum dose of sulphur dioxide which is necessary to kill the rats on board ship, and therefore one cannot tell the length of time for which the ship should be shut down in order to kill all the rats. The sulphur dioxide acts upon the eyes in the first place, and I have not the least doubt causes pain. The eyes become quite opaque from the irritant action which the sulphur dioxide produces. As I said, I returned these as painful experiments. I could see that the animal felt the irritation by its trying to wash off the offending matter. The sulphur dioxide irritates its trachea and lungs, and causes violent bronchitis, and it is chiefly in that way that death is brought about. These are the painful experiments which I have done.

14060. You were not using this sulphur dioxide on board a ship for the purpose of killing all the rats on the ship?—No, I was using it in my laboratory.

14061. You were making an experiment with a view of ascertaining what was the best method of killing a rat?—And the proper dose.

14062. That was clearly an experiment under the Act?—Yes.

14063. Then you say that you are comparing your position with a rat catcher's position?—Yes; I say that I am worse off than a rat catcher.

14064. The one is killing the animals and the other is experimenting on them; that is the difference?—Yes, but a rat-catcher can kill them by the thousand and I am limited to experimenting on only five or six. I say that the limitations of the Act are against research.

14065. Under the Act there is nothing to prevent a man shooting his pack of hounds, all of them on the same day, but if he hands one of them over to you to experiment upon, that is an experiment?—Yes.

14066. Although you may be going to kill it under anæsthetics?—Yes; but the rat catcher is not killing the rat in the most painless manner possible; it is a painful death.

14067. I think we see your point?—I wanted to mention it in order to show the necessity of painful experiments. I think that painful experiments are necessary.

14068. That, of course, is the whole question. There is no question if they are not necessary?—I mean painful experiments, as against experiments under anæsthetics.

14069. (*Sir Mackenzie Chalmers.*) You maintain that they may be necessary in certain cases?—Yes. Can I give another example?

14070. Certainly?—I have worked on the question of bleeding—on the influence of bleeding and transfusion. These are experiments that I did in Germany, where it was not necessary for me to be licensed. I did them with Dr. Gürber.

14071. (*Chairman.*) With a view to what point are you bringing this forward?—To show the necessity of experiments without anæsthetics. I want to bring out the fact that if one studies the question of bleeding or transfusion, it is impossible to give an anæsthetic. If one gives an anæsthetic, one cannot study the effect of bleeding and transfusion. It is of the utmost importance that we should know what is the effect produced upon a man by the loss of a large quantity of blood. These experiments were directed to see what would be the difference in respiration, and the rabbits were not anæsthetised at all; they were simply tied down, and they did not show any violent struggling or signs of pain. I may say that my laboratory man has brought two rabbits—I did not know whether the Commissioners would like me to show the experiment, but I would like to show one point in connection with this, which I think is overlooked—that is, that if an animal is bound down on its back it often passes into a condition of hypnotism, which I can show you if you will allow me. I could take a perfectly intact rabbit, simply put it down on its back, keep a little pressure on it for a short time, then take away the pressure, and the animal will pass into a condition of hypnotism. I have seen in Germany, when I have done these experiments without anæsthetics, that these animals, when tied down, pass into a condition comparable to hypnotism, and it is known in the case of man that operations can be done under the influence of hypnotism without the patients feeling pain.

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14072. Are you suggesting that hypnotism should be used in cases of experiments and no anæsthetics?—In some cases. I want to point out that you could dispense with anæsthetics and hypnotise the animal in this way.

14073. Would there be any advantage in using hypnotism instead of anæsthetics?—Yes, because in the case of respiration the anæsthetic diminishes the respiratory exchange by one-half. Would you like me to do the experiment? There is not the least sign of pain.

(*Chairman.*) Personally, I do not desire it. There are three members of the medical profession present who understand those things better than I do. I would like to know what they think about it.

(*Sir William Collins.*) It is not new to me.

(*Dr. Wilson.*) No, we may accept the statement.

(*Dr. Gaskell.*) Certainly.

(*Witness.*) Animals do not appear to feel pain then even without an anæsthetic, and I can give evidence of that from some of the experiments that I did in Germany.

14074. (*Chairman.*) That was your experience?—That was my experience.

14075. Using hypnotism for the purpose?—Yes. Directly after the bleeding and transfusion the animal was able to run about. It was kept warm for about two hours, because there was a certain amount of shock.

14076. Did it lie still while you operated?—Yes, the animal was tied down as well to keep it in position.

14077. Did it struggle?—It only struggled when it became unconscious from asphyxia. That is another point to which I want to draw attention—that all the evidence in man, and I could do the experiment upon a man to prove it, shows that in the first stage of asphyxia, before struggling begins, there is loss of consciousness. If students experiment upon themselves by breathing in and out of a bag so that there is a lack of oxygen, they will often become quite unconscious, and tumble down before any struggling movement takes place, and one can actually anæsthetise a man simply by producing this stage of asphyxia—from the lack of oxygen. In Java anæsthesia, for example, if one presses upon the two carotids (*describing the action*) to prevent oxygen going to the brain, the man becomes unconscious. Asphyxia is not painful; it is preceded by a condition of anæsthesia from lack of oxygen. A dentist can produce a like effect simply by making a man breathe in and out of a bag in which the oxygen is gradually diminished. Although asphyxia is constantly mentioned as a painful condition, I can demonstrate upon any objector that it is not painful.

14078. What is the precise meaning of asphyxia? We all use the term. What do you say it is precisely?—The precise meaning of asphyxia, I think, is that it is the condition brought about by the lack of oxygen in the blood, and the accumulation of carbon dioxide.

14079. That is the scientific explanation, but what is the condition that is brought about?—The condition of unconsciousness and convulsions; that is to say, in popular language, unconsciousness and convulsive movements.

14080. (*Dr. Wilson.*) Garrotting, then, you would call not a painful experiment?—Not if it be done properly. You can make the experiment upon yourself, if you press upon your two carotids; there is no danger, because directly one becomes unconscious the pressure goes. If you press upon your two carotids so as to produce asphyxia of the brain, you become unconscious; then your arms are paralysed, and you save yourself by relaxing the pressure.

14081. (*Mr. Ram.*) Do you cause the brain to be gorged with blood?—No, the brain becomes in a condition of asphyxia from the combination which I gave just now—from lack of oxygen and accumulation of carbon-dioxide.

14082. (*Dr. Wilson.*) Like charcoal poisoning?—As regards lack of oxygen, yes, but there may not be an accumulation of carbon-dioxide then.

14083. (*Dr. Gaskell.*) Does not the Japanese ju-jitsu, as they call it, put a man to sleep?—I do not know. There is only one other point that I desire to draw

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attention to, and that is this: I think one ought to recognise that pain is not the supreme evil, as one of your witnesses maintained. Pain, I maintain, from the physiological point of view, is a protective mechanism, and the modern idea of trying to abolish all pain is absolutely absurd. It is simply, from this point of view, one of the conditions of every-day life. If we take the distribution of pain we see that pain often protects an animal from danger. There is a fractured limb. The pain brings about uncomfortable sensations, so that that limb is kept in a condition of rest, which is the most satisfactory condition for repair; or there is an injury to the abdominal cavity, when pain brings about reflex alterations of the abdominal viscera, so that the viscera do not move, and therefore puts the patient in the best condition for recovery. In the case of some abdominal wounds it was notorious in the South African War that the men who were left out all night, and left to themselves to suffer this pain, and given no anæsthetics, were the men who recovered afterwards with or without operations, while the men who were treated at once and given brandy or something to relieve the suffering, were the men who died. Pain, then, is a protective mechanism, and I maintain that we are becoming so over civilised that we run a danger in our attempts to abolish pain of doing an enormous amount of harm. Take, for example, the extent to which anæsthetics are now used in cases of midwifery, in ordinary cases of delivery.

14084. (*Chairman.*) Is this an argument to show that there is no occasion for anæsthetics?—This is an argument to show that a common sense view should be taken of this question, and that pain must be admitted. I admit that I have done painful experiments, and I am not ashamed of admitting it. They are absolutely necessary. I want to show that pain is part of the scheme of nature, and that we must recognise its existence.

14085. We know that pain and pleasure and everything else are part of the scheme of nature, but I do not know that we can have the Act amended with a view to acting on that principle. When a dentist pulls a large double tooth out, he causes a great deal of pain, but I do not know that I should call it protective. It does not protect me in any way. So that if you perform an experiment deliberately on an animal and leave out anæsthetics altogether, you perform an experiment on an animal which is a painful operation?—I say that in some cases pain is absolutely protective. I will give you an example.

14086. Supposing you proceed to perform an operation on an animal, it may be protective in one sense; it may be better for the animal—I do not know, I am sure—that it should suffer some pain if it has to have an operation. But you are not protecting it in inflicting pain upon it, you are inflicting pain upon it simply?—But the pain that it actually feels is protective.

14087. I do not think we need go into such fine points as that. Surely it is a very simple proposition that if you take a knife to an animal and cut it open you inflict pain upon it?—Yes.

14088. And if you inflict pain upon it the Act says that you must use anæsthetics?—Yes.

14089. How does your theory affect that? If everything that you say about pain being protective is true, what has it got to do with the case?—In this way. I think the Act is not really what one would expect from the point of view of common sense. If an animal is cut and there is a reflex painful effect produced which would act upon its heart, under ordinary conditions—for example, the animal has been injured by a dog and torn—some of these very mechanisms which we may shut out by an overdose of anæsthetics would be protective to the animal, if the animal was to be kept alive after the operation. Therefore I maintain that it would be wiser to allow these operations often to be done without anæsthetics.

14090. The Act only applies to experiments; it does not apply to a veterinary surgeon using the knife for the purpose of saving an animal's life. This is a question only of the infliction of pain?—I think we ought to be given a licence to cover all experiments. I think that the Act is entirely antagonistic to the advancement of physiology. If we were given a licence for all experiments there would be no more cruelty. There would be a great saving of time and no limitation of work, and there would actually be in the long run a saving of life.

14091. What do you mean by a licence for all experiments?—I mean without any conditions.

14092. With or without anæsthetics?—Yes, without any limitation at all, and without certificates.

14093. That is to say, you are to put yourselves in the condition in this country which I understand physiologists are in in some parts of Germany, where there is no limit?—I think there should be no limit; that is to say, that a recognised physiologist should be given a licence to cover all experiments.

14094. Without anæsthetics?—Without anæsthetics or with anæsthetics and without certificates.

14095. And without limitations as to length?—Without reference to any conditions at all, but that he should, of course, be subject to inspection. I would point out that the difficulty of the present situation is this: I live in the country, and I could actually reckon myself as a farmer, seeing that I have more than three acres, and I do have to give a return to the Board of Agriculture. As a farmer, if I had lambs I could castrate those lambs without any anæsthetics; it would be absurd to give them anæsthetics. I have seen castration done over and over again. At Guy's I cannot castrate one lamb without a special licence, and if I tried to get a licence to castrate a lamb without giving anæsthetics it would probably be refused. My condition, then, is this: that as a private individual, if I liked to tax my conscience, I could make experiments in the country, and I could publish those experiments as a private individual, not as experiments, but simply as observations, which I could not do in my own physiological laboratory.

14096. I think you would find it rather difficult to do it if you published it, without bringing a great deal of odium upon yourself?—If I did it simply from a commercial point of view, I believe that legally I could not be touched. And then, of course, as regards pain, one causes very much more pain in the country, even unconsciously, than one does in a physiological laboratory. The worst pain that I ever caused I am convinced—I could bring evidence in support of it, but I am quite convinced—was in a case where I took away a calf from a cow. The cow was exceedingly fond of the calf; the calf was too big, and I wanted the milk for my children, so I took away the calf and sold it. For two days that cow was in obvious pain; it would not take its food; it went about bellowing, roaring, and moaning, and was so bad that the cottagers complained. There is a case in which it was perfectly obvious to me that the animal was suffering pain for two days. Never in any physiological experiment have I caused so much pain, even under the Act, as one does every day in the country.

14097. The Act deals with physical pain?—I cannot see the difference between physical and mental pain from a physiological point of view.

14098. There are a great many cases which come on the border line in everything, but I think there are a vast number of cases in which you would say that the pain is mental pain, and not physical?—I do not think that physiology can really separate the two.

14099. I am not using any scientific language. I am using the language of every day conversation. In a certain sense, of course, you may trace mental pain to certain physical causes?—I quite see what you mean; it is that mental pain is the worse form of pain.

14100. There is a great difference between what we call a broken heart and a broken leg?—Yes.

14101. You seem to think they are the same thing?—No, I do not admit that, if it is taken down in evidence.

14102. (*Sir William Collins.*) Is the mental pain protective, too?—I believe that it is protective, because it is followed by reaction.

14103. (*Mr. Ram.*) Would the pain caused by an operation for purposes of experiment be protective; is the cutting of an animal's skin protective to the animal?—If you take the condition in which you could see the animal's reacting mechanism it would be protective. What I want to show is that the protective mechanism must be there for meeting the contingencies which will probably arise.

14104. (*Chairman.*) What I put to you is this: If you inflict a pain upon an animal, and you want to do an experiment upon it, you do not do it to protect the animal against anything, the pain that you cause is incidental to your making the experiment?—Taking into account all the conditions which you mention, the

reactions are not actually protective, but in ordinary conditions, if an animal were to get such a cut as that, and it were kept alive, the reflex reactions would then be protective.

14105. But the pain that you would inflict would protect no animal?—In that individual case it would not be protective unless the pain was enough to produce syncope.

14106. It would be protective against something that it would not suffer unless you caused it?—But I say that even if I caused it, it might produce syncope, so that the animal would not suffer pain. There is only one other point which I think ought to be recognised. One of the reasons why vivisection is objected to is entirely from the theological point of view, and that comes out in the first chapter in this book, "The Shambles of Science." It is believed that physiologists are rank materialists. I for one absolutely deny it. The chief arguments in favour of vitalism, the modern arguments in favour of neo-vitalism, have come from physiology. The opposition from the clergy to vivisection largely comes from the fact that they believe that all physiologists are materialists. There has always been, so far as I know, a division in physiological thought—there have always been two schools of physiology, the materialistic school and the vitalistic school. The vitalistic school is increasing in force, and it is quite unfair for the writers of "The Shambles of Science" to maintain that physiologists are materialists. My experience as a teacher is that the students often come from the study of physics and chemistry, from their ordinary studies, and from their reading of the ordinary literature of the day, to start physiology with a materialistic bias, and, judging from their papers in our students' society, the study of physiology is the best corrective of these materialistic views. I should say that many of them change their views from the materialistic side to the vitalistic side owing to the study of physiology. I insist again that the best support of the vitalistic position at the present day comes from physiology.

14107. (*Dr. Wilson.*) The vitalistic side is the agnostic side rather?—A vitalist might be an agnostic. It depends upon the sense in which you use the term agnostic, because the vitalist has a definite position. A vitalist of course believes that there are conditions which cannot be explained by the ordinary terms of physics and chemistry, and is not agnostic therefore in that sense.

14108. (*Sir William Collins.*) How long have you held a vivisection licence?—Fifteen years. I was demonstrator first at Oxford in 1892.

14109. What certificates, if any, do you hold?—I have got a good many. I do not remember what they are now. I could look it up.

14110. Do you hold a certificate to permit you to dispense with anæsthetics?—I do not know exactly what certificates I have, because I have not been doing any vivisection experiments this year.

14111. You are, I think, joint lecturer on physiology?—No, I am entire lecturer; there is no joint lecturer.

14112. I took it from the title page of the book you handed in?—My late colleague is dead, and I am now full lecturer.

14113. As lecturer on physiology are you one of the persons authorised to sign certificates?—No, I cannot do so.

14114. Have you anything to do with supporting applications for licences?—No, I have not.

14115. I understood you to say in the course of your evidence that the Vivisection Act was entirely opposed to the advancement of physiology?—Yes.

14116. Is that opinion held by others engaged in physiological research besides yourself?—Yes, I think so. I have already given evidence as to experiments which I could not do in this country which I could do in Germany. The medical members of the Commission will recognise the importance of understanding the effects of bleeding and transfusion.

14117. I think I gathered your point with regard to the experiments in Germany?—It was that we were able to show that after the loss of half the quantity of the red blood corpuscles we could revive an animal and render it perfectly fit.

14118. Without anæsthetics?—Yes.

14119. Did I rightly gather that in your opinion anæsthetics are unnecessary?—Yes, I am convinced that they are often unnecessary, not only in the case of animals, but of men.

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14120. For vivisection and for surgical operations?—For vivisection and for surgical operations; and I mentioned the case of midwifery.

14121. Would it be going too far if I were to express your opinion as being that pain is a good thing in itself?—I say that pain is a beneficent and protective mechanism. I have written that in an article which I have recently published, and I am prepared to defend it. It is not a new view at all; it is recognised by surgeons, I believe, that a certain amount of pain is a protective mechanism.

14122. There was a good deal of argument of that kind, was there not, at the time of the introduction of anæsthetics?—Yes, a large amount. When we came to the question of midwifery and the use of anæsthetics, there is not the least doubt that unborn children are injured by the dose of chloroform that is taken in. It may not be a large enough dose to poison the unborn child, but it will interfere with the circulation through the mother's uterus and thereby injure the prospects of the child. The amount of pain that a woman feels in childbirth is protective because the pain will actually inhibit a movement; the pain in parturition will actually produce a condition in the mother which will prevent the too rapid or too slow passage of the child.

14123. I want to be quite clear that I took down your words correctly. I think you said that to abolish pain is absolutely absurd?—To abolish all pain in the condition in which we are at present would be absolutely absurd. The condition would be this: that if we met with an accident we should straight away go from bad to worse without any natural protective mechanism.

14124. Just one word about the marmot. I see that it was implied in one or two of the questions put to Miss Hageby that a post-mortem examination had been made of this marmot. Was that so?—That is Professor Thane's evidence. It depends upon your interpretation of a post-mortem examination. Post mortem means after death.

14125. Did Dr. Thane make any further *post-mortem* examination than that which honourable members have made to-day?—Yes, it was further, for the reason that the animal was much fresher then; it had not long been preserved.

14126. Was there any *sectio cadaveris*?—No, but it is absolutely correct to say that there was a *post-mortem* examination. There was a *post-mortem* examination in the strict sense—that is to say, the body was examined after death and under much more favourable conditions, because it was fresh, but there was no cut made.

14127. You mean that there was a *post-mortem* examination in the same way that we have made a *post-mortem* examination to-day?—Yes, but I do not think that was a thorough *post-mortem* examination to-day. I wanted you to let me cut through the skin and demonstrate to you and make you admit that its spinal cord has not been cut.

14128. I understood you to say that you drew a distinction between a brainless frog and a pithed frog?—Yes.

14129. You say that the law makes it necessary that the cerebral hemispheres should be destroyed?—Yes.

14130. I do not remember it in the Act?—Because the experiment must not be one calculated to cause pain. All the evidence shows that the pain is connected with the cerebrum. I can give examples of that.

14131. I only wanted to know whether there is any definition of pithing laid down in the Act?—So far as I remember I do not know of any.

14132. Would there be a difference in the physiological manifestations visible in the case of a frog which had only been deprived of the cerebral hemispheres compared with one which had only the spinal cord left intact?—Yes.

14133. Should I be right in thinking that in the former case purposive movements would be present as they would not in the latter, to the same amount?—No, not of the same order; of a different order; but they would be present in both cases.

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14134. Should I be right in thinking that the higher order of purposive manifestations would be present in the case where the cerebral hemispheres alone have been destroyed?—Yes.

14135. And you compared the condition of a frog to that of a brainless monster?—Yes.

14136. Do you think that purposive movements even of a higher character do not imply possession of consciousness?—I say not so far as one can judge. Shall I give you the reason? We can only judge of consciousness from ourselves philosophically—that is the first point. Then we must go to other people. If you take the case of a man with a section of the spinal cord (I have made the observation several times) that man with a fractured spinal cord will perform purposive movements—he will draw up the leg if tickled; but if he is asked whether he feels the tickling he says, No. He knows nothing about it unless he sees the movements.

14137. Do you not draw a distinction between reflex actions and purposive movements?—These movements are all purposive?—Further, I have seen the case of a woman, who was pregnant at the time, with a fractured spinal cord in the dorsal region. She was brought into the hospital in a condition of paralysis. The physicians and surgeons in charge of the case were doubtful whether abortion should be produced in the interests of the woman. They consulted Dr. Mott and myself from the physiological point of view, and we recalled at once the physiological experiments done by Goltz in Germany, which showed that pregnant animals with a section of the spinal cord could go quite safely to full term. This woman went to full term, and—this is what I want especially to draw attention to on the question of consciousness—all the complicated movements of pregnancy, all the movements in connection with the alteration of respiration, straining movements, and contraction of the uterus went on, and the woman was absolutely unconscious of the fact that she was in process of delivery; although she knew the condition from previous experience, being the mother of several children, she felt no pain at all. That child was born, and until the mother was shown the child and told she had been in labour she did not know that it had taken place. I say then that the common-sense view to take of the matter is this—we cannot be absolutely certain about the question of consciousness except in our own case. In the next case we must take what is told us by other people as regards consciousness. They may or may not tell us the truth. We can only argue from our own consciousness and from what other people who have a fracture of the spinal cord tell us about their condition of consciousness, whether an animal with its spinal cord only has any consciousness. That is a perfectly fair, straightforward statement. That is the only way. I say that I do not deny that there may be consciousness; I believe myself, for example, that philosophically one should believe in the germ of consciousness in the spermatozoon and in the ovum, and there is no reason to suppose that consciousness comes in at any special time during pregnancy. All life, I maintain, must possess the germ of consciousness. I am quite willing to be perfectly straight on that point, but judging by people with a section of the spinal cord—with a fracture of the spinal cord—they do not feel in the paralysed part, and yet they perform the most complicated purposive movements, such as contraction of the bladder, contraction of the uterus, contraction of the rectum, purposive obviously to get rid of the urine from the bladder, to get rid of the full time child from the uterus, and to get rid of fæces from the rectum.

14138. But all involuntary muscular fibre?—No, certainly not. If you tickle the feet, for instance, they are drawn up.

14139. Those that you referred to are involuntary muscular fibre?—I gave you the most complicated because I thought they were the more important. If you tickle one foot it is drawn up. And those that I referred to are not entirely involuntary muscular fibre, because in the case of parturition there is a large amount of accessory pressure brought about by contraction of the abdominal muscles.

14140. I was referring to your quotation of the uterus, the bladder, and the bowel. Those would be involuntary muscular fibre?—Yes.

14141. Can a brainless frog swim?—With the cerebrum alone removed.

14142. Can a man with the spinal cord divided high up?—The only case that I can draw attention to is this, and the case bears on the question of brainless animals. During the construction of the Forth Bridge a workman fell down, and, falling into the Forth, he struck against a great many planks of timber. There was a boat kept there, and the man was picked up and conveyed to the Edinburgh Royal Infirmary. The man was still breathing at the time, and breathed for some time after, and he showed reflex movements. When a *post-mortem* was made after the death of that man it was found that the entire cerebrum was completely destroyed; the skin had simply kept together the fractured skull; the brain was destroyed, but the lower parts, such as the medulla, were intact, and so he had been able to breathe and perform these reflex actions in the same way that a brainless monster will suckle and cry and can do purposive movements and can breathe perfectly well. That man showed no sign of consciousness.

14143. Can a frog with the spinal cord only left swim?—I have not seen one swim.

14144. (*Mr. Ram.*) With regard to pain I want to make your position clear. I understand you to say that pain is very often protective?—Yes, I believe that pain is very often protective.

14145. That means this, does it not—that when a man or an animal suffers a lesion, the pain produced by that lesion is protective?—Yes.

14146. But if there is no lesion, if you take the case of an animal that is perfectly sound and well, do you justify the infliction of unnecessary pain upon that animal?—You want me to assent to the "unnecessary"?

14147. I want you to answer my question?—I consider that it is perfectly right to inflict pain upon animals.

14148. Unnecessarily?—Not unnecessarily—no.

14149. Do you think it is right to inflict pain upon a healthy animal—I mean when there is no question of protecting mechanism—when you can discover all that you want to discover if that animal is under anæsthetics?—What I should say to that is exactly this. I will be perfectly straightforward. I say that you should not inflict pain if you can obtain the knowledge in any other way; but I say that even where there is an operation, the pain there is of a protective nature; it may produce syncope, and therefore less sensation of pain. Further, I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of hypnotism, which I offered to show to the Commissioners. That I think is one very important point. These animals pass into a condition, so far as one can see, comparable to hypnotism. If you give them anæsthetics you are introducing a complication which you could remove, and therefore without anæsthetics you actually save life and actually diminish the infliction of pain.

14150. (*Dr. Gaskell.*) Do I rightly gather that you would advocate the abolition of the Vivisection Act altogether?—No, I would not do that. I think only people interested and people who are competent should be allowed to make vivisection experiments—that is to say, there should be a certain limitation that these people have the requisite knowledge, and that no ordinary man without the requisite knowledge should be allowed to cut up an animal or to carry on such experimental work. I think that a licence should be given which covers all experiments, those with and those without anæsthetics, painful or otherwise.

14151. Do I rightly understand you to mean that you would diminish the number of people at present holding licences?—No, I would simply leave it for the Home Office to decide by means of special advisers—such, for example, as the Association for the Advancement of Medicine by Research—whether certain applicants for a full licence are competent people for carrying out experiments in physiology or pathology. There would be no limitation of numbers. The limitation would depend only upon the capacity to carry out the work.

14152. You would not consider that some of the points which you have put before us might be solved by introducing the term hypnotism as one of the anæsthetics under the Act?—Yes, I think that ought to be done. Further, I think that hibernation should also be introduced, because it is known, for instance, that

a hibernating mammal passes into a condition of anæsthesia by natural causes, and I think that all natural conditions of anæsthesia should be introduced in the Act. I am told—I am only speaking from memory—that in Denmark it is lawful to vivisect an animal during the winter when it is in a condition of hibernation, but it is not lawful to vivisect it in summer when it is not in a condition of hibernation. Hibernation is a natural condition of anæsthesia, and we ought to be allowed to avail ourselves of that natural condition of anæsthesia.

14153. (*Chairman.*) If you could satisfy the Home Office that an operation on a hibernating animal did not inflict pain, would you require a licence?—It is left to me to decide what operation is calculated to cause pain, but the difficulty that I find is that I may consider that it does not cause pain, and then the Inspector, who, with all respect, does not know anything about hibernating animals, may say that he does not believe that to be the case.

14154. Exactly. If the Home Office were satisfied that it would not inflict pain upon the animal, you would not require a licence?—No; but the Home Office throws the obligation upon me, and it is very difficult to simply convince them.

14155. Very?—But I am willing to show the Commissioners during this winter hibernating animals and to show them that they do not feel pain from the cut of a knife.

14156. (*Mr. Tomkinson.*) Did I rightly understand you to contend that pain of itself is not only protective but a blessing, and that all anæsthetics are a mistake?—No, I do not go so far as that. I say that pain in general, if one takes a wide view of biological processes, is a protective mechanism, and it is in that sense beneficent.

14157. But does not that extend only to this, that if one puts one's hands by mistake into the fire or into boiling water, thereby violating a law of nature, one is punished for it, and pain is naturally beneficent and protective in order to teach us how to obey the laws of nature and not to subject ourselves to danger in future? Surely it goes no further than that?—Yes, I think it goes further than that. How otherwise do you explain the pain that we feel with a fractured leg and the pain goes on and the pain is still protective after the fracture?

14158. I confess that I cannot see the analogy between that and the infliction of gratuitous pain upon an animal?—I gave you an example of the infliction of gratuitous pain upon an animal without anæsthetics which produces a condition of shock, so that there is less circulation of the blood and therefore the animal's brain is not so active and it does not feel pain so much, and therefore to that animal upon which you inflict pain, that pain is actually protective in the example which I have taken.

14159. Now I want to ask you a personal question?—I am quite willing.

14160. If you had to undergo a very severe operation yourself, say the amputation of the thigh or some very severe skin lesion, do I rightly understand that you would think anæsthetics a mistake?—I should consider the question of what would be the effect of the operation and the effect of the anæsthetic. For a small operation I should have no anæsthetic. I have had my lip sewn up without anæsthetics, and the pain is much less than the trouble caused by the anæsthetic. I have seen a man come into the theatre and sit down in the chair and without moving a muscle have a cancer cut out of the lip and have the lip sewn up and march out. If you give an anæsthetic you forget that one of the effects of an anæsthetic is pain. If you go round the wards after anæsthetics have been given, you see patients vomiting from the effect of ether. Is not vomiting painful? I am certain that in many cases in dentistry also, people suffer considerable pain afterwards from the anæsthetic used. In the case of an animal or man if the pain to be inflicted is of a trival kind an anæsthetic is a greater discomfort than the slight infliction of pain. That is my candid view.

14161. Surely you do not contend that the horrors of the cockpit on board a vessel in action and of field hospitals have not been marvellously mitigated by the blessed discovery of chloroform and other anæsthetics?—But these things also bring disadvantages. There is no doubt that the excessive use of anæsthetics is a modern danger. In proof of that is the death roll from anæsthetics. I have seen cases of people dying—of children dying from an anæsthetic given for simple operations like circumcision. I have circumcised children without anæsthetics. If they cry it does not matter; it is much better that a child should cry and moan than that its life should be lost. That circumcision is not such a serious operation is shown by the fact that the ordinary medical man does circumcision under anæsthetics and the Jewish non-medical man does it without anæsthetics. There is an exact case in point in which the introduction of anæsthetics has done a considerable amount of harm. Take also the case that I gave you of midwifery. The pain that a woman suffers in childbirth is protective. Extend the analogy of the use of anæsthesia and every woman in childbirth ought to be given an anæsthetic. What would be the result? You would make the nation more degenerate, if possible, than it is at present. And the very idea that women require anæsthetics during childbirth, I am convinced, has done a good deal towards bringing about the decrease in the birth rate—that is to say, women will rather take preventive measures or produce abortion, will rather be immoral, than suffer a certain amount of pain. It is really owing to the nonsense talked about destroying the bad effects of a small amount of pain. A small amount of pain is part of the scheme of nature, and should be so recognised.

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14162. One question more on another point. You mentioned having performed and seen performed the operation of castration. A good deal has been made of that from certain quarters before this Commission. You have seen it performed on sheep?—In hundreds of cases or more.

14163. And on pigs?—Yes.

14164. It is very momentary?—No; it is a short operation if it is done efficiently, but the man is an ordinary farmer in most cases. I will tell you the way it is done if you like.

14165. The effect upon the animal is apparently very slight?—I will not admit that. The lamb is taken by its four legs.

14166. I do not want a description of it?—But I cannot agree to your view that it is momentary pain. It is not. The lamb is taken by the four legs by one man. Then the farmer—this is exactly the way I have seen it done—seizes the scrotum, slits it with a penknife on one side, seizes the testicle, which is a sensitive part, with his teeth, which is quite a good method, pulls on it, and cuts it off with the knife. The operation is then repeated on the other side, the other testicle is removed and the lamb is put down. You see at once the condition of depression. The lamb, which was jumping and frisking about before the operation, stands with its tail between its legs in a marked condition of depression. That depression—I say nothing against it—is protective; it will keep that lamb quiet and that lamb's scrotum will quickly heal up, I grant you. But there is the depression; it is not only for a moment that it feels pain.

14167. But in the case of bulls, certainly, and of horses, it is a necessary operation, we know, for domestic purposes?—Why?

14168. Because they could not be kept?—Why? I can understand that it is advisable, but I do not see why it is absolutely necessary.

14169. Entire horses could not be used for draught purposes in towns and other places; they would not be amenable to discipline.

14170. (*Dr. Wilson.*) I think you said that you had not performed any vivisection—I mean cutting experiments—this year?—Not so far as I remember.*

14171. But you are a lecturer on physiology?—Yes; but I have done the experiments that I have mentioned with carbon monoxide.

14172. What I want to get at is this, that you did not consider it necessary this year, at all events, to give experimental illustrations of your lectures?—Not experiments under the Act—that is so. So far as I know I have done no experiments under the Act.*

14173. You have done no experiments on dogs?—I can state on that point that I do no experiments on dogs at all.

* *The witness subsequently wrote that after reference to his lecture note-book he found that he had performed three experiments.*

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14174. Yet Guy's Hospital is a large school of medicine?—Yes, I suppose I have about 70 or 80 fresh men every year, and of course those men do physiology for two years, so I may have practical classes of 75 men.

14175. You do not consider it necessary to use a dog for experimental illustrative purposes?—I have not used a dog at Guy's for illustration of experiments at all. I use rabbits, rats, and guinea-pigs. I am very fond of dogs, and if I want to vivisect, and can get a rabbit, rat, or a guinea-pig I do it in preference to vivisecting a dog.

14176. But even those experiments you dispense with. You can teach physiology thoroughly without?—I can teach physiology without experiments on dogs; but there are certain experiments of course which can only be done upon dogs.

14177. But you do not consider them necessary for teaching physiological courses?—In my case I do not use dogs for illustration of lectures.

14178. Then with regard to feeding experiments, do not you think that more knowledge would be gained by experimenting on human beings?—Yes, experiments have been done on fasting men. But the point that I want to bring out is that the reason why one takes hibernating animals is that in their case the deposition of fat is brought up to a point which is found in no other animal.

14179. But men do not hibernate?—No, but the deposition of fat takes place when the animals are not hibernating, so that from the point of view of the condition in men it is really useful to do this. The only other way of experimenting, which has been done, is to use fattened geese.

14180. I threw out that hint, because I was reading the other day that Professor Wiley has instituted or started what his students call a poison squad; that is to say, a certain number of students volunteer for feeding experiments and experiments with drugs?—I think that to safeguard the students those experiments should be preceded by experiments upon animals. Some of the drugs might produce poisonous effects upon the students. I quite agree that those feeding experiments should be done upon man, but in this case the animal may almost double its weight. The process therefore is accentuated, and one can study it better on the marmot, and of course during the deposition of fat the animal is not actually hibernating.

14181. You believe that even severe experiments could be carried out on hibernating animals during hibernation without causing pain?—Yes, when it was in the protective condition of torpor. It has actually been done.

14182. Can you not wake up an animal out of the hibernating sleep?—Yes, if the experiment or the actual performance of the operation took too long, the animal might wake, but the process of waking is a fairly long process, and the experiment should be timed to be over before the animal is fully awake.

14183. But how could you tell?—By previous control experiments. I could give you figures as to the time it takes.

THIRTY-SECOND DAY.

Tuesday, 5th November 1907.

MEMBERS PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. G. WILSON, LL.D., M.D.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Colonel DAVID BRUCE, C.B., F.R.S., R.A.M.C., called in; and Examined.

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14184. (*Chairman.*) You are a Colonel in the Royal Army Medical Corps?—Yes.

14185. And you are a Fellow of the Royal Society?—Yes.

14186. A member of the Advisory Board, Army Medical Service?—Yes.

14187. And you have acted on the Sleeping Sickness Commission of the Royal Society in 1903?—Yes.

14188. And you were Chairman of the Mediterranean Fever Committee of the Royal Society in 1904-6?—Yes.

14189. For a good many years past, I believe, you have been accustomed to perform experiments on animals in the study of disease?—Yes.

14190. Would you tell us, generally, the occasions and places at which you have been so employed?—After I came into the service, I was stationed at Malta for five years, from 1884 to 1889, during which time I worked at several medical subjects, among them, the principal one, from a medical point of view, being Malta fever. Then in 1889 I went to Netley Army Medical School and remained there five years as an assistant professor of pathology.

14191. And did that involve the study of bacteriology?—Yes; I was bacteriologist there.

14192. And animal experiments?—And animal experiments. Then in 1894 I went to South Africa, and soon afterwards went up to Zululand, where I remained for some two years, working at a disease called nagana, or the tsetse fly disease, which was causing a great deal of destruction of cattle up in Zululand, and giving the Government a good deal of anxiety.

14193. It was not at that time ascertained to be identical with the tsetse fly disease, I think?—No. Then in 1897 I returned to Natal and shortly afterwards began working at a very serious disease of animals called South African horse sickness, and after that the war began and I was put on a commission to study dysentery and enteric fever in soldiers, especially in the field, and there, of course, there was a slight amount of animal experimentation, but very little, as we were up in Pretoria, surrounded by the enemy, without any books and without any animals; the only animal we could get to use would have been the horse, and that is too large, of course, for ordinary subjects.

14194. Then you were not engaged in experiments on animals during the time you were in the field?—No, on account of our being in a beleaguered town without any opportunity. But, in my opinion, you could not work at dysentery or enteric fever with any chance of success without animal experimentation. It was only want of animals that prevented our working in that way at that time. Then I came to England in 1902, and in 1903 I went to Uganda to study sleeping sickness. I remained there some seven months, and on coming back to England in 1904 I became chairman of the Mediterranean Fever Com-

mission for the study of Malta fever. At the present time I have a licence to perform experiments on animals at the Royal Army Medical College, but at present I have not much opportunity of doing research work on account of other duties.

14195. You have had very large experience, at any rate, spread over a good many years in animal experiments?—Yes.

14196. And you have had an opportunity of forming an opinion as to the value of them in advancing medical knowledge?—I have.

14197. What is your opinion, generally, on the subject?—My opinion is that it is the most valuable method we have of advancing medical knowledge, and without it we could hardly advance a single step safely. These questions are so complicated, and one is so apt to fall into error, that unless each step is checked by animal experimentation one soon goes wrong.

14198. And have you observed during your twenty years' experience advances made in knowledge by means of experiments on animals?—Yes, certainly.

14199. And in the method of dealing with disease as well as the cause of it. I mean with regard to the serum treatment more especially?—Yes, I think I can say that.

14200. Have your experiments been more directed to the cause of disease?—Yes, my work has been nearly always on the causes of disease with a view to their prevention on broad lines rather than on drug or serum lines.

14201. You are going to tell us, I understand, what your experience is with reference to the causes of disease in some special cases of disease that you have dealt with?—If you please. I have worked at many different things, of course, but I have especially worked at three diseases, namely Malta fever, nagana, and sleeping sickness. I think in Malta fever you have a good example of the result of experimental work in elucidating the natural history of the disease and finding out how the infection is carried from one animal to another. The result of this work has been very striking; that is to say, we have had in the garrison at Malta an average of 700 men down with this fever, for, on an average, 120 days each up to last year. This year to the end of August there has not been a single case in the Navy, and only six cases in the Army, a reduction from 700 down to six; and during the two worst months of this year, July and August, there has not been a single case in the Army or in the Navy in Malta. So that there is yearly saved to the State the services of almost a whole regiment of soldiers, and 700 men from remaining in hospital 120 days with a painful disease, a disease which very often shatters the health for years and sometimes for a lifetime.

14202. Will you describe what the nature of the disease is as regards the pain and the suffering of it, and the length of time it lasts disabling a man?—It is a disease which is characterised by its extremely long duration; that is to say, the men remain, on an average, some 91 days in hospital in Malta, and as most of them are invalided to England and spend another two months or so in hospital here, you can put down the average duration of this disease at the extraordinarily long time of four months, or 120 days. Of course, it lasts much longer in some cases.

14203. You mean before they can be discharged from hospital?—Yes, before they can be discharged from hospital. Many are invalided from the service altogether, and a certain percentage of them die. Some cases of this fever last as long as three years, rendering the man absolutely incapable of work of any kind during this time; and two years is by no means an uncommon duration. In fact, it is a disease of very long duration; it is characterised by extreme weakness; that is to say, a man is brought down to the very edge of the grave without absolutely dying; he is extremely emaciated; he suffers from severe pains in the joints and nerves, and altogether we look upon it as a more severe disease than even enteric fever. Although the mortality in the case of Malta fever is not nearly so large as in enteric, on account, probably, of the different anatomical regions that the two diseases attack, enteric fever attacking the intestine and ulcerating it, very often giving rise to death by perforation of the intestine, whereas in Malta fever there is no ulceration of the intestine, and therefore that is one reason, no doubt, why the mortality is less.

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14204. Is it widespread as well as serious?—Yes.

14205. You speak of it as Malta fever. Is it confined to Malta?—No, it is found all over the world. It was first studied in Malta, hence the name. The Americans found it in the Philippines lately, and it has also been found to be very widely spread in South Africa, in the Orange River Colony, and in Kimberley and other parts of Cape Colony. It is also found in Egypt, and generally round the shores of the Mediterranean, and it has also been found in Arabia, India, China, the Fiji Islands, North and South America, and the West Indies, so that you may say it is a world widespread disease in the sub-tropical and tropical regions of the earth.

14206. And what is the particular micrococcus that you find causes it?—The parasite which causes it has been called the *micrococcus melitensis*. It is a minute bacterium, which, if you magnify it a thousand times, is just visible to the eye, and it is a parasite which can only live in the warm bodies of animals, on account of its requiring a high temperature for multiplication. It multiplies very little, or not at all, at ordinary temperatures, but when kept at the temperature of the blood it multiplies fairly rapidly, so that you may call it a parasite that depends upon its parasitic existence for its own existence. It cannot live outside the body and multiply there. It can only live in a dry condition, retaining sufficient vitality to set up again its activity if it again reaches the blood of a warm-blooded animal.

14207. It is called Malta fever because it was first found there or studied there?—It was called Maltese fever because it was first studied there. It was studied by Army medical doctors who go there on service, and at that time it was thought to be fairly well restricted to Malta, so that naturally, being one of these local diseases, or at least, thought to be local at that time, it received a local name. It has other names, for example, rock fever in Gibraltar.

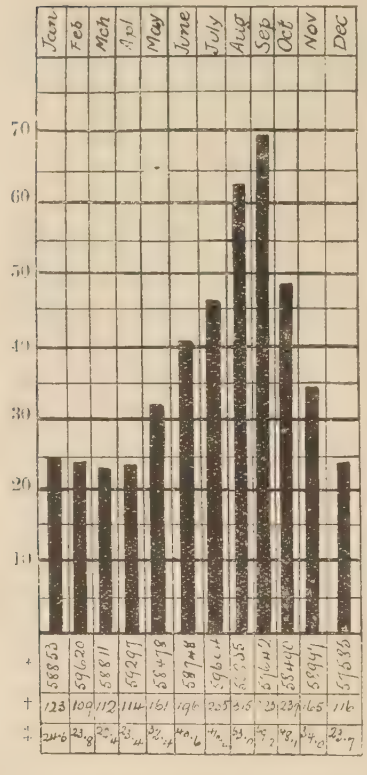
14208. (*Sir Mackenzie Chalmers.*) Is that the same fever?—Absolutely the same fever.

14209. (*Chairman.*) And in these other places it is not always known as Malta fever?—No; in Kimberley it is known as camp fever, and every place gives it its own name. Now, of course, it has been pretty well worked out, and everyone knows of it, and at present it is usually called Mediterranean fever. In the nomenclature of diseases it is known as Mediterranean fever, with the synonym of Malta fever. The change from Malta fever to Mediterranean fever was a little politeness to the Maltese people; but I think it is better to keep to the term Malta fever until they do something to clear their island of it. That is to say, we have suffered a great deal in the past from Malta fever, and therefore I always encourage the use of the term "Malta fever" in order to drive the Maltese to try to remove the stigma from their island. After they have purified their herds, then we will see about renaming this fever. The Italians call it the septicemia of Bruce; so we might call it by this name, as I have no objection to have my name associated with a disease, after they have purified their herds.

14210. You have a table, I believe, that you can hand in showing the amount of Malta fever in the troops at Malta at different times in the year?—Yes.

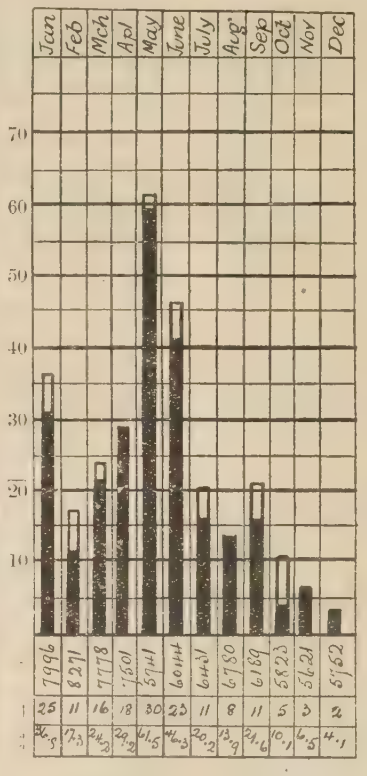
MONTHLY PREVALENCE—RATIO PER 1,000 OF STRENGTH EXPRESSED IN TERMS OF AN ANNUAL RATIO.*

CHART I
1899 TO 1905.



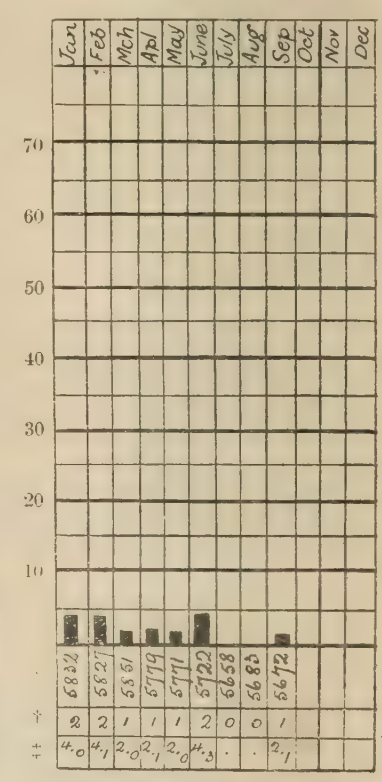
* Aggregate strength for 7 years.
† Admissions.
‡ Ratio per 1,000 of strength.

CHART II.
1906.



Readmissions shown thus.
* Average strength.
† Admissions.
‡ Ratio per 1,000.

CHART III.
1907.



* Average strength.
† Admissions.
‡ Ratio per 1,000.

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14211. That is from 1899 to 1905?—Yes, that is the chart made up from those years.

14212. It is very high in August and September?—Yes.

14213. And less just in the months each side of them and less still in the more distant months?—Yes, but at the same time there is not much to be said in regard to seasonal prevalence. When you make a chart of this sort out of a number of years you get a fixed chart, and naturally in the hottest months of the year, when people are lowered in health and when they probably take more of the infective agent in, there would be more cases; but sometimes epidemics arise in February or March, or December in some years, changing the aspect of this chart and showing that seasonal prevalence has very little to do with the disease. This chart shows that on an average there are some 400 cases of this fever among the soldiers in Malta; and at another part of this paper I give the number of cases for the Navy, and it comes to very much the same.

14214. I think you made it 37.6 per cent. per 1,000 men in garrison?—Yes.

14215. There are about 8,000 men in garrison?—Yes.

14216. Then there are three other tables which perhaps you will put in also?—Yes; the first table is the incidence of continued fevers (including Malta fever, simple continued fever, and enteric fever), among the troops in Malta from 1897 to 1906. That is put in to show that a number of cases that are returned under the name of simple continued fever are probably cases of Malta fever, and when Malta fever disappears a number of these simple continued fevers also disappear. Then in regard to Table 2 the incidence of Malta fever among officers, women and children in Malta, you find that the incidence among the officers is about four times as great as among the men. We discovered the reason for this when the cause of the mode of infection of Malta fever was discovered to be due to the milk of goats. Naturally, the officers drank more milk than the men.

14217. You mean that they were in the habit of drinking more milk than the men?—Yes.

14218. Therefore, when you discovered that, that explained the greater incidence among officers?—Yes, we could not understand before that, seeing that the officers live in good quarters and have good food and are not exposed to the same risks—that is to say, they do not go among the lower classes as much as the private does—why they, their wives and families, were much more struck by this fever than the private soldiers; but the moment we found out that this fever was due to the milk of goats, then the habits of the officers and their families in the way of drinking much more raw milk than the soldiers explained it at once.

14219. In Malta, I understand, the milk used is the milk of goats generally, just as it is the milk of cows here?—Yes, there are very few cows in Malta; even if you ask for cows' milk, and the contractor says it is cows' milk, in all probability it is goats' milk. The goat is the animal that supplies milk in Malta.

14220. Then the other table is to the same effect, showing the number of officers, non-commissioned officers and men invalided from Malta?—Yes. In the Army we were not in the habit of invaliding the men as much as in the Navy. In the Navy it was made a rule that if a man had Malta fever the moment he was able to be invalided to England he went. Of late years, for example, in 1905, there were 21 officers and 382 non-commissioned officers and men invalided from Malta, so that the habit of this invaliding was getting more marked in the Army than it had been. It was found that the men remained debilitated for such a long time that it was better to invalid them home to England. That, of course, means a great deal of expense to the State, providing passages to England, and treating the men in hospital in this country for some two months or more.

* *These charts were afterwards substituted by the witness in place of the one referred to during the evidence, in which the figures did not go beyond* 1905.

The following are the Tables handed in by the Witness.

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TABLE I.

INCIDENCE of continued Fevers, including Malta Fever, simple continued Fever and Enteric Fever, among the Troops in Malta, from 1897 to 1907.

Year.	Total admissions for continued Fevers.*	Total Admissions for			Malta Fever Ratios per 1,000.		Simple continued Fever. Ratios per 1,000 admissions. A
		Malta Fever.	Simple continued Fever.	Enteric Fever.	Admissions.	Deaths.	
1897	1588	279	1275	34	34·7	1·49	158·9
1898	1771	200	1509	62	27·1	1·08	204·2
1899	1423	275	1107	41	37·0	1·21	149·1
1900	1347	158	1158	31	19·4	·98	142·2
1901	1499	253	1205	41	31·1	1·10	148·1
1902	1174	155	981	38	17·7	·68	112·0
1903	1203	404	781	18	45·4	1·01	87·
1904	1749	320	1350	79	35·1	1·32	148 0
1905	1906	643	1199	64	77·5	1·93	144·6
1906	683	161	508	14	24·2	·60	102·6
to September, 1907.	298	10†	283	5	1·7	·2	49·7

* Continued Fevers includes Malta Fever, simple continued Fever and Enteric Fever.

† Including readmissions.

TABLE II.

INCIDENCE of Malta Fever among the Officers, Women and Children in Malta, from 1901 to 1907.

Year.	Officers.		Women.		Children.	
	Average Strength.	Admissions.	Average Strength.	Admissions.	Average Strength.	Admissions.
1901	341	25	435	26	813	9
1902	254	26	551	25	1120	18
1903	266	54	609	70	1226	33
1904	245	25	567	70	928	39
1905	217	44	548	48	860	15
1906	208	10	527	20	920	18
to end of September, 1907.	197	1	495	2	900	1

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TABLE III.

NUMBER of Officers, Non-Commissioned Officers and Men invalided from Malta to England for Malta Fever, 1897 to 1907.

Year.	Officers.	Non-Commissioned Officers and Men.
1897	9	110
1898	10	121
1899	8	120
1900	15	43
1901	9	92
1902	21	36
1903	21	133
1904	14	100
1905	21	382
1906	12	165
To end of Sept. 1907	—	1

14221. So much for the severity and distribution of the disease. The most important part, which follows, of course, is the question of animal experiment in the investigation of Malta fever. Will you now come to that?—In regard to the discovery of the germ causing a disease, it is very easy to be led astray, because when people are sick their tissues are very often invaded by other micro-organisms, so that when you examine a man's blood or examine the various excreta you may come across a bacterium, or bacteria, and it is difficult to know that you are dealing with the right one. And it is very important, of course, to know that you are really dealing with the cause of the disease, because, naturally, it would be a waste of time to work up the natural history of another harmless bacterium which had got in by accident; so that after one has found a germ in the body of a man or animal suffering from a disease, it is very useful if you can find one of the lower animals that will take the disease and so prove that the germ you are dealing with is the real cause.

14222. But it is when you have found one of these particular bacilli, wherever you find it, that it is important to test in a lower animal what its effect is?—Yes. It was laid down years ago by Professor Koch, the famous German bacteriologist, that the proof that a micro-organism is the cause of a certain disease must fulfil three conditions—(1) you must find the same micro-organism in every case of the disease, (2) you must be able to cultivate that micro-organism outside the body in such a way as to remove it from all products of the body, and (3) you must be able, by the injection of this artificial growth into healthy animals, to give rise to the same disease. Certainly, one of the most important things in beginning the study of a bacterial disease is to be able to put your finger on one germ and say that absolutely is the cause of the disease. Having settled that you can go on to study its natural history further. In the case of Malta fever another animal besides man is susceptible to the disease under ordinary natural conditions, and that animal is the monkey. So that after having tried the ordinary laboratory animals, such as the rat, guinea-pig, rabbit, and finding no result from them, I tried monkeys. When one injects this micro-organism into the former animals there is no rise of temperature and no appearance of disease, but in trying it on monkeys one found that they took a fever very similar to the fever in man, that they suffered from arthritic pains, and that, so far as you could see, they suffered in the same way as man did; and, lastly, when they died the same micro-organism was found in large numbers in pure culture in their organs. In this way Koch's three postulates were satisfied, and it was proved that the *micrococcus melitensis* is the cause of Malta fever. And it it difficult to know how you could prove this without animal experimentation, because in diseases such as dysentery you often find in the spleen, or the gall bladder, or the liver various forms of bacteria which have nothing whatever to do with dysentery, but have got in there on account of lowered vitality of tissues, and you might be led to think that they were the causes, or one of them was the cause of the disease, and in that way be led into error.

14223. Did you, by this process, discover what you now call the *micrococcus melitensis*?—Yes.

14224. You ascertained that that was the cause?—Yes.

14225. So far as you could ascertain it by those means which you think are satisfactory?—Yes.

14226. When was that?—That was in 1886.

14227. That was, I presume, by a series of experiments?—Yes.

14228. On monkeys?—Yes. The monkey is an expensive animal, and at that time there was no help given to private pathological work in the Army, so that one had to buy these monkeys out of one's own slender pay, and naturally I did not buy many, but I bought a sufficient number to enable me to say that this micro-organism was the cause of the disease.

14229. (*Mr. Ram.*) As long ago as 1886, did you say?—Yes.

14230. (*Chairman.*) The same bacteria I think you say you got from spleen of men who died of the fever in some cases?—Yes, I found this particular micro-organism in every case of death from Malta fever in man; after death, at the *post-mortem*, one removed a small quantity of the spleen and planted it out in various materials suitable for the growth of these things, and on a particular material and at a particular temperature this particular micrococcus grew, and it was from the growths, of course, on the artificial media that the animals were inoculated; that is to say, you find the micro-organism in the animal, you grow it outside their body, away from the animal, and, thirdly, you inject it into a susceptible animal and give rise to the same disease.

14231. Did you infect other animals from the blood of the monkeys that had the fever?—At that time we could only infect monkeys, because the monkey is the only animal sufficiently susceptible to take the disease by ordinary means; by feeding or by injection under the skin.

14232. Did you use experiments for the purpose of ascertaining how you could convey infection into man, or how it was likely to be conveyed?—Yes. Of course, later on, when we had the Commission, all these experiments were done in large numbers; I do not know how many monkeys were used as experimental animals, but there must have been some 150 or 200 monkeys; we had a large number of monkeys, and in using them they were used in many ways.

14233. That was in 1904-6?—Yes. For example, in finding out how the *micrococcus melitensis* leaves the body (which is naturally a very important thing to find out), it is a minute germ, and it might possibly come out in the expired air, in the urine, or it might come out as the bacillus of enteric fever principally does, from the alimentary canal in the dejecta. Then, of course, in examining how the micrococcus leaves the body, naturally you would examine the sweat, the saliva, the tears, and the other secretions. At the time this work began everybody supposed that Malta fever was due to effluvia, that it was due to this minute micro-organism finding its way into the air of wards from the sick or into the air of rooms from drains, sewers, etc., and so causing infection. Now, to put that to proof, you have to drive thousands of cubic feet through an apparatus in order to collect the micro-organisms contained in the air, and as you collect a number of ordinary air bacteria, you could not, by simple examination under the microscope, distinguish between the *micrococcus melitensis* and these others. So that here it is necessary to use an animal. The animal, as it were, picks out the pathogenic organism, which alone has the power of growing in the tissues and so causing the disease. All the other common organisms found in the air are killed off by the tissues and fluids of the animal, leaving the particular pathogenic one alive, and so its presence in the air can be proved.

14234. How would you infect the animals. By allowing them to breathe this air?—The micro-organisms would be taken from the air into a quantity

of water, that water would be centrifuged in order to throw the solid matter down, and that solid matter in a small quantity with water would be injected under the skin of a live animal. In this particular case, as the monkey was the only susceptible animal the Commission used monkeys. At one time it was thought, for example, that the grand harbour of Malta was the breeding place of this fever. On account of its being used as a sewer for hundreds of years it was until recently in a disgraceful condition, and it was considered that the sailors got the fever when going back to their ships at night. Now, in order to prove that, large quantities of the harbour water had to be examined. For that purpose a large quantity of water is passed through a Chamberlain filter in order to retain the little particles contained in the water, and then removing these germs from the filter you can experiment as to whether they contain the *micrococcus melitensis* by injecting them under the skin of a monkey. If there are any *micrococcus melitensis* they will probably set up the disease, and the one particular germ will be recovered from the animal afterwards, all the other harmless ones having been killed off by the tissues of the animal.

14235. And you found, I understand, from these experiments that the micrococcus was not conveyed either by the air, by the effluvia, or from the harbour water?—Nor from dust collected in suspicious places where one might expect to find it, contaminated places, nor from the air and dust, for instance, of fever wards, or of rooms where cases of the fever had occurred, and so on.

14236. (*Sir Mackenzie Chalmers.*) All those experiments were negative?—Yes.

14237. (*Chairman.*) And by that exhaustive process you came down to what is the only source?—By that the Commission came down to the fact that it was nothing but the milk of goats which conveyed the infection of Malta fever.

14238. (*Sir Mackenzie Chalmers.*) What put you on to the milk of goats?

14239. (*Chairman.*) First, I suppose, you found that it was something that was swallowed?—Yes, it was discovered by animal experiment that infection could be conveyed by the mouth.

14240. Something in food I meant?—Yes, this discovery came as it were as an accident at the end. In an investigation of this kind you naturally examine all the animals round about, in order to find out if any of them harbour the disease. The goat was looked upon by us as the most refractory and the most insusceptible animal we could imagine; we do not consider that the goat is readily susceptible to human diseases; it does not even take tuberculosis, which is such a common animal disease, so that we look upon the goat as the animal that is about furthest away from taking a human disease; and as only the monkey took this disease, and not the rabbit, guinea pig, rat, or dog, we considered that it was very improbable that a goat would take it. But, as a matter of routine, some goats were taken and inoculated under the skin with the *micrococcus melitensis*, and some were also fed on a small quantity of the culture—that is to say, a little of this *micrococcus melitensis* is taken out of a tube and put on the food, so that the animal eats it. These animals were examined afterwards as to their temperature and general appearance, but nothing happened. Now there is a a more delicate way of knowing whether an animal is affected by an infectious disease, and that is by an examination of the blood of the animal with the micro-organism that causes it. The blood of an animal which has been attacked by a particular micro-organism has the power of drawing all these micro-organisms into clumps. It is a very peculiar thing, but it is true that if you take the blood of an infected goat and mix it with an emulsion of the *micrococcus melitensis* in a few seconds all these micrococci are drawn together into little clumps or lumps. You then say this micro-organism has had a certain influence on this animal. So when on examination of the blood of these goats this clumping was noticed, it was seen that something had occurred; but for almost a whole year nothing more happened, and it was only, I think, in the middle of 1905, or the beginning of 1906, I forget which, that, the other methods proving useless or futile, again the goat was taken into consideration.

14241. Do I correctly understand that the goats which you inoculated got Malta fever, or only that it affected the blood in a certain way?—They did not get the fever in the way that we consider characteristic of fevers. They got the fever, but did not show any external manifestations of that fever.

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14242. They carried about the poison that gave the fever, but did not suffer from it?—Yes, the micro-organism did multiply in their bodies, but it did not give rise to any ill-health, it did not give rise to any fever. By looking at the goat you could not say that that goat was ill—it gave as much milk as a perfectly healthy goat, it was as fat and smooth-looking as a healthy goat, so that it was only by this blood examination that it was suspected that something was occurring.

14243. That is to say, you suspected that the goats had it in them before you put the inoculation in already?—We suspected that the stuff we put in had not died off as ordinary non-pathogenic organisms would when brought into contact with a non-susceptible animal, but that it had gained some ground and was multiplying to some extent, and giving rise to these changes. So that in June 1905 a careful examination was made into this question again, more goats were bought, and before injecting them, or before feeding them, their blood was examined as a matter of routine to see that the blood was all right, and, curiously enough, goats bought at this time gave this same Malta fever reaction; and then the idea struck us that the goats, the ordinary goats of Malta, might be suffering from this fever. So that at once, after having found that they give a reaction, you can examine the blood itself by removing a certain amount of blood from the veins for this micrococcus, and you can also examine the milk, and on examining the blood of these ordinary Maltese goats the *micrococcus melitensis* was found in it, and on going further and examining the milk of these apparently healthy goats a certain percentage of of them were found to contain that *micrococcus melitensis* living in their milk, and that, of course, threw a great flood of light at once on the whole question.

14244. (*Dr. Gaskell.*) Had you previously found out from experimentation that the micrococcus left the body in the milk and the urine?—Yes, in the urine, but not in the milk of human beings. You very seldom find a woman suffering from Malta fever, and at the same time secreting milk; and the monkey, of course, is not as a rule secreting milk in captivity. So that before this discovery was made the *micrococcus melitensis* had not been found in the milk of any animal. After the discovery was made, it was found in the milk of human beings—nursing mothers. But you can easily understand that beforehand one would not think of this.

14245. (*Chairman.*) The important discovery was that it was in the milk?—That was the important discovery, because it at once explained various things in the epidemiology of the disease: that seasonal prevalence had little or nothing to do with it; that whether it was a large town or small village nothing in different methods of sanitation seemed to matter; the curious fact that officers were much more struck by this fever than the privates was also explained; and after more work the Commission came to the conclusion that the only way that a man takes Malta fever in Malta is by the drinking of goats' milk. Perhaps once in a thousand times he may take it in some other way, but you can sweep all that aside; the drinking of goats' milk is the main path of infection, and naturally, of course, it is easy to stop to a great extent our sailors and soldiers drinking goats' milk, so that when the thing was once brought to the attention of the authorities, at once the disease was blotted out of the garrison of Malta, and from what I have said, of course, we look upon that as a very important and useful feat to have accomplished.

14246. Will you tell us what measures were taken then? Your Commission I suppose reported that you had discovered this particular micrococcus to be the cause of the disease?—Yes.

14247. Then were orders given that no goats' milk should be supplied to the troops?—At first there was a certain amount of hesitation, and an attempt was made to sterilise the milk by boiling and that kind of thing; but we soon pointed out that while boiling milk, if it was properly done, of course, would sterlise the milk, yet when you are dealing with the natives of these countries you never can trust that the thing is done properly. There is a small test by which you can tell whether milk has been boiled or not, and by carrying that in your pocket, and by going into the hospitals and demonstrating to the head of the hospital that his milk

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was not boiled when he thought it was, after a month or two of this the conclusion was come to that no goats' milk whatever was to enter any military or naval hospital, or any ship or regiment. The General gave an order that not a drop of goats' milk was to be used, and all the troops were put on condensed milk, and it was then that the great fall in the number of cases took place.

14248. Down to six cases instead of 700?—Yes, six fresh cases.

14249. Have you any doubt that your discovery was correct, and that you found the real cause?—I do not think there is the slightest doubt—it is so marked.

14250. You have spoken about Malta, and you have told us that the same fever exists in other places. Has it been traced to goats in those other places?—In other places they have a very strong suspicion of the goats, and certain goats have been found to be affected; but, of course, the thing has not been investigated as closely as it has been in Malta.

14251. Take, for example, a quite distant place, the Philippine Islands, where you say it exists; have any experiments been made there as to what causes it there?—I do not think so. I do not know that I know the whole literature of the subject in the Philippine Islands; but all I think that they have done at present is to find that men do have it there.

14252. Does this fever exist in places where goats' milk is not used?—Not that I know of. In India in the Punjaub, where it is found pretty frequently, goats' milk is used; and down in the south of Africa they have been bringing evidence forward lately that the more goats are used the greater the prevalence of this fever.

14253. Is it found in places where goats' milk is not used?—I do not think so. I would not say at the present moment that in every country in every part of the world the goat, and the goat alone, is responsible, because it is evidently a disease which affects in a mild way many species of animals; that is to say, that even a cow, if it was exposed to infection, would become infected. So that if you removed all the goats from Malta to-morrow and replaced them by cows, you might find yourself in the same position after some years.

14254. Do the natives go on drinking goats' milk?—Yes.

14255. Do they suffer from fever?—Yes; but, of course, not to the same extent apparently that the soldier suffers. The soldier goes there a susceptible man. The Maltese are always living among it. I imagine that nearly every Maltese has this fever at one time or another; but they drink it with their mother's milk, and they have it when they are young, so that they become, as it were, immune to it. The incidence amongst the Maltese at the present time is put down at about a tenth of the incidence the garrison suffers. But, of course, the diagnosis of the fever away in these country villages by country doctors, and the desire of those people not to offend their patients, and all that kind of thing, makes the notification returns very unreliable.

14256. (*Sir Mackenzie Chalmers.*) Is one attack of this disease protective?—That is not absolutely proved, but in my opinion it is to a great extent protective We very seldom find a soldier coming in suffering from a second attack, and the monkeys experimented with, after having recovered from a first attack, showed no signs of taking a second when infected again.

14257. They were resistant?—Yes.

14258. (*Sir William Collins.*) At what interval?—That, of course, is only an interval of about a year or so. This matter was not pushed very far. I do not think it is at all proved that one attack of enteric fever or Malta fever will make a person immune not nearly as much as scarlet fever, smallpox, or measles; those fevers do not seem to confer such marked immunity, and it is really a difficult question and a question that I would like to have answered—it has a bearing on our system of anti-typhoid inoculation in the Army as to whether one attack of typhoid fever protects sufficiently.

14259. (*Sir Mackenzie Chalmers.*) Or for a specified period?—Or for a specified period.

14260. (*Chairman.*) Supposing that you had been sent to Malta to discover the cause of Malta fever with a positive prohibition against any experiments upon animals, how would you have set to work?—Of course you could have discovered the micrococcus in the dead bodies of men, but then you would have stopped there, I think, because if you wanted, for example, to find out whether goats suffered you could not inoculate a goat; you could not even take a drop of blood from a goat's ear. I think you would have been stopped in the investigation at the outset.

14261. Supposing that you had suspected the goat, which you say you would not have done except after these exhaustive experiments in other directions—supposing you had happened to light upon the idea that it might have been due to goats—what would you have done?—In the first year of the Commission a Local Government Board man, who was recommended by Mr. Power, went out to Malta to work at the fever from the epidemiological standpoint, to examine by the method of statistics the incidence among those who drank water, beer, milk, and all that kind of thing, in order to see whether any light could be thrown upon this fever; but after very hard work for six or seven months, during which time he lost four stone in weight, he came to the conclusion that milk at least was not the cause; so that the epidemiological way of working at the causation of this fever absolutely failed. We sent out others; we sent out one or two men, and we sent out Colonel Davis, who is a man who worked at it in the same way, and he came to the conclusion that milk was not the cause, showing the difficulty of any other method of discovering the cause.

14262. Supposing you had discovered, as you might, from accidental observation in a place, that the people who drank goats' milk got it and that people who did not drink goats' milk did not get it, and that made you suspect the goat as being the cause, and that you had found this bacillus in the milk of the goat, would that have been sufficient?—I quite agree that it would not have been anything like so complete as it was after the experiment, but would you upon that have come to the conclusion that that bacillus caused the disease?—I think so.

14263. If you had found it in the spleen of all men who died of it, and you found it in the goats' milk, and that men who drank goats' milk died of this disease or got it, and that others who did not drink goats' milk did not get it?—Certainly that would be a very strong argument, and one would certainly be able to take practical measures to prevent the disease.

14264. By isolating people from goats' milk and testing it in that way you might find that you were right—you might be corroborated by the result in your conclusion that people who did not drink goats' milk invariably avoided the fever and that those that took it did not?—Yes; but what I would say with regard to that is that you would depend upon something fortuitous, some sort of accident, to make your discovery; whereas when you begin the study of a disease by the experimental method you simply go along, as it were, a well-marked track. You begin at one end and you simply go slowly forward, carefully scenting it out, and you feel sure you must find out the cause of the disease if you only have enough patience and perseverence.

14265. You go along a path which must lead you to the result. In the other case you are walking along a path with a vast number of turnings, and you do not know which is the right one?—In the one case you might sit for 20 years waiting for some fortuitous circumstance to arise which would point out the particular thing which you want to arrive at; but in the other case you begin working just as a man begins working at mending a road or any other piece of work, and you know that within a year, or two or three years, you stand a very good chance of finding out the cause of the disease and its mode of infection, or you find out the reason why you cannot find it out. For example, in cancer you cannot see anything, and you cannot get any further forward even after years of work, because there is nothing to see or cultivate or work with.

14266. (*Sir John McFadyean.*) But that experimental routine method is only certain to be successful in cases of human disease when that disease is communicable to an animal?—Yes.

14267. (*Mr. Ram.*) As I understand, the use that you made of animals, where they were of use to you, was that you wanted to eliminate all other cocci except the particular one that you wanted to find?—That is only one small part.

14268. You said that you tested these animals, and

that other micrococci might be taken up which become dead in the tissue of the animal?—Yes.

14269. But that only this latest one ultimately survived, and you found it in other animals?—Yes. That is the very beginning to prove that the particular micrococcus is the cause of the disease.

14270. But this particular micrococcus, given to animals, did not help you further than that?—Yes, it did.

14271. How?—How did the goat help us when we took a drop of blood from its ear? The goat was helping us very much, when we found the micrococcus in her blood; and the goat helped us very much when we fed it on the micrococcus, and found that, by again taking a drop of blood from its ear, a certain change had taken place, which showed that this micrococcus was living and multiplying in the interior of that goat.

14272. But with regard to other animals, monkeys and so forth, what you wanted was to enable you to recognise this particular micrococcus?—But we did not know that the micrococcus in milk, for example, was in a condition to give rise to the fever. By taking 30 monkeys and giving each one a drink of milk and finding that 25 or 26 got the fever, we had a very strong argument that the micrococcus was in a virulent form in the milk, and that the same thing would probably occur in man.

14273. After you suspected the milk you tried the milk in animals?—Yes.

14274. (*Dr. Wilson.*) Was the milk fresh when it was given to the monkeys?—Yes, quite fresh.

14275. (*Sir Mackenzie Chalmers.*) You found that the milk was not merely a concomitant, but was the cause?—Yes, we found that it was the drinking of the milk that gave rise to the fever.

14276. (*Sir William Collins.*) You mentioned the case of cancer as a case in point. Am I right in thinking that in that case investigation was not restricted to a fortuitous cause?—Yes.

14277. And that investigation has hitherto been unsuccessful?—Yes.

14278. (*Sir John McFadyean.*) I venture to suggest that there would be no objection on any ground to taking blood from a goat for examination if once a suspicion arose that the goat was actually diseased. That would not constitute an experiment even in this country. I submit that such a thing is not covered by the Act, the goat being suspected, and that it is not an experiment, but an operation conducted with a view to diagnosis; that it is analogous to the injection of mallein and tuberculin, which is often done, not with the intention of curing the animal, but with the express intention of killing it?—As I read the Act I cannot take a drop of blood from a rabbit's ear to examine, but I can knock a rabbit on the head and take all the blood in its body to examine.

14279. That is in the case of healthy unsuspected animals?—But these goats looked healthy animals.

14280. (*Dr. Wilson.*) But you suspected them of the disease?—No, we did not.

14281. (*Sir John McFadyean.*) I quite recognise that you examined their blood, although you had not suspected it?—As a routine examination, just as you take a temperature, and we were very much surprised to find that the blood reacted. I was exceedingly surprised, I remember.

14282. (*Chairman.*) You were speaking about accidental discoveries. There was an accidental experiment, was there not, on board a steamship with a cargo of goats from Malta?—Yes.

14283. Will you tell us what that was?—That took place about the time when we discovered the complicity of the goat. After the matter had been reported, of course, the attention of medical men all over the world was directed to it, and I got a letter from Alexandria saying that a sailor had been taken into the hospital there suffering from Malta fever, and mentioning that in all probability it would be found to be due to drinking goats' milk. That letter led to a complete working up of this ship epidemic. It turned out that the Americans had sent over a man to Malta, the Maltese breed of goats being a very famous breed on account of supplying a large quantity of milk, to purchase milch goats and bring them to Washington, there to set up a dairy for the supply of goats' milk for children; it was on account of the tuberculosis question." That man bought 65 goats, put them on board a steamship called the "St. Nicholas," and took them first of all to Antwerp. During the voyage in the Mediterranean the captain and nearly all of the crew—with the exception, I think, of three or four—drank the milk of these goats, and broadly every man who drank the milk took Malta fever; and even when the goats got to America, in spite of all the experience that they had had, the wife of the man who was in charge of them there also took Malta fever, and the man Thompson who had brought the goats from Malta to America died about that time, and there was a suspicion of Malta fever in his case; so that you could hardly have had a better experiment on the action of milk than in that particular case.

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14284. You do not know, I suppose, what was the history of these goats after they landed?—Yes. We, of course, informed the American Embassy here, and they took care to have those goats thoroughly examined after they landed, after they got to their destination; and I forget the percentage of the animals found to be affected by Malta fever, but it was quite a large percentage. Those animals were then slaughtered, and the healthy ones kept under observation. What has happened since I do not know. I have not heard anything since, but I imagine that even if a case of that sort had occurred under ordinary circumstances it would not have attracted any attention.

14285. That was in 1905 whilst you were pursuing your experiments?—That was the result of our discovery as regards the milk; that is to say the discovery of that epidemic on board that ship was due to our discovery of the fact that the *micrococcus melitensis* was found in the milk; because it was only that which made the man in Egypt write to me.

14286. It was only a confirmation of your discovery?—It was only a confirmation of it, and it would not have been discovered if it had not been for our previous discovery. One of these men was treated at Alexandria for Malta fever, which is a common thing there. Another man, the captain, called at Valetta, and asked the local doctor about his fever, and another was treated at the Seamen's Hospital here, and so on; this ship epidemic would probably never have been noticed if it had not been for the work of the Commission.

14287. (*Dr. Wilson.*) But no precautions were taken with regard to shipping those goats?—No, that was done without our knowledge at all.

14288. (*Chairman.*) Have the same precautions been taken with regard to the disease at Gibraltar as were taken at Malta?—I give in my *précis* of evidence a chart by Major Horrocks, which shows that the Maltese goat has almost entirely disappeared from Gibraltar in the last few years. Major Horrocks is the sanitary officer at Gibraltar. He has examined every goat on the Rock, and he has the power of slaughtering those that he finds affected, so that the precautions taken in Gibraltar are very much greater than the precautions taken in Malta.

14289. (*Sir Mackenzie Chalmers.*) And with what result?—There has been no Malta fever there for some years.

14290. Rock fever has died out?—Yes.

14291. (*Chairman.*) However, we may take it now as an accepted fact, may we, that Malta fever does come from the milk of goats?—So far as human proof can go I think it is absolutely proved.

14292. And you have told us the history and how far experimentation on living animals had a part in the discovery?—I think in any other case the discovery would have been accidental or fortuitous, but by having animals to experiment on, if you can find an animal that is susceptible to a particular organism, and if you can find the organism and grow it outside the body, it is only a question of time to discover the natural history of the organism, and find out some method of stopping the disease.

14293. Then I think you have also investigated nagana or the tsetse fly disease, which are both discovered to be the same now?—Yes, they are both the same.

14294. But that was not known at the time?—No.

14295. That, I think, was in 1894?—I went up to Zululand in the end of 1894. The tsetse fly disease before that was looked upon as being a disease caused

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by the poisonous bite of a particular fly, just as a snake bite would poison.

14296. Or as a wasp is said sometimes to kill people in England who are very sensitive?—Yes.

14297. Did you use experimentation on living animals in that investigation?—I used a large number of ordinary native dogs for the purpose, and also horses and mules and donkeys.

14298. Was that for the purpose of discovering a bacillus?—When I went up to Zululand at first, the natives sent me in a certain number of cattle suffering from this nagana. They looked very ill, but one did not know anything about their illness. The only thing to do was to examine them systematically. One of the easiest tissues of the body to get at for examination, of course, is the blood, so that one makes a very careful examination in all sorts of ways of the blood of the animal; and while I was doing that I came across a peculiarly shaped thing in the blood; it was very scarce in the blood of these cattle, but looked as if it was something foreign to the blood. At that time, of course, one was working quite in the dark. In nagana in cattle this particular micro-organism is very scanty; you may examine the blood for several days very carefully without finding one; but in some other species of animals they grow in much greater numbers in the blood, so that when I took as a matter of routine some of the blood of the cattle and put that blood into a dog it was found that the dog rapidly became ill, that its blood was swarming with this peculiar-looking thing, and therefore there was probably some connection proved between this peculiar looking thing and the disease.

14299. The dog is liable to the disease?—Yes, it is an acute disease with dogs; they die in ten days or a fortnight; whereas cattle will live on for a year, and even recover in some few cases. Often they will live for six months or a year or two years with very few of these things in their blood. The discovery that nagana and the tsetse fly disease were the same was a matter of accident. Within a few miles of the hill on which I was living was a fly-belt, and as I had read about this fly disease in books of travel, I was curious to know what the fly disease was. Accordingly, as a matter of curiosity, I sent down one or two horses, some cattle, and several dogs to live in this fly country for a fortnight, and when they were brought back to the top of the hill I examined them and, much to my surprise, I found this same creature living in their blood; and then I began to suspect that nagana and tsetse fly disease were probably the same, and that the disease was really caused by this trypanosome, this parasite.

14300. (*Sir Mackenzie Chalmers*). When the animals came back had they developed any signs of illness?—Yes, dogs develop nagana in a very few days; they had been a fortnight in the country.

14301. (*Chairman.*) You mean that you came to the conclusion not only that this thing which you found in the blood was the cause of the disease, but that the tsetse fly was the cause of its being deposited in the animal in each case?—That, of course, is a matter of experiment.

14302. And then you ascertained that?—Yes.

14303. I thought that nagana was a disease which did not necessarily occur in what is called a fly-belt?—All diseases are apt to burst over their bounds. Once in ten years or so, on account of some question of temperature or dryness of the country, the wild animals accompanied by the flies spread out over the country up the valleys in order to get moister places for grass and water, and therefore every 10, 15 or 20 years the fly disease is apt, as it were, to burst out and spread 50 or 60 miles from the fly country.

14304. It follows the wild animals wherever they go?—These flies live on the wild animals, and follow them wherever they go. If the wild animals cannot get sufficient food and water in the fly country they spread over the surrounding country.

14305. (*Mr. Ram.*) Are the wild animals made ill by the presence of these flies?—No.

14306. (*Chairman.*) Were any experiments made on animals, beyond those of which you have told us, for the purpose of discovering the cause of nagana? Did you make any preparation from them?—No, those organisms will not live outside the body on the ordinary media then in use. It is quite a different thing from a bacterium; it is an animal parasite, and at that time there was no idea of its being cultivated outside the body.

14307. The tsetse fly, I suppose, draws blood from the animal which it attacks?—Yes. In regard to animal experimentation, as I said, first of all there is the small foreign-looking thing found in the blood of cattle in very small numbers; you could not say that it had anything to do with the disease; but when you put a quantity of the blood into dogs and horses you found that this body pressed very much to the front. Then you found out by accident that the animals coming from the tsetse fly country have this trypanosome in their blood. The natives thought that the tsetse fly disease was caused by eating the vegetation in these unhealthy places, especially where there was wild game. So, in order to experiment further, I sent down horses into this fly country for a certain time, muzzled in such a way that they could not eat or drink anything. They were taken down in the morning, brought back at night, and not allowed to drink water or eat a blade of grass. The horses which were sent down in this way came back to the top of the hill and developed the same parasite in their blood.

14308. (*Sir Mackenzie Chalmers.*) They were sent down in the morning, and brought back in the evening?—Yes, and invariably we found this trypanosome in their blood, and in three weeks or so they died of this disease. Then, naturally, one began to think that the tsetse fly had something to do with it, because the flies were biting them when they were down there, and one began experimenting by bringing up these flies in cages, feeding them on a dog suffering from nagana, and then at various times afterwards feeding them on healthy animals, after twelve, twenty-four, forty-eight, and sixty hours, and so on. Thus I discovered that this fly could carry this trypanosome from an infected animal to a healthy animal readily up to forty-eight hours; so that one could see in the case of the animals going down to the fly country, and being bitten, that if they were bitten by flies even forty-eight hours after they had fed on an infected animal, it might give rise to the disease. Then another question arose: Were the flies themselves, as you found them in the fly country, capable of giving rise to this disease without any feeding on infected animals up above? Accordingly, I brought up cages full of these tsetse flies to the top of the hill, and put them straightway on healthy animals, and the healthy animals all got the same disease. Therefore, the fly down in the fly country was infected with this parasite, and could give it to any domestic animal that came near.

14309. (*Chairman.*) A fly, do you mean, that was not a tsetse fly?—No, only the tsetse fly.

14310. I thought you said the common or garden fly. You meant the common tsetse fly?—Yes. I meant the ordinary tsetse fly as found in the bush and not artificially infected. Then one naturally wondered where these things got this little parasite from, and, in order to find that out further experiments were made. The flies feed, of course, upon the wild animals; what more natural then than that they got this parasite from the wild animals? The wild animals are perfectly healthy looking and sleek, and do not appear to be suffering from anything. Accordingly, I went down and shot many wild animals, buffaloes, koodoos, wildebeeste and others, and the moment an animal was killed I sent up a bottle full of its blood to the top of the hill, where there was no nagana at the time, and injected that blood into healthy dogs. A certain number of these dogs took nagana, and I found this same parasite in their blood. In this way I showed that these wild animals had this thing in their blood; and afterwards, to make this more sure, by microscopic examination of the blood of the wild animals, and by patiently looking for a long time microscopically at the blood of these wild animals, I discovered the thing itself in their blood, in small numbers, of course; in large numbers it would affect their health. In small numbers, so far as I know, it does not affect their health. So that there, again, by animal experimentation, it was proved that the wild animals act as a reservoir of this particular parasite, and I do not see how one could have proved it otherwise. Even when one knew that the parasite was in the blood, it took a whole day of examination of very many specimens to find a single parasite with the microscope; and one would not have examined so

long and so carefully unless one knew that one would find it if one had patience.

14311. It helped to ascertain the cause of the disease?—I think that animal experimentation helped in everything.

14312. Has it helped you at all at present to discover any remedy?—It was in 1895 I sent home a dog suffering from nagana to London, which dog arrived here alive; that is to say, as one dog died another was inoculated, and at last a dog was delivered in London with this living trypanosome in its blood, and these trypanosomes have been spread from London from that one dog to scientific workers all over the world, and there has been an enormous amount of work done in the last ten or twelve years with regard to these trypanosomes. There have been, I should say, thousands of animal experiments with a view to find a method of serum treatment, and quite an extraordinary number of experiments have been made to find a method of medicinal treatment: and, as you know from the daily press, within the last few days there has been discovered a powerful drug, one injection of which, for example, into a guinea-pig which is just dying of this disease, which would not have ten minutes to live (I do not think I am exaggerating), which is at death's door, will drive all the trypanosomes out of its blood, and in a day or two that animal will appear healthy, and animals so treated have remained without trypanosomes in their blood for, I think, two months.

14313. What is the drug?—That is not published yet, so I will not mention the name; but another drug which has been used very much is a drug which is called atoxyl, an arsenical preparation.

14314. (*Sir Mackenzie Chalmers.*) That is a dangerous preparation, is it not—dangerous to patients, sometimes?—M. Nicolle, of the Pasteur Institute, said that he thought it might be dangerous by accumulating in the system, but I do not think there is any proof that with care there is danger to human beings. Koch has been to Uganda, and in the last few months has been using it on many thousands of natives, as far as I am aware, without any fatal results.

14315. (*Chairman.*) I understand that you are not specially engaged in the discovery of remedies at present?—I am on the Royal Society's Committee for the discovery of a drug to cure sleeping sickness.

14316. I presume that in almost every case the discovery of the cause of a disease is the first step towards the discovery of a remedy for that disease?—Yes.

14317. And, to that extent, at any rate, you think you have got on to assured ground with regard to nagana?—In regard to these trypanosomes, certainly. Sleeping sickness is another disease of the same kind.

14318. Do you think that progress is being made towards recovery and remedy by means of experiments on animals?—Yes. I never dreamed that we would be able in time to cure an animal of trypanosomiasis. It is a very difficult question, and one does not want to say anything decided, the matter being still *sub judice*. When I was in Zululand, in 1894 and 1895, I tried arsenic on horses affected by nagana, and I was able to keep those horses alive for a year by giving them arsenic in their food. As a rule, horses die in about three weeks of this acute disease. Of course, during that year I thought I had a method of curing horses of this very fatal disease. But at the end of the year the trypanosomes came back into the blood, and, although one doubled or trebled the dose of arsenic in the animal's food, the trypanosomes paid no longer any attention to the arsenic, but multiplied and killed the animal, and in that condition our knowledge has remained till within the last year or two. Lately a great deal of work has been done, especially in regard to sleeping sickness, and now the idea is that by giving an arsenical preparation first, and killing all those parasites that are susceptible to arsenic, you can kill off the remainder by giving them another poison. The poison used has been, first, atoxyl, and then a preparation of mercury, and no doubt Mr. Plummer has kept animals alive in this way for some 300 days. These rats ought to die in five days. The Liverpool School of Tropical Medicine have got much the same result. It is a question whether we can say that these animals are really cured; but, however sceptical I may be, I imagine that when a rat lives 300 days instead of five, it looks very much like a cure.

14319. (*Sir William Collins.*) These are all chemical agencies to which you refer, not sera?—Purely drugs, just like quinine in malaria. The parasite in sleeping sickness and nagana is an animal parasite closely related to the malarial parasite. It is much more delicate than a bacterium. If you put it on this table it would die in a few minutes; if you put the *micrococcus melitensis* on the table, it would live for months. The one is much more delicate and much more easily got at by drugs. These protozoal parasites are more easily affected by drugs given by the mouth than are bacteria.

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14320. (*Dr. Gaskell.*) They are supposed to be mere animal parasites, and the others are supposed to be vegetable parasites?—Yes.

14321. (*Chairman.*) There is something you can tell us about experiment with sleeping sickness. You, I believe, went out to Africa to study the disease?—I went out to Uganda in the beginning of 1903. Up to that time there had been a certain amount of work done on sleeping sickness, especially by the Portuguese, who sent out a Commission to the West Coast of Africa. The result of their work was the belief that sleeping sickness is caused by a certain streptococcus, one of these vegetable parasites belonging to the same class as the micrococcus melitensis. When I arrived in Uganda, the Royal Society's Commission had been at work for about a year, and the conclusion they had also come to was that this disease was caused by this bacterium. I found one member of the Commission still there, and in the course of conversation he told me that on five occasions he had seen a different kind of parasite in the sleeping sickness patients, namely, one of the trypanosomes. Trypanosomes by this time, of course, had become well known, and on the West Coast of Africa they had been found in the blood of man. These parasites had been named Trypanosoma Gambiense, and cases of this disease had been sent home to England. Curiously, it was never dreamt that this trypanosoma fever, as it was called, had any relation to sleeping sickness; so that when this man in Uganda mentioned to me that he had seen on several occasions trypanosomes in sleeping sickness, he said that it was curious that this trypanosoma fever extended into the interior of Africa, and he also, like the others, had no idea that the trypanosoma had any causal connection with sleeping sickness. Now, on account of my old work in Zululand, having worked at a trypanosome disease for two years, I was naturally very interested in this observation of his, and I said to him—If you will wait a few weeks longer (he was going to England at the time) I will examine cases with you on the hypothesis that this trypanosoma may really be the cause of the disease.

14322. You mean that you would examine cases of people with sleeping sickness?—Yes. So he remained for three weeks longer, during which time we examined some 30 cases of sleeping sickness for trypanosomes, and for trypanosomes alone. I simply wanted to find out in how many cases one could find trypanosomes.

14323. Taking blood from them, do you mean?—No, in that case we took the cerebro-spinal fluid.

14324. After death?—No, during life. We gave the natives chloroform, and removed about an ounce of fluid from the vertebral canal, and had it centrifuged in order to throw any little solid bodies down to the bottom. I examined the sediment, and in most of these cases we found the trypanosome. In fact, after this man left, and I went into the thing more carefully, this trypanosome was found in the cerebro-spinal fluid in every case of sleeping sickness examined. Now, the moment one finds a parasite occurring in 100 per cent. of a particular disease, one begins to think it may have something to do with it. Of course, it is no proof that, because you find it in 100 per cent., it is the cause of the disease, but still it is a hopeful beginning. I put aside the idea of sleeping sickness being caused by a bacterium, and went for the trypanosome. After finding it in 100 per cent. in the cerebro-spinal fluid, we went on to examine the blood. Just as in the cattle in nagana the trypanosomes are exceedingly scarce in the blood in sleeping sickness; and it is only by very patient work and examining large quantities of blood that you can find them. In this way we found this same trypanosome in every case in the blood, except on one occasion. So we had now learned the fact that in the blood of sleeping-sickness cases, and in the cerebro-spinal fluid, we could always find this parasite. The next point we investigated was this. If this parasite is an ordinary harmless guest of the people of this country, then it will be found in the natives who live outside the sleeping sickness area, as well as in those

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who live within this area. Just as in nagana you have a fly country, so in sleeping sickness you have a sleeping sickness area, outside which the natives will not suffer from sleeping sickness. So we examined some 200 cases of natives from a region outside the sleeping sickness area, and we could not find this parasite in a single case.

14325. Examining in the blood?—Yes. We examined the blood in the same careful way that we examined the sleeping sickness blood, and in not a single case could we find the parasite. Then it began to look as if this trypanosome had really something to do with the disease. Accordingly we took cerebro-spinal fluid containing these living creatures, and injected this fluid into the ordinary animals we found round about. The monkey, of course, is very common there, and so are dogs and cattle. I had taken out some guinea-pigs and rabbits, and so we injected this parasite into all these animals and awaited results.

14326. Did you consider that the sleeping sickness must be caused by some bacillus or coccus in the cerebro-spinal fluid?—I thought, of course, that it must be caused by some living organism, because when you have a so-called infectious disease attacking large masses of men it is probably caused by a living organism.

14327. It was only in the place where you looked for it? You spoke first of having examined the cerebro-spinal fluid?—The principal symptoms in sleeping sickness are nerve symptoms, and you would naturally make a careful examination of the nervous system.

14328. You had expected to find traces of it there?—Yes. Just as Pasteur, in hydrophobia, went to the brain of the animals to find the virus, so when an animal is suffering from a nervous disease you go to the nervous system.

14329. It was the symptoms that induced you to look there?—Yes, the symptoms are purely nervous symptoms. To return to the injected monkeys. The blood of these monkeys was examined every day, and their temperature was taken very carefully, and they were watched. After five or six days the same trypanosome appeared in their blood, and we found it daily for some time. Sleeping sickness is a very slow, chronic disease, and at the end of three, four, and five months these monkeys began to show the same kind of symptoms that men show; they became very lethargic, and sat all day, as it were, with their heads on their knees, apparently asleep, and in the course of time these monkeys died. Then, on some occasions, we examined the cerebro-spinal fluid of a monkey during life, but it is difficult matter to examine the cerebro-spinal fluid of such a small animal; after death we examined the cerebro-spinal fluid, and we found this trypanosome in it as we had found it in the blood. In man there is a particular change in the brain, first described by Dr. Mott, so that if you cut a section of the brain you can tell by the appearance of it that he has died from sleeping sickness. If these monkeys died of the same disease, you would naturally expect to find this peculiar anatomical change of the brain. This anatomical change of the brain was found first, I believe, by Captain, Harvey, R.A.M.C. And therefore the conclusion has been come to that the disease of which the monkeys died is the same as the disease in man, and that in all probability the trypanosome is the cause.

14330. (*Sir William Collins.*) Is that change always found in the monkeys?—No, not nearly always, because the monkeys die much more quickly than man, and this change is only found in the chronic cases.

14331. (*Chairman.*) Only found in man, do you mean, in the long chronic cases?—It is only found in the monkey in those cases which last a long time. It is a very slow, chronic change which takes place. The monkey dies in three or four months, and man dies it may be in three or four years. The length of time the poison has been acting brings about these chronic changes in the brain; whereas if a man dies in the early stages of the disease, or if a monkey dies after three months, neither of them will show this peculiar anatomical change.

14332. (*Sir Mackenzie Chalmers.*) For instance, if a man infected with sleeping sickness was killed accidentally three or four months afterwards, you would not expect to find this particular anatomical change?—No, I do not think that it would be found after an illness of only three or four months.

14333. (*Sir William Collins.*) Are these microscopical changes that you allude to often absent in the brain of monkeys that are subjected to the disease?—Yes.

14334. (*Sir Mackenzie Chalmers.*) If the disease runs its normal course you usually find this microscopical changes?—Yes, it is a difficult matter, of course, making this particular observation, and in Uganda it was not done very often; but the examination was made for the Commission in London.

14335. (*Sir William Collins.*) It was found by Dr. Mott, was it not?—Yes, and the cases in which he found it were usually chronic cases. We sent him specimens of every brain we examined in Uganda, so that he had a great deal of material to work on during this investigation. We did not cultivate these trypanosomes outside the body, as was done in the case of Malta fever, so that the proof is not as good. You may inject into a monkey cerebro-spinal fluid, which you know to contain a certain living thing, but it may also contain something else, so that the proof is not as complete as in the case of a bacterium which you can grow outside the body, and then inject into a susceptible animal. The growth of the trypanosome outside the body of sleeping sickness has never been accomplished. But when you have done a number of experiments of this kind; when you find the same parasite in every case of the disease; when you find it giving rise to the disease in animals, you come to the conclusion that in all probability you are dealing with the right thing. Then the next point to investigate was how are these natives infected, and, from my old work on nagana, I at once thonght that a tsetse fly would be the carrier of the trypanosome. At that time it was not known that there were any tsetse flies in Uganda, but when we went down to the lake shore and began to hunt for them we found a particular species of tsetse fly, different from the nagana one present, and in large numbers. We caught these flies, kept them in cages, fed them on sleeping sickness natives, and then on healthy monkeys, and we found that these tsetse flies could carry sleeping sickness from a sick native to a healthy monkey. And then we made the further experiment. As there were a great number of infected natives on the lake shore, we collected these flies from among these natives, brought them up to the laboratory, and put them straightway on healthy monkeys, and we found that these monkeys took the disease. Then the next thing we did was to investigate the distribution of the disease. The distribution of sleeping sickness in Uganda is very extraordinary. It is confined to a narrow belt running along the margin of the Lake Victoria and its islands. Further, the disease is only found where there are forests. You do not find sleeping sickness where grass plains came down to the shore. You find no sleeping sickness if there is a belt of papyrus reeds growing along the lake shore. In the interior of the country there is no sleeping sickness. It is a very peculiar distribution. Therefore, I said to myself, if this tsetse fly carries it, very probably this fly has also a peculiar distribution. The tsetse fly which carries nagana is spread all over the country. Then we got hold of the native chiefs, and they set their minor chiefs all over Uganda to collect flies, and within a couple of months they sent in about a thousand collections of flies from all over the country. At the same time, we asked them to send in a word as to whether sleeping sickness was found where the flies were found. Then we separated all these biting flies into two categories, namely, this particular tsetse fly and the others, and we took two large maps, and whenever a collection of flies came in from a place containing one or more of these tsetse flies we put a red mark on that spot on the map. We also had another map of the same size, and if a chief reported that sleeping sickness was found at a particular place, we put a red mark on the same place on the second map. This went on for some months, until at last we had about a thousand observations, and then, when you looked at the two maps, you found that the red spots on one map coincided with the red spots on the other map, and the blue discs which we put on the places where no tsetse flies were found coincided exactly with the places where no sleeping sickness was found. Accordingly, we came to the conclusion that the distributions of sleeping sickness and of this particular species of tsetse fly were identical, and that gave us another proof that this trypanosome was the cause of the disease, and that there was only one way by which

man could become infected, and that by being exposed to the bites of this particular species of tsetse fly. Since then a good deal of work has been done, but that is now, I think, established beyond doubt.

14336. (*Chairman.*) From what quarter did the tsetse fly get it in order to convey it to man?—I think in Uganda the principal reservoir is man himself.

14337. From man to man?—From man to man.

14338. (*Sir Mackenzie Chalmers.*) Carried by the fly?—Yes.

14339. (*Chairman.*) In Africa, then, the tsetse fly bites mankind?—Yes; I think they all bite man. All the flies we worked with were caught on men. The tsetse fly gives the sleeping sickness trypanosome to man, but it does not give the nagana trypanosome to man.

14340. (*Sir Mackenzie Chalmers.*) The tsetse fly of nagana feeds on man and on animals, but man is not susceptible to the disease. It does not live in man's blood. The trypanosome of nagana is communicated to man, but does not affect him?—That is so; it dies off; it does not live in his blood.

14341. Do you find the trypanosome in the body of the tsetse fly itself?—Of course, if you examine the blood from the stomach of a tsetse fly, naturally if it has fed upon an animal containing trypanosomes, you find them there quite readily, and you find them there for some five days; it takes about five days for the full feed of blood to disappear altogether from the stomach, and as long as there is any remnant of blood in the stomach you may find the trypanosome.

14342. Do you know at all the mechanism by which the tsetse fly communicates the trypanosome from itself into the wound that it makes in man?—No; I believe it is by simple mechanical transference. There may be a more complicated mechanism behind. All that we have been able to discover up to the present time is that in the proboscis, which is a well-marked tube (I have examined thousands of these probosces), up to forty-eight hours after feeding on an infected animal, you can often see living trypanosomes. Whether they wriggled up from the stomach, or whether there was any other mechanism by which they got, as it were, from the blood, or whether they simply remained there after feeding, is not known, and it is rather difficult to find out. But let us suppose there is a living trypanosome in that tube. Now, when the fly thrusts that tube into a new animal, it injects a certain amount of fluid. This is the cause of the inflammation of the part—it is the cause of the greater supply of blood to the part, just like the sting of a gnat. If, then, you have a tube with a living trypanosome in it, and the fly squirts down it a quantity of fluid, in all probability the living trypanosome will be thrown into the blood of the attacked animal, and when it is there it at once begins to multiply.

14343. The trypanosome inoculates you, in fact?—Yes, the tsetse fly inoculates you with the trypanosome.

14344. (*Mr. Ram.*) Is there any way of accounting for the distribution of this fly in Uganda? You spoke of its distribution as being very curious?—It is a very curious distribution, but it is very difficult to arrive at the reasons in nature for the peculiar distribution of animals. You will find on one hillside in England one particular species of butterfly, which you will not find in any other place. I do not know that any person can give the reason for that. Sometimes you may have a theory, but it is difficult to find the true reason. In this particular case they evidently depend upon the presence of clear water. We do not know anything in their natural history which would make them depend upon clear water.

14345. They are always found in the company of clear water?—Yes, if you have a river full of reeds, a marshy river, which is a common kind of river in Uganda, the fly never goes up there; it requires a river which has always lots of water in it; it must have forest and clear water; it will not live in grass alongside water; it will not live in a banana plantation alongside water. It lives where there are tall trees and jungle and clear water. Koch says, or is reported to have said, that they live on crocodiles principally, and that may be the reason for their distribution.

14346. (*Sir Mackenzie Chalmers.*) How far away do they fly?—In Uganda I do not think they are found, as a rule, more than 150 yards away.

14347. (*Dr. Gaskell.*) The jungle must be close to water?—Yes.

14348. So that if you cleared away all the jungle close to water you would clear away the fly?—Yes.

14349. That is being done at Entebbe, is it not?—Yes, and at ferries, and such-like places.

14350. (*Chairman.*) You have told us that you used experiments on monkeys for the purpose of finding this trypanosome and identifying it as the cause of the disease, and I understand that you also used experiments on monkeys for the purpose of testing whether the tsetse fly was the distributor?—Yes.

14351. Do you consider that those experiments were necessary in order to make the discoveries which you believe you have made?—I think so. It would be very difficult to imagine any way of proving that the tsetse fly carries the disease unless you have an animal to experiment with. You could not do it on man, and after finding the trypanosome in the cerebro-spinal fluid and in the blood of man, you would have stuck—you could not have gone a step further. It was only by using the monkey in place of a man that you could prove that this thing did give rise to a disease, which was similar to sleeping sickness, and also that the flies feeding upon the natives on the lake shore could give the disease to a monkey. Further, it would be difficult to prove that flies fed on a sleeping sickness patient could give rise to the disease in healthy monkeys unless you used animal experimentation. I do not think you could have gone any further than the mere discovery.

14352. You were first set on inquiry about the capacity of the tsetse fly by the recollection of what had happened with the tsetse fly in nagana?—Yes.

14353. And having done that and looking upon it as a suspicious individual, then you got the chiefs to give you this information in regard to the distribution of the flies?—Yes.

14354. That was not a matter of animal experiment?—No.

14355. That seems to have satisfied your mind that the tsetse fly did convey the disease?—Yes.

14356. So that so far you may have used experiments as confirmatory of your theory about the tsetse fly, but you were almost morally certain, before you made those experiments, that the tsetse fly was the distributor?—These animal experiments were made to begin with; the chiefs were called in after the experimental work had been done. Of course, there were no more experiments made on the monkeys than were supposed to be necessary to prove that the trypanosome was the cause of sleeping sickness.

14357. I was mistaken. I thought your experiments to find the distributor of the trypanosome had been subsequent to your giving these instructions to the chiefs?—No.

14358. You gave those instructions in consequence of finding the trypanosome in the tsetse fly?—In consequence of finding that this tsetse fly could convey this trypanosome to healthy animals. The next point was to find out why the distribution of sleeping sickness was so peculiar.

14359. Do you think that in regard to this sleeping sickness as in regard to nagana, the animal experiments were valuable and essential to confirm your discovery?—Yes, I think so.

14360. As to remedies, you have not gone into that in regard to sleeping sickness?—There has, of course, been an enormous amount of work done on sleeping sickness in regard to remedies, but I have not done any.

14361. I understand that you have not made a special study of remedies?—No. Not in sleeping sickness.

14362. I think we shall have another gentleman from Liverpool who has done so.

14363. (*Sir William Collins.*) I should like to ask you a few questions to supplement your very interesting evidence. In the first place, as regards the experiments that you have made in these various researches and experiments, should I be right in thinking that they were very painful experiments?—No, none of these experiments were painful. Most of them were done by a hypodermic needle being inserted into the skin, and a small quantity of fluid injected.

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14364. I rather gathered that you would tell us that your experiments were mostly confined to punctures with the hypodermic needle—drawing blood, feeding experiments, and so forth?—Yes.

14365. Was there anything more painful than that?—I do not think so.

14366. And would the result in the animals in which there were pathogenic results be painful illnesses?—No, I do not think so. In the case of Malta fever most of the monkeys recover after a week or two of fever. A small percentage of them die, and, of course, they must have a disagreeable feeling due to the high temperature, and those who recover may have a chronic disease as in man, lasting for some months. I dare say they may have symptoms of arthritis, slight swelling of the joints, and that kind of thing, just as in man, but I would not say that in the monkey there was what you would call an agonising or even painful disease.

14367. Have any of your experiments necessitated the performance of anything like surgical operations upon animals of any extensive character?—No, I have not been associated with many experiments of that kind. I worked with Wright at one time on the leucocytes of the blood. We made a few experiments on placing a canula in the thoracic duct of dogs and removing a certain amount of fluid for the purpose of examining it; and I have also performed a few operations, such as removing the spleen of dogs, to find out what effect it would have upon the leucocytes of the blood; but those were all done under chloroform, and the dogs were killed after the thoracic fluid was drawn off. I do not think that any experiments, in the performance of which I have been associated, have been what one would call painful experiments.

14368. I understood you to say that as the result of your experiments with Malta fever you claimed that you had been led to a system of prevention of that disease, on what you call broad lines rather than on serum lines?—Yes.

14369. Would you tell me exactly what you mean by that?—If, after a great deal of work, you discovered a serum which would immunise a man against Malta fever, you would require to inject each of the men several times with the serum, and that would be what I call narrow lines. If you give an order that no goat's milk is to be used in the Army or in the Navy, and you stop the disease at one blow, like cutting off the head of a snake, I call those broad lines.

14370. I thought that was what you had in your mind. When you claimed to have saved the garrison and the Navy, it was by the method of stopping certain articles of food, not by methods of vaccinating or inoculating?—That is so.

14371. (*Sir Mackenzie Chalmers.*) Or medicine, or cure?—Yes, that is the kind of way of stopping disease I like to aim at always.

14372. Prevention?—Yes, it is far better.

14373. (*Sir William Collins.*) I think it has been called the stamping out and stamping in method?—Yes; stamping out.

14374. Has an attempt been made to prepare a serum against Malta fever?—Yes, and it has up to the present failed. Eyre, in this country, tried to prepare a serum from the horse, but his conclusion was that the serum was of very little use.

14375. Did it sometimes produce rather alarming symptoms?—I did not see the experiments, and I do not know. You will find his paper in the reports of the Royal Society on Mediterranean fever, but I do not think he gives any alarming results in that paper. I do not think there were any alarming results; they were merely experiments made on guinea-pigs.

14376. On page 120 of Part VI. of the Reports of the Mediterranean Fever Commission I find that he speaks of the disquieting results recorded by Lieutenant Bousfield?—Is that serum or inoculation fluid?

14377. That is in the Report of the Commission?—Was Bousfield working with serum or with inoculation fluid?

14378. It is under the heading of "Sequelæ of Inoculations"?—Those are very different things; the one is the serum of an animal immunised against the disease; the other is simply dead bacilli injected under the skin of a man to bring about the same sort of changes in a minor degree that the man would get by having the disease itself.

14379. My question was rather whether any of the methods, which you spoke of as narrow lines, had resulted in any disquieting symptoms?—I do not think so. The method of immunising by an injection of the dead *micrococcus melitensis* has been tried to some extent; that is to say, Wright has tried it, Bassett Smith, of Haslar Hospital, tried it as a curative agent, and several others; and we tried it in Malta, but up to the present I should say with very little result indeed, either as a curative or as an immunising agent.

14380. You compared the mortality of Malta fever with that of typhoid, and I understood you to say that although Malta fever was relatively rarely fatal, it caused a prolonged amount of suffering and ill-health; but could you give us some proportion as to the case mortality of Malta fever as against that of typhoid?—I do not suppose that the mortality of Malta fever exceeds 2 per cent., as a rule; whereas, of course, in typhoid you may have 10, 15, or 20 per cent.

14381. In the pamphlet which you have handed in you give the total admissions of Malta fever, simple continued fever, and enteric fever among the troops from 1897 to 1906, and you call attention to a drop in the Malta fever admissions from 1905 to 1906 of considerable magnitude; is that not so?—The preventive measures came into being on the 1st of July, 1906, and it was after that that there was a big drop in the number of cases.

14382. Then, during the first half of 1906 were there no preventive measures at work?—No.

14383. Could you complete the figures for 1906, because these are only to the end of October, and if the preventive measures began on the 1st July, and you give the figures only to the end of October, that would only cover a very short period?—I have not got the figures with me. We have them in the office, of course. But I have the figures for 1907. You must remember that when you start preventive measures of this kind naturally there are a number of men who are sickening, as it were, for the disease. The incubation period of Malta fever is probably in some cases long—a month or two months, it is supposed—a man may be carrying the micrococcus without evincing the disease, so that after you begin your preventive measures you expect, of course, a certain number of cases to occur for some time to come. Accordingly, in that Chart 2, in July there are still a number of cases—there are ten cases, which gives an annual ratio of 18.6. In the chart from 1899 to 1905, in July there are 235 cases in the seven years, giving a ratio of 47.8. In July, 1905, there were 77 cases.

14384. I was confining my question to Table I. on page 4. There you see that the total admissions of Malta fever in 1905 were 643?—Yes, that is the same chart as Chart 2, on page 10.

14385. I daresay. Then the figures there given for 1906 to the end of October are 147. I understand from you that the preventive measures began on July 1st, 1906?—Yes.

14386. I was anxious to know whether you could complete the figures to the end of the year 1906, or whether you can say that the drop from 643 in 1905 to 147 in 1906 to the end of October could be attributed wholly or in part to the preventive measures, seeing that they did not begin, I understand, until the 1st of July?—In 1905 the total admissions of continued fever were 1906.

14387. I am speaking of Malta fever?—Of Malta fever there were in 1905, 643, and in 1906, up to the end of October, 147.

14388. But do not you see that there was a great fall from 643 cases in 1905 to 147 down to the end of October, 1906? I understand from you that the preventive measures began on the 1st of July?—Yes.

14389. So that, really, during the first half of the year 1906 those preventive measures could not have been operative?—In the first half of 1906 there may have been preventive measures to some little extent, because the thing was getting known, but it was only about the beginning of July that the order came out that no goats' milk was to be used under any circumstances whatever. It would be tinned milk in the year before that they had been using, or milk that they tried to boil or sterilise.

14390. But in order scientifically to attribute a particular fall of a disease to some particular cause, it is necessary to eliminate other causes which might have been at work, is it not?—Yes. But I think this thing is so clear, and so clearly dependent upon the

milk, that I shall be obliged if you can show me any fallacy.

14391. Then will you tell me, do you think that the whole drop in admissions of Malta fever from 643 in 1905 to 147 down to the end of October, 1906, is attributable to those preventive measures?—Yes.

14392. Although I understand you to tell me that those preventive measures did not begin until the 1st of July?—Yes.

14393. (*Dr. Gaskell.*) Have you not stated, on page 11 of your pamphlet, that in July, August, and September, 1905, there were 258 cases, whereas in the same months of 1906 the numbers fell to 26?—Yes, and I have said that the preventive measures only came into use about the beginning of July, 1906, and the remarkable diminution is at once seen.

14394. (*Sir William Collins.*) I will not trouble you further on that point. If you would still look at that table on page 4, you will see there is a great drop in admissions of continued fever in the same period from 1905 to the first ten months in 1906 from 1,199 to 462. To what do you attribute the fall in simple continued fever; would that also be due to the preventive measures that you adopted?—Yes. Simple continued fever is one of those vague terms in which is probably included a number of mild cases of Malta fever, and also mild cases of enteric, so that anything which would lessen the cases of Malta fever or lessen the cases of enteric fever would have an effect upon the number of cases returned of simple continued fever.

14395. Does the same answer apply to the next column, where the enteric fever fell from 64 to 9?—To answer your question categorically, I should say no, because enteric fever is a more definite entity than simple continued fever; but it also is a difficult fever to diagnose from Malta fever. When I went out to Malta first, every severe case of Malta fever was called enteric, and all the milder cases were called remittent; so that, doubtless, in spite of the excellent diagnoses made by medical officers of the British Army, cases of Malta fever do get returned as enteric in spite of all the care that is taken, because the symptoms of the two diseases are the same for some time. I daresay that a man with experience at the end of either of the two diseases would give a very accurate guess whether it had been Malta fever or enteric; the longer duration of the one, perhaps, and these arthritic pains and swellings of the joints in a well-marked case, would help to make the diagnosis, but people with less experience are very apt to say: "This must be enteric; the man has a black tongue, high temperature, and a little delirium"; and it is diagnosed as enteric. And it is a difficult matter, as you know, to change the names of diseases in the Army. There is a certain amount of changing names in books and that sort of thing involved, and in many cases I daresay the diagnosis remains, and that might account, to some little extent, for that nine among the enteric fever.

14396. You think that the preventive measures in respect of the prohibition of milk put in force in July, 1906, are responsible for the drop in all these cases of Malta fever, simple continued fever, and enteric fever from 1905 down to the end of the first ten months of 1906?—Why not? Enteric fever is very often carried by milk. If you blot out the chief cause of Malta fever—namely, milk—you will also make a difference in enteric fever, because you must remember that as the Malta water supply is beyond reproach, enteric fever would be carried by flies contaminating milk, and that kind of thing, I should say, frequently. We did not investigate into enteric fever, but if you blot out the whole of Malta fever you will make a difference in simple continued fever and enteric.

14397. You blot it all out?—If you blot out the whole Malta fever.

14398. Do you mean that there is so much error in diagnosis that the whole typhoid will disappear if these measures are put in force with regard to Malta?—No, I do not think so. I do not think that enteric is carried only by goats' milk. I imagine that a fair percentage of enteric fever is caused in Malta by milk, and if you blot out all goats' milk, and only use condensed milk, say from Switzerland, in tins, you will blot out a certain amount of enteric, but you will not blot it all out, because enteric does not depend altogether, I should say, on milk. In fact, we do not know all the routes by which enteric is carried. I should be very happy to work at that most important question, but enteric fever has never been subjected to as searching an investigation as this Malta fever; and at the present time we do not know exactly how enteric fever is carried, especially in different parts of the world. In England you have often water-borne enteric, or you have an epidemic when it is milk-borne. In India you may have an epidemic where you cannot say it is from water or milk, but you must go in for some other theory, such as contamination of food by flies. In enteric fever it is fairly unknown as to how the infection is carried from one case to another.

14399. (*Sir Mackenzie Chalmers.*) There are probably many channels in England?—Yes, probably.

14400. (*Sir William Collins.*) With regard to the special case to which you called our attention, of the "Joshua Nicholson," and the goat's milk on board ship, the full story is told, is it not, in Part VII. of the Reports to which you have already alluded?—Yes, by Staff-Surgeon Clayton, R.N.

14401. Did I rightly understand that all the crew of the "Joshua Nicholson" were affected?—No, there were several of the crew who were not affected, so far as one can make out. It is all written at very great length by Clayton, and I think the general result that is brought out is that the men who drank that milk in any quantity got the fever.

14402. What happened to the goats at Antwerp?—We have everything that is known about the matter in that paper. We know where the goats were kept; they were kept in some kind of enclosure in Antwerp; but we failed to trace any fever cases in Antwerp, and we failed to trace any cases of fever between Antwerp and America.

14403. I just wanted the story completed. I think that is so?—It is all put in that paper, so far as I remember, but that is my impression. We could not follow these cases any further; we could not get hold of the people after the goats left the "Joshua Nicholson."

14404. I find that at Antwerp: "The staff of the quarantine station and many individuals in the neighbourhood are said to have partaken of the milk, both raw and boiled, during the five days the goats were interned here, but no information can be obtained of the subsequent occurrence of cases of illness resembling Mediterranean fever." And, further, "The s.s 'St. Andrew' carried 30 cattlemen and the three goat herds, and Mr. Thompson, in addition to a crew of 30 men. Most of these drank of the milk, but the master of the ship and also his owners state that none of the men suffered from any illness"?—The captain of a sea-going ship is often not very desirous of giving information; it may give him trouble, and that kind of thing. As a matter of fact, any person reading the history of that "St. Nicholas" epidemic cannot come to any conclusion but that it was the milk of the goats that caused it, and because there are hiatuses in the story that does not, so far as I can see, weaken that evidence. If you take 30 monkeys, and give them all one drink of milk containing these micrococci, and the great majority of them take Malta fever, I think that is pretty conclusive. A judgment is formed, as a rule, not from one fact but from several. There are a series of arguments that come and affect your mind, and in the end you have a certain opinion. My opinion is absolutely fixed that the goats' milk gives Malta fever, as I think I have made clear to the Commission.

14405. Am I right in saying that recently, in the "Times" of September 12th, some attempt was made to pluck the laurels from your crown in regard to this discovery?—There was a letter written by a retired officer of our own corps, but he is a well-known gentleman in our corps, and I was the unfortunate means of breaking him about two years ago. I was sent out to Barbadoes, where things were not going on well, and the result of my inquiry was that he was put on the sick list, and sent home, and left the service; so that anything which he would say about me I should say must be received with caution.

14406. Was not the claim put forward by Colonel Fitzpatrick that Captain Hughes was the discoverer?—Yes.

14407. Yes. I only wanted to give you an opportunity of stating it. The claim made on behalf of

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Captain Hughes was that he had priority in regard to the discovery which you have announced to the Commission of the goats' milk as the cause of Mediterranean fever. Is that not so?—Yes.

14408. To what did Captain Hughes attribute the Malta fever? He worked greatly at it and published a work on the subject?—Yes, he published a large work on the subject. He attributed it to effluvia from drains.

14409. I think his words were: "A specific micrococcus emanating during hot weather from a saprophytic existence in soil polluted with the fæces and urine of those suffering from the same disease"?—Yes, that is the danger of a scientific man, instead of using experiments, going in for theory and speculation.

14410. Do you say that Captain Hughes did not make experiments on animals?—He made a few—some one or two experiments, and he did not do much good.

14411. Should I be wrong in saying that Captain Hughes made many experiments on animals?—I do not know that he made many; I think he only made a few.

14412. Have you seen his book?—I knew him very well. He was a pupil of mine at Netley.

14413. He is no longer living?—No. Captain Hughes was a great friend of mine, and I speak with great respect of him; but he had not that particular stamp of mind which we call the scientific mind. He was not able as it were to distinguish very clearly between speculation and fact. As a matter of fact, his book, which is a large book, does not contain a single sentence which advances our knowledge of this particular disease. He was very industrious and a very nice fellow indeed, but he had not that particular thing which the gods give a man to enable him to do scientific work—investigation work—advancement of knowledge work.

14414. Did Dr. Allan McFadyean possess that rare gift?—I do not know his work very well.

14415. Did he not fall a victim to inoculation in the laboratory with typhoid and Malta fever?—He is said to have died of Malta fever on account of experimenting with it in the laboratory.

14416. Was any serum used in his case?—I do not know. He had been working, of course, for years at typhoid inoculation.

14417. At the Lister Institute?—Yes.

14418. In regard to nagana, do I rightly understand that in your opinion the trypanosome is transferred by the tsetse fly from wild animals to domesticated animals?—It must be so, I think.

14419. And that in wild animals it appears to be non-pathogenic?—So far as we know it is non-pathogenic.

14420. But in domesticated animals it is pathogenic?—In certain domesticated animals.

14421. And in man is it pathogenic?—I have never seen a case in man.

14422. That would point, would it not, to the great importance of the influence of soil upon the seed that you implant in it?—By soil you mean the human blood and tissues?

14423. Yes?—If you put it in that way, of course, but you can only put these things boldly. One animal is susceptible to the disease, and another animal is not. The house mouse is susceptible to anthrax, the field mouse is not. The two animals look very much the same; with any chemical analysis or microscopical examination you cannot see any difference between the blood and tissues of the two animals, but one is susceptible and the other is not. It is a part of Nature that is too hidden for us to be able to examine with our limited intelligence.

14424. Then as regards sleeping sickness, I think you mentioned that before the discovery of the trypanosome, a claim had been set up for a streptococcus as the cause?—Yes, by the Portuguese, and by the first Royal Society's Commission.

14425. That was very stoutly supported at one time, was it not?—I never believed in it.

14426. Did not Castellani support it?—I was a Fellow of the Royal Society and a member of the Sleeping Sickness Committee when his paper came home giving this coccus as the cause, and I said to the committee that there was absolutely nothing in the paper which amounted to anything like proof. It wanted animal experimentation. I wondered why he had not been able to give this sleeping sickness, by means of his particular coccus, to some animal. He showed no signs of ever having tried to do so.

14427. Were no experiments performed by the Portuguese Commission?—I think so, but I must say that I have not read the Report of that Commission very thoroughly, because it is written in Portuguese.

14428. And did they point to this streptococcus as the cause?—Yes. Castellani also pointed to it.

14429. And Castellani afterwards changed his mind, did he not?—He changed his mind after I wrote my paper.

14430. No doubt the influence of the divine gift?—In that particular case I should say that it was great luck my having worked for two years at a similar disease. I gave Castellani the paper describing the observations made by us during the three weeks he remained in Uganda after my arrival. I gave him that paper to take with him to London, in order that he should not go back empty-handed. In the interests of scientific history he ought to have associated my name with his, or at least described in the text the part I took, but he did neither.

14431. You point out the importance of differentiating and avoiding fallacies. I am anxious to know whether, besides this streptococcus, there was not at one time another cause asserted very strongly, and held by a great many as the cause of sleeping sickness?—*Filaria perstans* was one.

14432. It was found in a negro in London?—Yes, I think so.

14433. And then found in many others?—I should say, judging from the natives that I had to deal with in Uganda, that if you looked sufficiently long you would find it in 100 per cent., in the same number as you find the trypanosome, showing the difficulty of distinguishing between two parasites. The one is evidently a harmless parasite, living in the blood of man, just as Nagana trypanosomes live in the blood of wild animals. Here you have a parasite, and 100 per cent. of the natives have it in their blood.

14434. Healthy natives?—No, many of these natives had sleeping sickness, and we used to find the *perstans* in them almost as regularly as the trypanosome.

14435. Was it also found in healthy natives, and in the Indians in America?—Yes, and it is found also in natives living outside the sleeping sickness areas, and in certain sleeping sickness areas where there was no *perstans* found, the trypanosome was found.

14436. Do you know Sir Patrick Manson's work on tropical diseases?—Yes.

14437. He says: "In dealing with newly-discovered parasites, our experience with *Filaria perstans* should serve as a warning against precipitancy in drawing conclusions from the mere fact of concurrence." Do you agree with that?—Certainly.

14438. And he says: "Moreover, what we already know about some African trypanosomes should also make us hesitate in definitely committing ourselves to a trypanosome as the cause of sleeping sickness." You do not agree with that?—Well, you see, I look upon Sir Patrick Manson as what we call an armchair naturalist. He lives here in London, and is very clever at reading papers and prophesying what is going to happen, but he has not the practical knowledge; he does not go and work at the thing practically on the spot; and if a man is going to write a text book he cannot expect every word in the text book to be true, or even to be fairly reasonably true. There must be a lot of padding and nonsense in a text book. A text book is the lowest form of literature that I know of.

14439. I take it that you do not agree with Sir Patrick Manson when he says: "Experimental evidence, therefore, so valuable in the settlement of such questions, has in this matter, to say the least of it, been far from conclusive." That is referring to sleeping sickness?—Is that five or six years ago?

14440. It is the year before last?—I was opening a discussion on sleeping sickness some three years ago in Oxford, at the meeting there of the British Medical Association, and I quoted Sir Patrick Manson on sleeping sickness, he having opened a discussion on the same question, I think, the year before. I put down all the conclusions that he had come to, and I said:

"Every conclusion of a year ago is now proved to be false except one, and that is that medicinal treatment has no effect." Unfortunately, that still remains true. All the other conclusions were absolutely wrong. I put down in the same way, under separate headings, some ten or twelve statements, and I asked the British Medical Association to bring these up a few years later, and find out how many of them remained true. I think at the present time all mine remain true.

14441. How long ago was that?—1904, three years ago.

14442. What about the medical treatment? Does not Koch claim that a subcutaneous injection of arsenic is beneficial?—Yes, but Robert Koch is getting an old man, and he is much frequented by interviewers, and it is very difficult to get an interviewer to repeat exactly what you say.

14443. You do not accept the crocodile?—No, I do not, and I do not think Koch himself does. He has never himself said that atoxyl will cure. He said that he had every faith in it, and so he gave the lay mind to understand that he was curing them by the hundred, but I have never believed it, and I do not think that any person who knows the subject has believed it; and I think he has gone back to Berlin very disappointed that the atoxyl treatment has been useless.

14444. You think that attention should be directed rather to destroying the haunts of the tsetse fly than to an onslaught on the crocodile?—That is the broad way. I think myself that in a place like Uganda, especially after the population has been so reduced, if the Government and the chiefs made a big effort, they could take the whole of the natives out of the sleeping sickness areas, and put them in healthy places, and if they could do that, theoretically in forty-eight hours there would be no more sleeping sickness; there would be no fresh cases of sleeping sickness in Uganda. The tsetse fly only carries infection for forty-eight hours, and if you could remove the reservoir of the disease, in forty-eight hours every Glossina palpalis in Uganda would be harmless, and if you did not expose affected people to the tsetse fly again, then sleeping sickness would be blotted out by a wave of the magician's wand sort of business.

14445. If you were made plenipotentiary there would be no fear of its spreading in East Africa or down the Congo or the Nile; is that so?—It is mainly a question of money. If you know the distribution of the fly, and you know the natural history of the disease, you can do a great deal. The great thing in all these cases is to know the natural history from beginning to end, and not till then to try to do anything. You require to keep the people away from the sleeping sickness areas until the healthy-looking ones have developed the disease; you require to keep them away in the interior of the country. Those who are affected by sleeping sickness must, of course, never go back to the fly area. If at the end of forty-eight hours you sent back the population to the lake shore you would not be able to distinguish between the healthy and the unhealthy; you would therefore require to keep the sleeping sickness area population away from the tsetse fly for several months.

14446. (*Mr. Ram.*) You must give the fly perfectly healthy food for forty-eight hours?—You must give it either healthy food or none at all. At the end of forty-eight hours it is harmless.

14447. Those whom it had infected might again go on and develop the disease afterwards?—Certainly, after, let us say, six months.

14448. Affording unwholesome food again at the end of the six months?—Yes; you require to keep the place clear for some time, but I think at the present time it is not out of practical politics to keep the fly area of Uganda clear of population for a certain time. The Uganda natives are docile, and their chiefs are intelligent, energetic people, so that I would not look upon it now as outside practical politics to stamp out sleeping sickness in Uganda.

14449. (*Sir Mackenzie Chalmers.*) Does Sir Patrick Manson still hold the opinion that sleeping sickness is not due to a trypanosome, or has he altered his mind in any way, do you know?—I should say he has altered his mind.

14450. At any rate, his opinion at that time was one of scepticism. He thought it might be due to a trypanosome, or it might be due to other causes. Was that it?—It is impossible for me to say.

14451. You do not know how far he had studied the subject at the time when he gave out these dicta?—I do not know.

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14452. May I take it generally that, in your opinion, without experiments on animals you could not have discovered the cause of Malta fever, and that without experiments on animals it would have been a mere chance if you had attributed Malta fever to the milk of the goat. Would you go so far as that, or not?—Yes. As I said before, it would be an accident. Two epidemiologists were sent out to Malta to work at it, from the statistical standpoint, but they both failed. Perhaps, if a third had attacked the question, the third might have succeeded. But I say that the other way is the proper and reasonable way.

14453. It is not only the proper way, but it is the way that you pursued, and it was by means of animal experimentation that you came to these conclusions as to the cause of Malta fever?—Certainly.

14454. I am not sure that we have them, but can you give us the figures at all for 1907. Do you know what the effect of stopping the milk has been in 1907?—Yes, certainly. I put these figures down here, because it is very striking. I saw the Director-General of the Navy a few days ago, and he told me that they had not had a single case in the Navy this year.

14455. What about the soldiers?—Among the soldiers we had two cases in January, two in February, one in March, one in April, one in May, two in June, none in July or August, and only one in September; that is ten cases. These are the official figures, but I have received private information from Malta showing that at least 3 of these of these are readmissions; deducting these, therefore, there have been 7 cases this year.

14456. Can you give me the total for the same months in the year before?—From January to September, 1906, there were 151 cases.

14457. Then take from January to July, 1905 and 1906—I should like to get the three years. Perhaps you can give it later on?—From January to September, 1905, there were 538 cases among the soldiers alone.

14458. Can you also give us the corresponding months for 1905?—I have not got that here, but will supply it. No change has taken place in the incidence of this fever among the native population in Malta this year; it is not a particular year.

14459. The soldiers and sailors having been deprived of goats' milk, you have this curious fall in the incidence of Malta fever?—Yes.

14460. There have been a few cases amongst the sailors in 1907?—Yes, there have been 10 cases, or, deducting readmissions, 7.

14461. But there have been 7 or 8 cases tabulated as Malta fever?—Yes.

14462. It is always possible, I suppose, that those soldiers in the town may have access to goats' milk?—It is impossible to prevent their getting it, because in those small public-houses soldiers very often have egg-flip, a glass of milk with some brandy in it. It is rather a common thing if they are out late at night.

14463. Apart from stopping the supply of goats' milk to them as soldiers, have they been forbidden in any way to touch milk in Malta?—They have been lectured a great deal on the subject.

14464. Do you yourself attribute the few cases that have happened this year to the probability of milk infection?—Certainly. Some of the cases which have occurred this year are distinctly returned as being due to the drinking of milk.

14465. There are conceivably, I suppose, other ways? You say, for instance, that the micrococcus is excreted with the urine?—Yes.

14466. Is it possible that there could be contamination in that way; for instance, in the case of drying urine, you might get it through the air?—We held that theory for some time rather strongly.

14467. But you have no evidence in support of it?—We could not get any evidence in support of it—not in nature. If you grossly contaminate dust with the *micrococcus melitensis*, and blow it about a room in which a monkey is living, you will give the monkey the disease; but the Commission never could give the disease by blowing dust gathered from natural positions, say, in the neighbourhood of urinals, or from fever wards, or places which might be contaminated. Major Horrocks, R.A.M.C., a member of the Commission, made many experiments on these lines. He

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never got any evidence in favour of the dust theory except under artificial conditions. And naturally if you take billions of micrococci, and mix them with a small quantity of dust, it is like feeding an animal; it gets its tongue all covered and its food all covered; it is like a feeding experiment; so I do not think infection by dust takes place once in 1,000 times. We cannot tell, of course, but there is no proof that dust causes the disease.

14468. So that, so far as you know, the disease is communicated purely by food, and not by inhalation?—Purely by food.

14469. Possibly you inhale the micrococcus, and it gets destroyed in the lungs?—If you inhale sufficient then that would be a feeding experiment, because it sticks in the mucous membrane of the nose and mouth and is swallowed. But no doubt if the micrococci reached the lungs they would be absorbed into the circulation and not destroyed.

14470. But before the disease germinates it has to get to the alimentary canal?—No, it gets from the alimentary canal into the blood, and it grows in the tissues and especially in the spleen. There is no evidence that it grows when it is in the alimentary canal.

14471. Where does it start?—It is such a small thing that it may be absorbed by the mucous membrane of the tonsils. In my opinion, it may be absorbed by any mucous membrane.

14472. It starts into the blood, you say?—The moment it gets into the lymphatic system it passes, probably, from there to the blood, and from there to the different organs, settles down in the spleen, and grows there, and gives rise to a certain poison, and so on.

14473. May we take it generally that if remedies are to be found for those diseases experiments must be made on animals before they are made on man?—Certainly.

14474. For instance, when a toxyl was suggested as a remedy, was it tried first on animals or first on man?—You ought to have Professor Cushny here to answer that. He is a pharmacologist. I do not know the history of drugs in that way.

14475. You have not studied these tropical diseases with reference to medicinal remedies yourself?—I have done a certain amount. I tried, for example, arsenic and other drugs on nagana when I was in Zululand, and I tried without success many drugs on Malta fever. I did not try any medicinal treatment on sleeping sickness, because I was only in Uganda seven months.

14476. You were investigating merely the cause, and not the treatment?—Yes. The Commission afterwards, of course, did try different remedies, especially, I think, Captain Grieg, I.M.S., Captain Gray, R.A.M.C., and Lieut. Pollock, R.A.M.C.

14477. Were you a member of any Plague Commission?—I am a member of the Plague Committee of the India Office at present.

14478. Have you at all considered the Reports of that Commission as to the transmission of plague by rat fleas?—Yes.

14479. And in your opinion is it established that the rat flea is the ordinary mode of transmission of plague from animal to man, and from man to man?—Yes, that is my opinion distinctly. The ultimate proof, of course, the giving it to man, cannot be made, but except for that every step in the proof has been taken.

14480. Have those conclusions been arrived at by experiments with animals?—Absolutely.

14481. Could you have arrived at those conclusions without experiments with animals?—Certainly not.

14482. And, so far, by means of that knowledge, are we put on the right lines in the way of prevention of plague?—Certainly.

14483. (*Mr. Ram.*) If I gathered aright from you you discovered this *micrococcus melitensis* in 1888?—1886. I think the report was published in 1887 for the first time.

14484. There was no Commission until 1906?—1904. It lasted for about three years.

14485. Was anything being done in all these years to attempt to find out the cause of Malta fever or to stop it?—Captain Hughes worked from about 1893 to 1897, and I should say that he worked at the fever, but was unsuccessful in discovering the true cause; and, as you can see from the statistics, nothing was done to practically limit the cases. That is to say, Malta fever cases were getting more numerous in the Army and Navy up to the year of the Commission, and it was on account of that probably that the Commission was formed.

14486. Is there any cows' milk at Malta?—Very little.

14487. There are some cows kept there, are there not?—Yes.

14488. Have any experiments been made with regard to their milk?—Yes.

14489. Has it been found to be free from the *micrococcus melitensis*.—No, there was a suspicion that some of the cattle were affected. I do not remember whether it was actually found in the milk, but there was some suspicion that some of the cattle were affected by Malta fever.

14490. You have told us that there has been almost an entire elimination of the disease from the Army and Navy now. Have you any statistics to show that the native population are still as subject to Malta fever as they were previously?—I have a letter from Dr. Zammit, who is a member of the Board of Health, and he informs me that the number of cases of Malta fever in the native population is as great this year as ever.

14491. The goat continues to be used for milk?—Yes, absolutely. The Government have been able to do very little yet. Malta is a very conservative place.

14492. The evidence now seems to be that the *micrococcus melitensis* is swallowed; that the contamination is given to man by swallowing the micrococcus?—The infection is taken by swallowing.

14493. And you have almost eliminated the idea of dust and that sort of thing being the cause?—Yes, I think so, or mosquitos. The mosquito theory was also strongly held for some time, but we could not get any proof of the mosquito being the carrier.

14494. You were asked one or two questions about sera. You have not done much with sera yourself?—No, very little.

14495. As I understand, when you found animals useful, and, indeed, necessary, was when you were anxious to discover what the particular micrococcus was that was the cause of this disease. And was it by experiments upon animals that you were able to discover it, to the elimination of other micrococci that might be present?—What I meant was, that in investigating a disease, the important thing is to be able to state definitely that you are quite sure that a particular organism is the cause of the disease, and it is often very difficult to be absolutely able to say this unless you have an opportunity of animal experimentation.

14496. When you find that a particular organism does produce certain symptoms, and is followed invariably by certain symptoms, you arrive at the conclusion that that is the cause of that particular disease, and in order to test it you want to try it on different animals to see the result of your different experiments. Is that where the use of animals comes in?—Yes, that is one use.

14497. That is one. Further, when the disease has been established, and you are anxious to know the cause of the disease, do you then inoculate animals with the blood or other secretion of a person known to be diseased in order to see the effect upon that animal?—I do not think, as a matter of practical work, that that is very often done. You mean for purely diagnostic purposes?

14498. I want to get it accurately from you, if you can say exactly where it is that experiments on animals come in to be useful. I think I have got one instance that you explained in your examination-in-chief very accurately. I am not sure that I follow the second one as accurately. You spoke about taking the serum from some infected patient, and inoculating animals with it. Why did you inoculate these animals; to see what?—I do not remember that I did say that I took sera from any particular man and put it into an animal.

14499. I thought you said that from a good many natives you took their blood?—From wild animals in Nagana.

14500. But you took the blood of natives in sleeping sickness in different parts of the country, some parts where there was sleeping sickness and some parts where there was not?—I only examined it through the microscope.

14501. You did not experiment on animals?—No, that was purely microscopical examination; we found by microscopical examination of the cerebro-spinal fluid and the blood of sleeping sickness patients that practically in 100 per cent. we could find this particular parasite. We then took 200 people from a part of the country outside the sleeping sickness area, and examined their blood in the same way—not the cerebro-spinal fluid in that case, but only the blood, and we found it in none of them. If you will only consider for a moment, that is a very strong argument in favour of the trypanosome being the cause of sleeping sickness. That was the beginning of the work. You must remember that we were under the disadvantage of believing that this trypanosome was well known, and that it was the cause of a disease called trypanosoma fever, which was supposed to be quite distinct from sleeping sickness. On account of that, we had to work, as it were, against the difficulty of a preconceived notion—we had to get rid of that—and it was only by these experiments that we gradually lost the preconceived idea, and took on the new one.

14502. (*Sir Mackenzie Chalmers.*) Did you ever inoculate an animal with the blood of a person suffering from sleeping sickness and containing trypanosomes?—Yes, on many occasions. First, we had to find out if monkeys would take the disease.

14503. With the result that, having inoculated the monkey with the blood of a person containing trypanosomes, sleeping sickness was produced?—Yes, that was an experiment to help to prove that the parasite is the cause of the disease.

14504. (*Dr. Gaskell.*) Could you tell us at all how the Malta fever spreads, or the *micrococcus melitensis* spreads, from goat to goat?—No. We know that if we put into a small space a certain number of affected goats, and a certain number of healthy goats, after a certain time some of these healthy goats will be found to be affected by the disease by close contact of that kind. If you put a healthy monkey and an affected monkey together, so that they feed out of the same dish, and the urine may contaminate their food, if you keep the affected monkey and the healthy monkey together for some time, you find that the healthy monkey gets the disease; but if you keep them separate from each other, so that they can only touch each other, and the urine of one cannot get at the food of the other, then they do not take the disease.

14505. There is one question that I wanted you to make clear to the Commission, and that was that in these cases of bacterial infection, bacterial disease, one of the important points is to inject a particular bacillus or micrococcus into an animal in order to see that it gets that disease. Is not that so?—Yes.

14506. When you do that, do you not inject both the fluid substance and the bacteria—you inject both?—Yes.

14507. Then it is open, is it not, for people to say that the disease was caused by the fluid substance rather than by the bacillus?—That is the reason why we try to cultivate the bacilli outside the body for several generations on a neutral substance.

14508. You also mentioned another point, I think, which I want to bring out, and that was the Chamberlain or Pasteur filter?—Yes, that is merely to separate the solid particles from the fluid.

14509. Does it effectually separate the microbe from the fluid in which the microbe is contained?—As a rule, yes. You can take a fluid containing millions or billions of bacteria—say typhoid bacilli—and filter that fluid through a Chamberlain filter, and not a single bacillus will come through. The fluid that comes through will be absolutely sterile.

14510. And what is the effect of injecting that fluid that comes through into an animal?—It may give rise to a temporary rise of temperature. There may be a little poison in it, there may be a few toxins of the bacteria in the fluid, and that would give rise to a temporary rise of temperature and a feeling of seediness, but it would not give rise to the disease itself.

14511. Whereas, injecting the microbes, on the other side of the filter, would give rise to the disease?—Yes, if the animal is susceptible.

14512. I think it has been tried again and again, and we may take it as proved, may we?—Yes, I think you may take it as proved. There are some toxins, of course, so powerful that a little filtrate without any bacteria will kill animals, but it kills them as strychnine kills them.

14513. It would not necessarily give the kind of disease you were investigating?—No. Tetanus, for example, is a very powerful poison in that way.

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14514. In these cases of sleeping sickness in monkeys, and also in man, do you think there is much pain when the animal is suffering from that sickness?—I think man may suffer at first to some extent from headache and from a feeling of uneasiness, but I do not think he ever feels much pain. I think in a very few cases, say 1 per cent., acute symptoms of mania may come on on account of the poison affecting the brain, and that, I should say, was very uncomfortable. I mean to say that the patient is in a constant state of restlessness, talking, laughing and throwing himself about, and sleepless, but I do not think there is any pain. With regard to monkeys, I do not think there is any pain at all, at least so far as one can see.

14515. They always go into a lethargic condition?—They get into an absolutely lethargic condition; they feel nothing.

14516. Do they waste very much?—No; man does not waste as a rule; he remains quite plump to the end.

14517. We have had a picture shown us here in the Commission of a dog that was infected with sleeping sickness, with the trypanosome, at Khartoum. It appeared in a very emaciated condition, and it was suggested that this dog had been in terrible pain in consequence of the disease. I was wondering whether you could say that that was likely or not to be the case. There was emaciation there undoubtedly?—I do not think that sleeping sickness trypanosome would cause emaciation in a dog, and I am quite sure, from all I have seen of dogs inoculated with the sleeping sickness, that they did not suffer at all. They are very refractory to the disease. But was it certain that this dog had sleeping sickness? I should imagine that it was one of the other trypanosome diseases.

14518. (*Dr. Wilson.*) You admit that this disease, Malta fever, is very widely distributed?—Yes.

14519. And you contend that its mode of propagation is entirely through drinking goats' milk?—Yes, in Malta we think it is; probably in 999 cases out of a thousand. These are only figures which are not worth anything, of course, merely figurative; but we feel sure that most cases of Malta fever are caused by drinking goats' milk, but we cannot assert that for the rest of the world. The evidence up to the present time is that where there is Malta fever there is a grave suspicion of goats being at the bottom of it—in the Punjaub and in South Africa.

14520. Do you refer to Maltese goats entirely?—I do not think that the Maltese goat has found its way to the Punjaub. I think there is certainly a suspicion that the Maltese goat finds its way to South Africa, because if they want to improve the breed of goat you send to Malta for some of the Maltese goats.

14521. Then you would only put this as the probable mode of propagation—not a certain mode. You would not say that it is the absolute sole mode of propagation?—In Malta I should say that it was almost the only mode of propagation. I cannot speak for places that I do not know anything about. I have a strong suspicion that the goat is mixed up with it everywhere.

14522. But is not this microbial fever, because you call it a microbial fever, very singular in this respect, that you have the micro-organism excreted by an apparently healthy animal, and it produces all these febrile symptoms when taken in food?—By man do you mean?

14523. Yes?—Yes. There is nothing very singular about it, because the one animal is less susceptible than the other. A goat does not suffer from fever and inconvenience by taking this thing inside it. Man does. But the micrococcus can live in the goat, and, curiously enough, it finds its way after a time into the mammary glands. It is killed out in the spleen, killed out in the kidneys, killed out in the liver, killed out in the blood; but it gets into the mammary glands, and remains there and multiplies for years.

14524. But is the mammary gland in a diseased condition; have you found it so at all?—I am now thinking of tubercle bacilli in cows milk. In the Maltese goat you have a very highly developed mammary gland, developed beyond all nature, and it is, as it were, the place of least resistance. There must be some reason for it, but I put it broadly, only that here you have a gland developed out of all proportion, a gland that almost touches the ground when the animal walks

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along—a huge over-developed gland. You can imagine that an over-developed gland of that sort would have tissues of less resistance than ordinary normal sized and normal glands. Therefore it is that in the Maltese goat the *micrococcus melitensis* is killed out of all the other organs, and remains in the mammary gland chronic, and established, so far as we can see, for years.

14525. Do the glands and teats show any signs of disease?—No, you cannot tell by examining a goat that is has Malta fever.

14526. So that they entirely differ from the tuberculous glands in the cow, for example?—Yes. The tubercle, of course, forms nodules, and the *micrococcus melitensis* does not form any nodules; it does not form any tumour formation, as it were, because the little nodules of tubercle are like little tumours.

14527. When you were out in South Africa, had you opportunities for personally inquiring into the effects of anti-typhoid inoculation during the South African War?—I have had a good deal to do with it. I wrote a report upon the subject for the Government which caused anti-typhoid inoculation to be stopped for the time being, because I did not think it was being done sufficiently carefully, and with sufficient forethought.

14528. Do you think it is of value as a preventive?—We—that is, the Royal Army Medical Corps, and especially Colonel Leishman, Captains Harrison, Grattan and Kennedy—are working at it just now, and we are getting good results, but I would not like to say at present whether it will be found of great practical use in practice. We had a very good example the other day (I do not know whether you know of it, but it is a beautiful example) in the 17th Lancers. Every regiment going out to an enteric area is carefully inoculated before it leaves this country, inoculated twice, with this anti-typhoid fluid. The 17th Lancers were inoculated, went straight out to India, and fell straightway into an epidemic of enteric. There were 63 cases in that regiment; 62 of these occurred among the non-inoculated, and only one among the inoculated, and that one case had only been inoculated once instead of twice—the man refused to have the second inoculation. These men were all living together under the same conditions, with the same food, and the same everything, and yet that was the result. That, of course, is a beautiful result. But then you see the men were inoculated twice a very short time before they became exposed to the poison, and we do not know yet how long this immunity will last. There is no doubt that there is a certain amount of immunity attained in plague, cholera, and typhoid by injecting the dead bacilli under the skin. It imitates the disease to a certain extent. The disease of typhoid fever is caused by the typhoid bacillus, and the fever and general effect are caused by the poisons excreted by the bacilli, and also held up in the bodies of the bacilli. So that if you insert 50 millions or so of these bacilli under the skin, and they all dissolve up, that man gets a dose of typhoid poison just as he would if he had typhoid fever, only in a smaller extent. Therefore it seems to me quite natural that these inoculations of dead bacilli should bring about a certain amount of immunity.

14529. Has not Strong, while working in the Philippines, stated that he does not believe in the dead bacilli, and has taken to inject living bacilli?—That was in plague. If you heat typhoid bacilli to 65 degrees Centigrade in order to kill them you destroy the toxin that causes the immunity; you therefore kill them at as low a temperature as possible, 53 degrees, or thereabouts, and then do not get the same effect as with the living bacillus. Haffkine used the living cholera bacillus when he started. And certainly if we could use the living typhoid bacillus it would be better, but he would be a bold man who would start anti-typhoid inoculation in the British army at the present moment with living typhoid bacilli.

THIRTY-THIRD DAY.

Wednesday, 6th November 1907.

MEMBERS PRESENT:

The Right Hon. the Viscount Selby (*Chairman*).

Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir J. McFadyean, M.B.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. G. Wilson, M.D., LL.D.
Captain C. Bigham, C.M.G. (*Secretary*).

Mr. George Granville Bantock, M.D., F.R.C.S.Ed., called in; and Examined.

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14530. (*Chairman.*) You are a Doctor of Medicine and a Fellow of the Royal College of Surgeons Edinburgh?—Yes.

14531. And you practice in London as a consulting surgeon?—That is so.

14532. And you are surgeon to the Samaritan Free Hospital for Women?—Consulting surgeon now.

14533. And a Founder and Fellow of the British Gynecological Society?—That is so.

14534. And a Fellow of the Obstetrical and other societies at home and abroad?—Yes.

14535. And you have practised a great deal and written a good deal, more particularly on abdominal surgery?—I have.

14536. And you have been asked to give evidence here by the chairman of the Parliamentary Anti-Vivisection Association?—Yes.

14537. I believe you are not a member of the Society?—I do not belong to any society.

14538. But you hold some views which affect the question of experiments on living animals which are well known, and that is the reason why you have been asked to come, I believe?—I think it is more particularly because of my views on bacteriology that I have been asked to give evidence, because I have never taken any active part in the question of vivisection, either for or against.

14539. I only said that you held some views which bore upon the question of experiments on animals?—It has some bearing.

14540. I understand your general view to be—I am going to take you through your evidence, of course—that bacteriologists are mistaken in the view that a great number of the most important diseases are proved to be due to the presence of a particular bacillus in the body?—I have a strong belief in that respect.

14541. And it would follow, I presume, that in your view all experiments upon animals made for the purpose of ascertaining and identifying the particular bacillus are useless?—For identifying the bacillus they may not be useless, that is to say, the bacillus may be characteristic of a particular condition; but my contention is that it is not the cause.

14542. You do not deny that it exists?—No, and it may be diagnostic of the condition, but I say that the bacillus is never the cause. My contention is that the bacillus is an accident of the disease—that is to say, it results from the disease, that the conditions are favourable for its development; for we are swarming with bacilli of various kinds, and it only wants certain conditions to favour the development of certain bacilli. That is the view I hold.

14543. I presume that if your view were adopted by physiologists generally they would be obliged to come to the opinion that a great many experiments which they at present make would be unnecessary?—I think so.

14544. Therefore I am only pointing out how in my view, and I fancy in yours, your view about bacteriology affects the question which we have to consider, which is as to how far experiments on animals are justifiable?—Undoubtedly.

14545-6. You are prepared to state to the Commission your views generally upon this subject?—Quite so. Like a good many other surgeons who had constantly to perform serious and delicate operations, I took a very keen interest in the system of what was called antiseptic surgery, which was introduced by the present Lord Lister some thirty-five years ago, and like them I tried it in my practice. The system was founded on the hypothesis that germs floating in the atmosphere fell into wounds, there developed and multiplied and produced all the evil effects which sometimes followed surgical operations. In order to combat these lamentable results this system, when it was fully exploited, included operating under an antiseptic spray,

and treating all instruments, ligatures and dressings on antiseptic lines. In the numerous operations which I had to perform, I soon found that the antiseptic (carbolic acid) spray had an injurious effect on the delicate lining membrane of the abdomen and other tissues, and I gradually began to reduce the strength of the solution until at last I used only ordinary London water to wash out or irrigate wounds, and finally depended on clean hands, instruments, ligatures and dressings, without the aid of antiseptics. In other words, I, in company with a very few other surgeons, dispensed with so-called antiseptics altogether as not only unnecessary but injurious, and I may be permitted to say that I took a not inconsiderable part in the somewhat acrimonious warfare which was aroused in the attack on Listerism by reading a paper on "Hyperpyrexia (or excessive rise of temperature) after Listerian Ovariotomy." In the course of my cross-examination I should like to elaborate that point, namely the results of my reading that paper at a society in London. As is well known, what is called antiseptic surgery, or Listerism, was ultimately discarded, and the system which was eventually adopted was the aseptic, which is the absolute negation of the antiseptic system. But aseptic surgery, which may be defined as clean surgery, is certainly not the outcome of bacteriological research, as many scientists and most people still blindly believe; but a commonsense revolt against the illogical details of surgical practice to which bacteriology gave rise. At the International Medical Congress, held in Berlin in 1891, Lord Lister acknowledged his error with a frankness which all must admire, though some, like myself, have difficulty in understanding the tenacity with which he has all along clung to the modern doctrine of bacteriology. In his address to that Congress Lord (then Sir Joseph) Lister spoke as follows in reference to surgical details in my own practice and in that of the late Mr. Lawson Tait. "Dr. Bantock, whose remarkable series of successful ovariotomies may seem to justify his practice does not, I believe, prepare his ligatures antiseptically. The success achieved by Bantock and Tait without, it is said, the use of antiseptic means, proves a stumbling block to some minds." No doubt, so long as they hold to the germ theory. "I can see that while the measures" (comprehended under the term cleanliness) "to which I have referred are, so far as they go, highly valuable, it must be in itself a very desirable thing to avoid the direct application to the peritoneum of strong and irritating antiseptic solutions." This latter is in itself a strong justification of my abandonment of carbolic acid. He continues: "As regards the spray, I feel ashamed that I should ever have recommended it for the purpose of destroying the microbes in the air. If we watch the formation of the spray and observe how its narrow initial cone expands as it advances with fresh portions of air continually drawn into its vortex, we see that many of the microbes in it, having only just come under its influence, cannot possibly have been deprived of their vitality. Yet there was a time when I assumed that such was the case, and trusting the spray implicitly, as an atmosphere free from living organisms, omitted various precautions which I had before supposed to be essential." He then describes how in a case of operation "the air passed freely in and out of the pleural cavity in a cloud of spray," and he arrives at the conclusion that "it is physically impossible that the microbes in such air can have been, in any way whatsoever, affected by their momentary presence in the air." "If then," he continues, "no harm resulted from the admission day after day of abundant atmospheric organisms to mingle unaltered with the serum in the pleural cavity, it seems to follow logically that the floating particles of the air may be disregarded in our surgical work, and if so we may dispense with antiseptic washing and irritation, provided always that we can trust ourselves and our assistants to avoid the introduction into the wound of septic defilement from other than atmospheric sources." What these sources are we learn from his address at Liverpool, in September, 1896, six years later. "'Hence I was led to conclude that it was the grosser forms of septic mischief rather than microbes in the attenuated condition in which they existed in the atmosphere that we had to dread in surgical practice." Here I may be permitted in passing to give expression to my admiration of the character of the man who can confess his error with such candour and honesty, seeing that such a confession of error must detract from the credence we should otherwise give to his later views. Would that his disciples were like minded! To proceed: What then are the "grosser forms of septic mischief"? "If," in the words of the late Dr. Campbell Black, "they are what is vulgarly called 'dirt,' then we are all agreed that to remove dirt (not, however, by killing it) and to keep wounds clean is perfectly scientific and proper treatment." What is this but the doctrine of "cleanliness," which I have advocated for so many years? Thus it will be seen that it only required that Lord Lister should have taken one step more to fall into line with me. For while he gave up the theory of atmospheric germs, he admitted that we may dispense with antiseptic washing and irrigation, and virtually came to accept the principle of cleanliness—one of the two principles in the enunciation of which I played no unimportant part, and which are now generally accepted in the case of ovariotomy. So much for antiseptic and aseptic surgery; and if I have alluded to this phase of the subject at some length, it is because I wish to dispel the belief, which is so persistently advanced, that aseptic surgery is the outcome of bacteriological research. Bacteriologists have long since discovered that in order to convert filth or dead organic matter of any kind into harmless constituents, Nature employs micro-organisms or microbes as her indispensable agents. Thus, in the modern septic tank, which is now so largely used in the treatment of sewage, it is the action of the micro-organisms, whether ærobic or anærobic, which dissolves the sewage, and it is the continuous action of these microbes which converts all manurial matter into the saline constituents which are essential for the nutrition of plant life. In the natural purification of filth-polluted streams, or in the conversion of dead animal or vegetable matter into the flora of the vegetable world, it is admitted that the micro-organism plays a beneficent part, and so I am prepared to contend that however these innumerable and infinitely minute vegetable organisms may be designated, they always play a more or less beneficent part when they are found to be associated with disease, and that, however characteristic any micro-organism may be of any particular form of infectious disease, it cannot be classed as pathogenic, in the sense that it is the actual agent causing the disease. The microbe in its relation to disease can only be regarded as a resultant or concomitant, and in that respect is of more or less value to the physician in assisting him to diagnose the form or kind of disease with which it is associated. But even in this respect there are abundant sources of error. For example, the bacteriologists themselves admit that there is frequently great difficulty in differentiating the tubercle bacillus from other bacilli belonging to allied groups or species, and the diphtheria or Loeffler's bacillus from the Hoffman or pseudo-bacillus. Again, the true Loeffler's bacillus is often found in healthy throats, and I may say also as a gynecologist that it is sometimes found in the generative passages of healthy women. On the other hand, in what may be termed to be undoubted cases of diphtheria (clinically) it is very often not found after continued and careful examination, and similar difficulties and uncertainties present themselves with regard to other so-called specific micro-organisms. Thus, the staphylococcus pyogenes and streplococcus pyogenes, which are supposed to cause suppuration, have been found by various observers in the vaginal discharges of healthy women, as also the bacillus coli communis and the bacillus typhosus. Dr. Stoker's well-known treatment of ulcerative conditions by oxygen gas incontestably proves that the stapbylococcus and streptococcus pyogenes were not only necessary for healthy granulation (or healing), but that any interference with them by means of germicides retarded the healing process. In several severe operations which have been performed by myself, and in which the healing process progressed satisfactorily, without any disquieting symptoms, I have had specimens of discharge examined by competent bacteriologists, and with one exception so-called pathogenic organisms were found to be present in all of them. Is it not, therefore, reasonable to conclude that these micro-organisms, while they may be necessary for processes of healing, are certainly not causative of disease? While it cannot be denied that there must be a virus or *materies morbi* to account for the spread of every communicable disease, I contend that another strong argument against their microbial origin is the fact that in respect to several of the most infectious

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diseases no specific micro-organism has yet been found, in spite of the most minute and persistent research. Thus, no specific or so-called pathogenic organism has been discovered in small-pox, vaccinia, measles, scarlet fever, whooping cough, or hydrophobia, and it seems very singular that the tetanus bacillus, which is believed to be the sole cause of tetanus or lockjaw should be found so abundantly in manured or garden soil, or on the surface of roads fouled here and there with horse-droppings. It is also worthy of note that in the disease for which microbal origin is claimed to have been most fully established, the pathogenic organism is usually associated with necrosis or death of tissue. Thus it is now generally admitted that the tubercle bacillus is not found in the sputum in the early stage of phthisis, but only when necrosis of the lung tissue sets in. The diphtheria bacillus, again, is associated with the septic or membranous throat, the typhoid bacillus with ulcerated intestine, the cholera vibrio with the inflamed intestinal canal, and the plague bacillus with the inflamed bubo or pneumonic lung. In all these instances is it not logical to conclude that these micro-organisms, instead of being pathogenic, are playing the part of nature's scavengers? On this point Dr. Vicentini says: "Considering the assimilating activity of bacteria on the one hand, and on the other their reducing activity at the expense of histological elements in decay, I have often asked myself whether this ubiquity of the bacterial elements is not, perhaps, destined to perform some important service, as, for instance, the expurgation of the fluids to act as scavengers in relation to decayed particles or cells of the body. The simple fact that bacteria are present in the products or morbid seats does not imply anything specific, so long as their totally extraneous origin and their quite independent entity be satisfactorily proved. Losing sight of this truth, as simple as it is essential, one risks falling into strange exaggerations, and is likely to include the most varied complaint (even the ingrowing nail, according to Regnault) in the list of bacteriological infections." In order to illustrate more fully my contention that so-called specific diseases have not a microbal origin, I will now deal with two of the diseases already named in which no specific micro-organism has been discovered—namely, small-pox and vaccinia—and two in which a specific organism is persistently paraded as the actual *causa causans* of the disease—namely, tuberculosis and diphtheria. With regard to the first two, I may here fitly quote from my pamphlet, to which I referred at the outset: "Investigators have, all the while, overlooked the fact that these diseases are due to a specific material poison which can be handled, and can be conveyed, from one subject to another, by inoculation. There is no mystery about this as there is about the natural mode of transmission—or should I not rather say propagation?—of these diseases, or of such diseases as typhus, typhoid, measles, scarlatina, etc. Take the case of small-pox, for instance. We know that if we insert an almost infinitesimal quantity of the contents of a small-pox vesicle, at the proper stage, under the skin of a perfectly healthy subject, an enormous multiplication of the poison takes place, as exhibited in the innumerable vesicles which appear on the surface of the body, each vesicle filled with a fluid possessing the same virulent properties as the original vesicle—in other words, an attack of small-pox, presumably under the influence of what we, in our ignorance, call a chemico-vital process. That this extraordinary state of things should be due to the action of a special form of bacillus passes one's comprehension and is incredible. It is not a mere local action, such as is attributed to the staphylococcus or streptococcus, when either of them is said to produce suppuration. It affects the whole constitution of the subject, and reminds one very forcibly of the effects produced by snake or other animal poison—which we know to be not the product of a bacillus, but of an organ specially constructed to that end. Vaccinia, under the same process, produces such a change in the constitution of the individual subjected to it, that, in at least a majority of cases and for an indefinite period of time, it renders that subject proof against the inoculation of itself and even of small-pox. And here I may be allowed to express the hope that my views will not meet with a "conscientious objection," but be examined dispassionately. What is the nature of this extraordinary change which is wrought in the constitution of an individual by an attack of small-pox, vaccinia, or other of the eruptive fevers, such as measles, scarlatina, etc., and which, in a large majority of cases, renders the subject insusceptible of a second attack? It is all a matter of conjecture, and, so far as I know, no one has offered even a plausible explanation. Is it a change in the actual constitution of the individual, or does it merely affect the nervous system only? Of the other two diseases I take the case of diphtheria first. This disease is said to be due to the influence of a specific bacillus—Loeffler's. How does it happen that this bacillus can be found in the throat of a subject weeks—yea, even months—after all trace of the disease has disappeared? This doctrine has suffered much discredit of late from the fact that this bacillus of Loeffler's is frequently present in some exanthemata (rash), and also in healthy persons. A still "more striking example is afforded in cases of tonsillotomy, wherein, upon the incised surface, a greyish membrane is formed, in which the bacilli abound without constitutional disturbance or any sign of diphtheria." I anticipate the argument that, if you allow some of the discharge from the throat of a subject of this disease to gain access to that of a presumably healthy individual, you *may*, but not necessarily, produce the disease in him. And you may point to a number of cases in which medical men, in the fulfilment of what they conceived to be their duty, have sacrificed their lives in the heroic attempt to succour their patients—as, for instance, in the course of the operation of tracheotomy. But the answer to this is the very valid one that you do not convey the bacilli only. You also convey the fluid in which they are bathed, in which I contend they live, and which, in my opinion, constitutes the real essence of the disease. As the vesicle in the case of variola (small-pox) and vaccinia is the outward manifestation of the disease and the contained fluid its essence, so in the case of diphtheria the condition of the throat is the outward manifestation, and the fluid oozing from its surface the essential element. Many observers of eminence and authority in this field concur in denying the connection of the Loeffler bacillus with diphtheria as cause and effect. I am bound to accept as matter of fact the statements made as to the association, even in a majority of cases, of the Loeffler bacillus with diphtheria, for they are not questioned; but to reverse the proposition, and say that their presence is the result of the disease, appears to me to be the more sound reasoning. The preceding argument appears to me to be unanswerable, and no one, as far as I know, has attempted to reply to it. But it will not be amiss to fortify it with some more *facts*. Thus Scharoz, writing in the "Deutsche Medicalische Woche," says: "There are certain cases which are clinically diphtheria, but in which the bacillus cannot be found." "It seems probable that widely distributed saprophytes, such as the bacillus, cannot alone be the cause of diphtheria." He quotes two cases in which diphtheria bacilli, so called, were obtained from wounds of the eye after cataract operations, in neither of which infection occurred. In a paper by Drs. Hewlett and Montagu Murray, published in the "British Medical Journal" on June 15th, 1901, the authors begin by saying that the purpose of their communication is to illustrate "the frequency with which children whose throats are presumably healthy may serve as sources of diphtheria," and, referring to appended tables, they continue: "It will be seen that the figures support two statements which are commonly made. The first statement is that the Klebs-Loeffler bacillus and pseudo-diphtheria bacillus exist in the throat in a large number of cases, especially in children, without producing any recognisable symptoms Out of 385 children under the age admitted for operation, or for some illness other than diphtheria, only 235 were free from both kinds of bacteria. In 92, or 24 per cent., the pseudo-diphtheria bacillus was found; in 58, or 15 per cent., the Klebs-Loeffler. Neglecting for the moment the presence of the pseudo-diphtheria bacillus, the pathogenic power of which is uncertain, these figures suggest that one out of every seven children, or at least one out of every seven sick children, is a possible source of diphtheria infection." They admit the possibility that "in some cases bacilli may have been present, but yet have escaped detection" (thereby underestimating the proportions), and "in view of this possibility it is interesting to note that in only seven of the cases was there any clinical evidence in favour of diphtheria, and that on the first examination in three of these cases no Klebs-Loeffler bacillus or pseudo-diphtheria bacillus was found in the throat." And one of "the morals deducible" from these figures "is that it is clear that babies and young children are not the harmless and innocent (*sic*) creatures usually imagined, and that kissing and other artificial

(*sic*) demonstrations of childish (*sic*) affection should be discouraged." I can imagine the effect such a recommendation by the family doctor would have upon a mother. Then they give "some of the results obtained by other observers with regard to the occurrence of the Klebs-Loeffler and pseudo-diphtheria bacilli in the throats of persons with no sign of diphtheria"; thus "Loeffler examined 160 healthy school children, of whom four showed diphtheria bacilli in their throats." "Park and Beebe, out of 330, 250 are described as children, healthy throats examined, in eight, virulent Klebs-Loeffler bacilli were present." "Rober has made an extensive series of observations upon the presence of diphtheria baccilli in the healthy throat. There were 128 persons examined, who had been in contact with cases of diphtheria, and 600 who had, not knowingly, been exposed to the disease. Of the 128 persons, the diphtheria bacillus was found in ten, a percentage of eight, and of the 600 persons it was found in fifteen, or 2.5 per cent." Under these circumstances what ground there is for regarding the pathogenic power of the one bacillus as certain, and that of the other as uncertain, does not appear. Coming now to tuberculosis, I will not dwell on Koch's "tuberculin fiasco," which was hailed by the profession as a panacea for tuberculosis, but it should be borne in mind that this fluid of Koch's was intended to kill the bacilli, but it was soon found that, in too many instances, it had the very opposite effect—of killing the patients—twenty-seven or twenty-eight in Berlin alone, according to Virchow, in the course of two or three months. It was shown that the injection of this fluid into a healthy subject produced little or no effect, but into a tuberculous subject it produced, in a few hours, what was called, a violent "reaction," and that this reaction was, in many cases, attended with the production of a fresh crop of tubercles, especially affecting the serous and mucous membranes. Virchow, speaking of these new eruptions after the injection of Koch's fluid, and resting on the assumption that "all tubercles are produced by bacilli," says of one particular case of pulmonary tuberculosis in which "the so-called pericardium" was affected, that "there was no alternative but to suppose that the germs had reached the place by way of metastasis. How could we help thinking of metastatic processes here, and conjecturing whether, in fact, the bacilli had not been mobilised, and whether they had not been diffused *through the body* by a process of infection? And since, as you know, Dr. Koch himself considers the bacilli to be sufficiently refractory to his remedy—*and we have not found that they are destroyed*—the possibility must not be overlooked," that, "by a process of softening, whereby the products of disintegration are rendered more fluid" this metastasis takes place. Does not this lend countenance to the doctrine I hold—viz., that the injection of the fluid is only so much added to the amount of the specific poison with which the system is impregnated, and that hence the bacilli are found in all parts of the body, following their food, as fish find their way into the overflow in cases of flood, or advance and recede with the tide? The acute symptoms which follow the injection of this fluid point clearly in this direction, as does their subsidence, so soon after, on the exhaustion of the supply, provided the dose be not a lethal one.

14547. Could you tell me what metastatic processes are?—The transference of disease from one part of the body to another. What, then, should be the remedy? This, viz., not to endeavour to destroy the bacilli, but to maintain the vital forces and processes at their highest state of efficiency, by providing such as are already possessed by, or are predisposed to, tuberculosis, with pure air, abundant light, nutritious food, and, in a word, all the conditions that tend to the maintenance of good health. This is actually the method now in vogue. But not because, as has been put into the mouth of the Marquess of Londonderry at the opening of a hospital for consumption, "the best germicide to kill off the microbe was to provide air, light, and sunshine," for we now know that these conditions favour their growth. I call attention to the fact that many cases of so-called tubercular peritonitis have been cured by opening the abdomen, and removing the fluid contained therein. I specially direct attention to the case of a young subject, aged sixteen, on whom I operated on March 25th, 1895. In making the incision through the thickened parietes, and just below the umbilicus, I cut through several deposits of cheesy matter, and exposed a brain-like tumour, several pounds in weight, involving the omentum. As it was impossible to entertain any idea of removing the tumour, I sponged out about a pint of fluid lying in the lower part of the abdominal cavity, and then closed the wound, two or three of the sutures involving these cheesy masses. The wound healed by first intention. Previous to the operation the patient was much emaciated, her hair was falling off, and she suffered from a peculiar form of diarrhœa with very offensive evacuations. I put her on an absolute milk diet, not sterilised, until the evacuations assumed a healthy character, and improvement set in so rapidly that she returned home on the 26th day. She is now in perfect health, well nourished, and with an abundant crop of hair, and all trace of the tumour and of the deposit in the parietes has disappeared. That this was an example of tubercular disease I have no doubt whatever, if we are to put our trust in macroscopical appearances and the attendant symptoms. Will some bacteriologist be good enough to explain these facts on the basis of the germ theory? It is a remarkable fact that this young lady scarcely ever tasted milk. Had she been in the habit of taking milk, being the daughter of a farmer, it might have been contended that she had been infected in this way. But we are now assured that the cow is at least one of the chief sources of the spread of this disease by means of its milk. In his Harben lectures on "Consumption," the late Sir Richard Thorne-Thorne laid great stress on this aspect of the question. He stated that while, in the adult, tuberculosis had diminished 50 per cent. in the last quarter of a century, it still claimed a very large number of victims amongst children in the form of tabes mesenterica, and he attributed this prevalence to the fact that children are fed so much on milk. Now, if we look at the facts of the case, we find that tabes mesenterica occurs chiefly, if not wholly, amongst the children of the poor and indigent, and that it is extremely rare amongst the children of the well-to-do. The latter have an abundant supply of milk, and other nutritious food, while the former chiefly drink tea, and scarcely know the colour, much less the taste, of milk in their homes. Lest it should be thought that I am drawing upon my imagination, I quote from a paper read by Mr. R. Henry Rew before the Royal Statistical Society, on April 26th, 1892. After quoting from Morton, in his book, "The Diary of the Farm," 1885, that the consumption of milk "has very greatly increased of late years," Mr. Rew says: "I have been able to obtain some figures showing the average daily delivery of milk per family by retailers in a few fairly typical localities. From these I have calculated the consumption per head on the basis of five persons per family." He then gives the following table:—

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STATISTICS OF MILK CONSUMPTION taken from the Books of Milk Retailers in various Districts.

No.	Description of District.	Approximate Number of Families Served.	Average Consumption per day, per Family.	Average Consumption per head.	
				Per Day.	Per Annum.
			Pints.	Pints.	Gallons.
1.	West-End of London	4,000	3·750	0·750	34·218
2.	North of London	2,000	3·250	0·650	29·656
3.	Manchester		2·666	0·533	24·318
4.	„ (Middle-class District)	146	1·500	0·300	13·687
5.	„ (Working-class District)	200	1·000	0·200	9·125
6.	Small Country Town	90	1·624	0.325	14·823
7.	Putney and Wandsworth	700	2·286	0·457	20·838
8.	Small Town (Health Resort)	500	2·000	0·600	27·375
9.	East End of London (one gallon of Milk divided into 37 portions		0·432	0·086	3·923

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I should just like to point out that amongst the well-to-do, the average consumption per head per day is in the West End of London 0.750 pints, or 34.218 gallons a year, whereas in the East End of London the average consumption per annum is 3.923 gallons.

14548. When it says that in the West End of London 4,000 families were served, does that, and do the following figures relating to the West End of London mean that those are the figures given by a particular trader in the West End of London?—I go on to explain that in the following paragraph. "With reference to No. 9 (the East End of London) it is necessary to explain that the figures are based on the fact that a gallon of milk is divided on an average into thirty-seven portions, each portion being, I believe, fetched by the consumer. I have assumed that each family consumes one of these portions every morning and evening. It will be seen that even on this assumption the consumption per head is infinitesimal, and it reveals, incidentally, a significant fact in connection with the dietary of the poor. I may, perhaps, add that the gentleman who was good enough to procure me that particular return is one of the highest authorities on the London milk trade."

14549. That did not seem to me quite to explain it. Does he put together 4,000 families supplied by a number of West End dairymen?—Yes, of course, the returns have been obtained from different milk dealers of families supplied by a number of dairymen, not by one dairyman.

14550. They are such round figures—north of London, 2,000; working-class district, 200; small town, 500—that they do not seem to be actual figures. However, you do not know about it?—I do not. This is a quotation from Mr. Henry Rew's book; he has given this table. Again tabes mesenterica is essentially a disease of mal-nutrition, and is often cured by nutritious diet, of which milk should form an essential part with cod-liver oil and some preparation of iron. And why does this form of the disease attack the digestive tract of children rather than the pulmonary? Because, says the lecturer (Sir Richard Thorne-Thorne), the tubercle bacillus gains entrance through the stomach. I venture to affirm that not a single case has ever been put on record in which the disease has been thus communicated. In this connection I may quote Dr. F. M. Sandwith, who has said that in Egypt women suckled their children for even two years, and all other milk was boiled, and among the cattle tubercle was rare, yet tuberculosis was common. His experience, moreover, had been that tubercular injections gave bad results in surgical tuberculosis. With regard to the tubercle bacillus of Koch, Professor Spina opposed the theory on the ground that if this bacillus were the true cause of tuberculosis the bacillus ought to precede the formation of tubercular nodules. Spina, Charrin, and Kuskow failed to find this bacillus in acute miliary tuberculosis, the last-named affirming that acute miliary tuberculosis does not at all proceed from the bacili of Koch's, and that such bacili only appear there second-hand, proceeding from pre-existing tubercular nodules. Professor Middendorp is of the same opinion, and in a letter to Dr. Vicentini says "the bacilli only accompany pulmonary phthisis in caverns communicating with the bronchi. . . . These microbes, therefore, cannot be called tubercular bacilli, as they are not found in tubercles as such, and have no etimological relation whatever with tuberculosis. For this reason I maintain that tubercular bacilli, in the sense of Professor Robert Koch, do not exist at all." With regard to plague, Mr. Hankin, Chemical Examiner and Bacteriologist for the North-West Provinces and Oudh, reported to the Plague Commission in January, 1899, that there was no doubt that cases of plague occurred among human beings in which no microbes were visible at the time of death. Ridicule seems to be the appropriate weapon to use against the theory that plague is conveyed to the human subject by rats through the agency of fleas! There was a time when the bacillus coli communis was regarded as a most virulent microbe, and by many it is still regarded as such, and yet it is now recognised by the best authorities as a normal inhabitant of the digestive tract, and necessary for the maintenance of health. In the case of typhoid fever, I have shown in the paper already referred to that in not a single instance in which the outbreak has been attributed to water has the bacillus typhosus been found in the incriminated water. I claim, then, to have shown that the poisons of variola, vaccinia, and several other acute infectious diseases, are not and cannot be the product of a bacillus; that Loeffler's bacillus is not a constant, and therefore cannot be the essential, element in the production of an attack of diphtheria; that the essential element in the case of typhoid fever is not the bacillus typhosus; that this bacillus cannot live but for a few hours in ordinary sewage; that not a single specimen of this bacillus has ever been discovered in sewer air, and hence that typhoid fever cannot be attributed to it because of its contained germs; that in the cases of the epidemics at Maidstone and King's Lynn there exists no proof of contamination of the water by typhoidal matter, as indicated by the presence of the bacillus typhosus; that there is no evidence worthy of the name that tuberculosis is due to the ravages of the tubercle bacillus; that the comma bacillus cannot be regarded as the essential element in the production of an attack of cholera, and that the same can be said of plague and its special bacillus; that the so-called pathogenic micro-organisms are constantly found under conditions consistent with perfect health, and that in more than one notable instance they not only appear to, but actually do, exert a beneficial influence. To all, then, who hold by the germ theory I would say, in the words of Oliver Cromwell, "I beseech you . . . . think it possible you may be mistaken."

14551. (*Sir William Church.*) Referring to the first page of your *précis*, eight lines from the bottom, am I to understand that only germs floating in the air were supposed to be dangerous when Lord Lister founded his system of antiseptic surgery, and that those which had settled upon other bodies were not dangerous?—That is not included. The Listerian system was founded on the theory that germs floating in the atmosphere fell into the wounds, and to counteract their influence the spray was used.

14552. But only those floating in the atmosphere?—Quite so. That was the basis of the Listerian method.

14553. What is your ground for stating that?—Professor Lister's statement.

14554. It is not Lord Lister's own statement?—Undoubtedly.

14555. Can you tell me where he states that germs only that are in the air are dangerous, and not those which have settled?—I think if you will just look over what Lord Lister himself said at Berlin you will find that that was the basis of the theory—the germs floating in the atmosphere.

14556. But are you not aware that Lord Lister, at the very time that he was investigating this question, was most particular with regard to the cleanliness of the skin both of the patient and of himself, and also of all the instruments that he used?—Sir Joseph Lister, as he was at that time, actually stated that he did not even aim at surgical cleanliness. He stated that at King's College Hospital after he arrived in London. And you see here what he says: "As regards the spray I feel ashamed that I should ever have recommended it for the purpose of destroying microbes in the air."

14557. Quite so, but that was an additional precaution, was it not?—That was the essence of the treatment, and he used the carbolic acid as a germicide.

14558. Before ever he used the spray what was his object in using germicides? Carbolic acid was what he used. What was his object in washing the skin, washing his own hands, washing the surfaces of the wounds, and keeping them dressed with carbolic acid?—I am not aware that he did it before he introduced the spray. In fact, I think that the use of carbolic acid and the use of the spray were simultaneous, and I believe the idea was founded upon the supposed effect of carbolic acid upon sewage matter—that the carbolic acid destroyed the microbes in sewage; whereas we now know that it does nothing of the kind. I believe that gave Lord Lister the idea of using carbolic acid for the purpose of destroying microbes in the air. Lord Lister seems to have had no idea of destroying microbes in the skin, which were already in the skin, and of which perhaps he was not aware at that time.

14559. But he states himself the necessity of cleansing the skin of the patient and his own hands and the instruments that he uses?—With carbolic acid.

14560. Yes?—Which he believed was a germicide, but now we know is not a germicide.

14561. Do you mean to tell the Commission that if you use carbolic acid in sufficient quantities it does not destroy the bulk of known germs?—Certainly

it does, along with the tissues of the body to which it is applied. You cannot destroy the germ without destroying the tissues of the body, that is to say, that the germs will live in a solution of carbolic acid which will be destructive of tissues.

14562. Quite apart from the body, if you please, do you mean to say that it will not destroy germs in a solution in water or, in what you just now said, in sewage, if you used sufficient?—It will not kill them all; it may destroy a certain number, but carbolic acid to destroy germs must be used in such strength that it will destroy living tissue.

14563. That is not the point I am on at the present moment. You stated that it would not destroy germs in sewage?—That it would not destroy them all; that is to say it did not deprive the sewage of its septic matters; you could not apply it in sufficient quantity, and the practice, as you know, has been given up. The treatment of sewage by carbolic acid has been proved to be an absolute failure, and the very opposite method now is adopted of cultivating the microbes to eat up the sewage matter, not to kill it, but to eat it up.

14564. (*Chairman.*) You say in the second page of your statement that it was in 1891 that Lord Lister acknowledged his error?—Quite so.

14565. What is the date you fix for the introduction of the antiseptic treatment first by Lord Lister?—I really forget the date, but it was many years ago.

14566. You speak of his introducing the spray and the use of carbolic acid at the same time?—That was when he was at Glasgow.

14567. Is that what you call the beginning of the system?—That was the beginning.

14568. When was that?—I could not give you the exact date, but it must have been in the early sixties, if not in the fifties.

14569. As far back as 1860, at any rate?—Yes, undoubtedly.

14570. (*Dr. Wilson.*) You say about 35 years in your *précis*?—Yes; it would be quite easy to find the exact date when he introduced it at Glasgow.

14571. (*Sir William Church.*) Upon page 2 of your *précis* you say that you gradually began to reduce the strength of the solution until at last you used only ordinary London water to wash out or irrigate wounds, and you also, I think, mentioned Mr. Lawson Tait as doing the same thing?—Lord Lister refers to Mr. Lawson Tait.

14572. Did you use the water straight out of the tap?—Straight out of the tap.

14573. Without boiling it?—Without boiling it.

14574. Then you differ from Mr. Lawson Tait?—I do not think so. I think Mr. Lawson Tait did not always boil his water. I do not think he took any trouble in that respect.

14575. Do you think that boiling water sterilises it or not?—Sterilises of what?

14576. Sterilise the water—kills the germs in it?—You have to prove first that there are germs in the water before you can sterilise it or kill them.

14577. You know that Mr. Lawson Tait, although he did not mention it in his paper, did boil the water?—I do not think he did; he did not boil all the water, anyhow.

14578. And you never did?—Never. I had to use hot water, and if the water was too hot I added some cold water to it. I should like to explain my reason for my giving up the carbolic acid treatment. I lost a patient through acute inflammation of the kidneys as the result of carbolic acid poisoning; it was a case of ovariotomy.

14579. I think it is admitted that carbolic acid poisoning was frequent?—I was going to explain how I arrived at my views. A fortnight later I had another case in which the temperature went up to 106 within a few hours after the operation. I had to put the patient in a pack, and got the temperature down, and then I recognised that the symptoms were due to carbolic acid absorption, and I made up my mind to reduce the strength of the carbolic acid. I went from 1 in 50 to 1 in 60. I still got evidence of the absorption of carbolic acid by chemical test. Then I reduced it to 1 in 80; the evidence got less and less. Then I reduced it to 1 in 100, and then, as Lord Lister told us, that a strength of 1 in 40 was the weakest solution that was of any service, I thought when I had arrived at 1 in 100 that the experiment was at an end. And from that time my results began to improve. I do not know whether I am correct, but I think I am correct in saying that there is no surgeon in this country who has performed so many successive successful ovariotomies as myself in a public hospital, for at the Samaritan Hospital I have a record of 90 successive successful ovariotomies without the use of any antiseptic whatever. I think I was justified in every respect in leaving off the antiseptic treatment.

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14580. You seem to place in contradistinction to each other the antiseptic treatment and the aseptic treatment?—Yes.

14581. But are they not one and the same?—Not at all. Antiseptic means the use of a substance with a view of preventing septic mischief, or destroying microbes. It is quite correct to say that you have done an antiseptic operation, but it is absolute nonsense to say that you have done an aseptic operation, because you do not know what the result may be. The patient may die of septic mischief within 48 hours.

14582. But surely is not the distinction between the two, if there is any (I do not admit that there is), that the antiseptic treatment was attempting to destroy germs which had got access to the body, and the aseptic is preventing their access?—Attempting to prevent the action of the germs was the idea of the antiseptic method.

14583. What do you understand by surgical cleanliness?—The use of nothing, and the absolute exclusion of dirt of any kind.

14584. But who showed us wherein dirt was harmful? How was that knowledge arrived at?—Dirt may contain specific poisonous matter apart from germs altogether.

14585. And may not specific poisonous matter be in the air, too, and so get on to the dirt?—I do not think it is possible that that specific poison can be in a dry air.

14586. How do you think that what you call dirt, I do not know quite what that is, becomes poisonous and dangerous?—Dirt may contain, as I have just said, some poisonous matter, whatever it may be. We do not know, and we have no means of knowing, what that poisonous matter may be. Even, you may say, you may find dirt free from microbes, and yet it will poison a patient, poison the wound.

14587. What is your authority for saying that?—My own observation.

14588. How do you know that the dirt had not germs in it?—I say it may have.

14589. I understand you to say that dirt quite free from germs is poisonous?—No, I say it may be free. You may not be able to find any germs in a particular portion of dirt, and yet that dirt will be poisonous to the wound.

14590. Would you give me your authority for that?—I have no authority beyond my own experience and observation. I do not think that any observations have been made on the subject.

14591. And are no organisms of any kind obtainable from that dirt?—You seem to misunderstand me. I have not said that dirt has been found free from germs, but I say that it may be free from germs and yet be poisonous.

14592. How do you explain what used to be so common in hospitals—hospital gangrene?—Shall I relate to you an experience of many years ago? When I first came to London, before I began practice, I went into one of the general hospitals of London, where I saw a very distinguished surgeon go up to a case on which an operation had been performed for hernia. He put his little finger into the wound, a suppurating wound, and he took a towel and just wiped it like that (*describing*). He went on to see other patients, and coming to one other patient of the same kind he ran his finger into the wound and did the same thing again. That is what I call dirty surgery; there was no attempt at cleanliness there.

14593. What do you think is the dangerous element in it?—Transferring the fluid from a suppurating wound to another wound.

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14594. So you did not believe in the possibility of contamination of wounds or the carrying of infection through means of the atmosphere?—Certainly not.

14595. Lord Selby wishes to know what would be the difference between a surgeon who acted antiseptically and a surgeon who acted aseptically. Did not Lord Lister himself introduce the term aseptic?—I do not think so.

14596. I think he did?—I think Lord Lister had ceased to operate by the time that that term was introduced.

14597. I think he introduced it himself, and he was very pleased to find afterwards that it was a word used by Hippocrates?—I was not aware of that, but I think it is immaterial even if it be so.

14598. (*Chairman.*) What I wanted you to do, if you could, was to describe to me and to others who do not understand these matters professionally, what a surgeon would do if he were performing a particular operation antiseptically, and how otherwise would he act if he were performing it aseptically?—In performing an antiseptic operation the surgeon would first of all wash his hands in some so-called antiseptic or germicide such as corrosive sublimate, carbolic acid, spirits of wine, or other things. A very elaborate process was invented by Dr. Howard Kelly, of Baltimore. He first of all washed his hands in a strong solution of permanganate of potash.

14599. I do not want every precise detail, of course, from you?—It was a process for the purpose of destroying bacilli or bacteria in the skin. And it has been found that that is an impossibility, because many of these microbes are too deeply seated——

14600. Instead of arguing upon each thing he does, will you just tell us what in fact he does?—Then he would have his instruments in a solution of corrosive sublimate or carbolic acid. He would then wash the skin, probably with germicide. In an operation upon the abdomen in which he would use a sponge, or any operation in which he would use a sponge, the sponge would be soaked or washed in a solution of carbolic acid or some other germicide. The aseptic surgeon uses no germicide to the patient except it may be to the skin. He does not allow the antiseptic to touch a wound or raw surface.

14601. Does he wash his hands?—Yes, he washes his hands.

14602. In the same sort of solution?—Yes, but he does not allow any germicide to touch the wound.

14603. (*Mr. Ram.*) Does he sterilise the instruments?—He may either sterilise the instruments or keep them in a solution of corrosive sublimate, or carbolic acid, or such like.

14604. (*Sir Mackenzie Chalmers.*) What does he do that for?—His idea is to destroy the germs.

14605. Is it effective in sterilising the instruments in the hands?—It is not effective in sterilising the hands or sterilising the skin.

14606. (*Chairman.*) I did not want to go into the question whether it was right or wrong, but I wanted to get first what actually was done. I do not gather from what you have told us so far that there is any difference between the two methods except that in aseptic treatment certain things are not applied to the skin of the patient?—And the tissues of the patient, that is the most important thing.

14607. Is that the difference?—Practically.

14608. (*Sir William Church.*) Did you in your practice make use of sterilised dressings?—No, I used the dressings that I got from the surgical instrument makers or the man who supplies these things. The gauze I used directly as I got it from the surgical instrument maker.

14609. You did not take any precautions to sterilise your wool, or your gauze, or your bandages?—None whatever. And I am not afraid of exposing a wound to the open air. I have operated on the breast and applied no dressing whatever. Any case where I could not draw the edges of the skin together I have kept exposed to the open air, and it has healed up perfectly.

14610. All surgeons used to do that at one time. They had to leave the wound exposed if they could not get a skin covering?—They did not leave it exposed; they put some dressing upon it, some ointment or something of that kind. But I purposely in a public hospital did that as an experiment, and everybody knows that the best method of treating a raw surface, if you graze your skin, for instance, is to let the blood dry upon it—do not apply any dressing to it, and it heals up forthwith.

14611. The answer that you gave me was what I wanted; that you in your own practice made no use of sterilised dressings at all?—Absolutely none.

14612. You used absorbent wool, or anything of that sort, as it came from the maker?—Yes, quite so, absolutely. That I have done now for the last 30 years, I should think.

14613. On page 4 of your *précis*, towards the bottom of the page, you say: "Thus in the modern septic tank, which is now so largely used in the treatment of sewage, it is the action of the micro-organisms, whether aerobic or anaerobic, which dissolves the sewage, and it is the continuous action of these microbes which converts all manurial matter into the saline constituents which are essential for the nutrition of plant life." Do you mean that plants do not suffer from parasitic organisms?—Undoubtedly they do. At least my observation in a greenhouse is of this nature, that it is only the unhealthy plants on which you find the aphides.

14614. By parasites I did not mean aphides, I meant micro-organisms?—I do not know that they suffer from micro-organisms. They suffer from visible organisms such as I mention, but I do not know that they suffer in any other way.

14615. Not from microscopical fungi?—Yes, from fungi; but fungi are not microbes, I believe.

14616. Am I mistaken or not in thinking that if you filter the fluid of small-pox vesicles it does not convey the disease?—I am not aware of any experiments of that kind. I know, as everybody knows, that if you take an infinitesimal quantity of the contents of a vesicle and put it under the skin you get an attack of small-pox.

14617. But you were not aware—I think I am correct—that filtered vaccine fluid does not convey the disease?—I am not aware of any observations in that direction.

14618. If that is correct it looks as if the disease was dependent upon some particular matter?—But you know that you do not prove anything. You do not prove the exact cause.

14619. Something must be left behind in the filter?—You prove that there is something, but you do not know what that something is. And it certainly is not a microbe.

14620. You say so?—No one has found the microbe yet, anyhow.

14621. I admit that it has not been found?—And you have no right to assume that it is the result of the action of a microbe.

14622. You also say on page 6 "it is now generally admitted that the tubercle bacillus is not found in the sputum in the early stage of phthisis, but only when necrosis of the lung tissue sets in." What is your ground for saying "it is now generally admitted." What are your authorities for that?—I have referred to several authorities, and I think that is the result of the reference to those authorities, that no one has yet discocered the tubercle bacillus in the early stage of the miliary tuberculosis.

14623. Is that so?—That is so, I believe. If you can refer me to any author or any experimenter who has discovered it I should be very pleased to know who he is, but, so far as my reading goes, the result is that no one has yet discovered the tubercle bacillus in the early stage of miliary tuberculosis.

14624. You mean before tubercles are found?—In the early stage of miliary tuberculosis.

14625. I wish to know what that is?—Miliary tuberculosis can be found in a very early stage. I do not know what the microscopical appearances might be; you know more about that than I do, but I take the word of the eminent men to whom I refer.

14626. That is just what I am asking. Who are your authorities that you quoted?—I have quoted some here. I do not know whether I am correct in using the word "generally," but I mean that it is extensively admitted.

14627. I will put my question in another way. Do you mean by the early stage of miliary tuberculosis a

stage before you can see tubercles with the naked eye?—I do not know whether the term "early stages of tuberculosis" is confined to that stage in which it is not possible to see them by the naked eye, but they can be seen by the microscope.

14628. I wished to know what you meant by the early stages. But you do not maintain that when tubercles sufficiently large to be seen by the naked eye are found you do not find the bacillus in them?—I do not know whether the bacillus is found; I cannot say anything about that; but in a still earlier stage the bacillus is not to be found. When the tubercle is large enough to be seen with the naked eye it must be considerably advanced.

14629. I wanted to know what you meant by the early stages?—Yes, that is what I meant.

14630. You say that they are not early stages when the tubercles are visible to the naked eye?—I should say not by any means.

14631. Might I ask who Dr. Vicentini is?—Dr. Vicentini is an Italian doctor, living at Chieti, in Italy. He is a very good English scholar, and he reads and writes English perfectly, and when my paper on the "Modern Doctrine of Bacteriology," or the germ theory of disease, was first published, he sent his pamphlet to me. He had seen mine and read it, and was struck by the similarity of the views which he and I held, and sent me a copy of his pamphlet. I am sorry that I have not been able to bring it down with me, because in changing houses unfortunately I have mislaid it, but these quotations in inverted commas from Vicentini are from that pamphlet.

14632. Is he a skilled bacteriologist?—Evidently, yes.

14633. What has he published, could you tell me?—Unfortunately I did not put down the name of the pamphlet; but he has written a work on whooping cough and the micro-organisms associated with it.

14634. And when did he publish it, if you could tell me?—My paper was read in 1899, and Dr. Vicentini sent me his paper the same year; my pamphlet was published in the early part of the year.

14635. His work was published before 1899?—It must have been, as he sent me the printed pamphlet.

14636. I do not quite follow what you say at the bottom of page 7 of your *précis*. Why is it incomprehensible and incredible that smallpox can spread through the action of a special form of bacillus? Why should it not spread through a bacillus just as much as through a poison?—There is no condition that I know of in which such a state of things has been found or has been suggested even.

14637. But we do not know very much about it, and it seems to me no more incomprehensible or incredible that its spread should be due to bacillus than that it should be due to the poison?—To my mind it is utterly incomprehensible. I am giving my own opinion.

14638. You have given the reason, that it is something that is incomprehensible to your mind?—Yes, we do not know what the action of smallpox poison is in the human subject or the animal subject.

14639. Turning to page 9 of your *précis*, you say that this bacillus of Loeffler's is frequently present in some exanthemata, and also in healthy persons. Are you referring to Dr. Hewlett's paper on that?—Not alone, but the literature upon the subject is so full of that idea that I did not think it necessary to refer to any particular writer.

14640. Then what is your authority for saying that in cases of tonsillotomy the Loeffler bacillus is present?—I did not say that it is present, but that it has been found in cases of tonsillotomy.

14641. You say "A still more striking example is afforded in cases of tonsillotomy, wherein, upon the incised surface, a greyish membrane is formed, in which the bacilli abound without constitutional disturbance or any sign of diphtheria"?—I think you will find that I gave my authority for that.

14642. A greyish membrane is an exceedingly common thing after an operation for tonsillotomy; but apparently we do not get diphtheria with it. What is your authority for saying that "Loeffler's bacilli abound without constitutional disturbance or any sign of diphtheria"? You may have diphtheria after tonsillotomy too?—You may have; but I think I gave my authority for that statement. You see that is a quotation, "A still more striking example."

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14643. I want to know what your authority is and where you are quoting from?—I think Scharoz is the authority for that, but it is a quotation, as you see. You will find his name at the top of page 10. That is my authority for quoting it from the "Deutsche Medicalische Woche."

14644. Have you any evidence of fluids from these throats producing diphtheria without the presence of the bacillus?—No. I have no evidence; but there is the evidence that in a great many cases of true clinical diphtheria the bacillus is not to be found, and then again in a perfectly healthy throat the bacillus is to be found without any of the symptoms.

14645. But here you state "if you allow some of the discharge from the throat of a subject of this disease to gain access to that of a presumably healthy individual you may" produce the disease?—Yes, you may, but not necessarily.

14646. Have you any evidence of the disease being produced in that way?—I think you will find the answer to that in the following sentence, if you read it:—"You may point to a number of cases in which medical men in the fulfilment of what they conceived to be their duty have sacrificed their lives in the heroic attempt to succour their patients."

14647. Then they got the bacillus too?—There is no evidence that there was a bacillus. We know that in many cases of clinical diphtheria the bacillus is not to be found. You have no right to assume that in those cases where death has resulted from getting some fluid into the throat in the operation of tracheotomy the bacillus was present.

14648. But you have no right to assume that it was absent?—No. Then we are on an equal footing. You have no right to be top sawyer over me in that case.

14649. I thought you illustrated this as showing that it was the fluid that was the dangerous part?—I say that it is the fluid which is the dangerous part because the fluid is always there, but the bacillus is not always there.

14650. That leads me to ask another question. You say that there have been numerous cases of what you call "clinical diphtheria" in which the bacillus has not been found. What do you mean by "clinical diphtheria"?—Where you have all the symptoms characteristic of diphtheria.

14651. And followed by the after-results of diphtheria?—It does not matter what the ultimate termination of the case may be. What you have to determine is, is this a true case of diphtheria clinically or not? The patient may die or may recover. We do not require to refer to the after-results.

14652. Then you merely mean to state that it is (what everybody will allow) a very difficult question to decide whether the patient is suffering from diphtheria or not?—I imagine that there is some considerable difficulty, and I believe that many cases of sore throat are returned as cases of diphtheria at the present day.

14653. But to finish the clinical picture, can you bring before us any cases of what you say clinically were diphtheria which were followed by the ordinary sequels of diphtheria, that is to say paralysis, etc.?—I am not prepared to bring forward any cases on my own account, but I refer to the general literature on the subject, which is quite sufficient for me.

14654. Then what you mean by "clinically diphtheria" is a case in which there may be a difficulty in forming an opinion from the appearance of the throat and symptoms of the patient, whether it is diphtheria or not?—No, that does not represent my views, I mean by a case which is clinically diphtheria, that it must present the characteristic symptoms of the disease, whatever you may consider the characteristic symptoms.

14655. Then if I choose to consider the presence of the bacillus as a characteristic of the disease, I am right in excluding those cases which have not that from the category of diphtheria?—But that does not correspond with practice; that is not practical. It may be your view, but it does not correspond with the observation of the profession generally.

14656. You have given me no reason for proving that clinically these cases are diphtheria. You cannot tell

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me whether these cases furnish in about equal numbers of the ordinary sequels of diphtheria as those in which the bacillus is present?—I do not think I am called upon to express myself definitely on that point. You cannot exclude a case of diphtheria because the patient does not get paralysis after it.

14657. I only wished to know whether you can give me any instance of these cases in which the symptoms occurred without the bacillus being present?—I cannot from my own practice, but from the literature of the subject.

14658. You state here distinctly that by the inoculation of bacilli: "You also convey the fluid in which they are bathed, in which I contend they live, and which, in my opinion, constitutes the real essence of the disease"?—Yes, that is the poison.

14659. Still you cannot bring any evidence before us that these cases which have contracted the disease from the fluid alone without the bacillus have any of the ordinary sequels of diphtheria?—Not from my own observation. I take it from the literature of the subject.

14660. Then might I ask you who the "many observers of eminence and authority in this field" are to whom you refer on the same page, who "concur in denying the connection of the Loeffler bacillus with diphtheria as cause and effect." Could you tell me some of them?—There again I am dependent upon the common literature of the profession on the subject. I have not put down all the names.

14661. You cannot give me the name of any man of eminence?—I cannot. But that is the result of my reading. It is impossible always to remember the names of the authors you read.

14662. I should like also to know what proof you have that if you reverse the proposition you are correct?—It is a matter of reasoning simply. It is not a question of proof. I say it appears to me to be the more sound reasoning. It is not a matter of proof at all; it is a matter of opinion.

14663. Passing on to the bottom of page 11, you say "Coming now to tuberculosis I will not dwell on Koch's 'tuberculin fiasco,' which was hailed by the profession as a panacea for tuberculosis." Was it not rather hailed by the public than by the profession?—No, the profession also.

14664. Was not Koch himself averse to it, and did he not have his hand forced to a great extent?—Not by the public alone, but by the profession. I remember perfectly well that there were some American gentlemen over here watching my practice at the Samaritan Hospital, and they expressed a very strong desire to go over to Berlin to get some of this tuberculin.

14665. You admit that he himself was not desirous of tuberculin being brought out then as a cure?—I believe it is correct to say that.

14666. His hand was forced, as you say, by the profession; I thought rather by the public?—It was not by the public. I think the profession had as much to do with it as the public.

14667. That is a matter of no importance; but you admit that Koch himself did not wish it—it was against his wishes?—I have heard so. I believe I have read so.

14668. Do not you think that light has been thrown upon the action, or I might even say the dangers, of tuberculin treatment, by what we know now as opsonins?—I do not know anything about that subject; that has recently come up, and I have not paid any attention to it.

14669. You have not followed Sir Almroth Wright's work?—No, not with any degree of attention.

14670. What is your authority for stating that the bacillus of tuberculosis is favoured in its growth by air, light and sunshine?—I think it is a fact that those conditions are favourable to its growth instead of to its destruction; that is to say, so long as it has nutritive material to live upon.

14671. Surely that is not generally held, is it?—I am afraid that I hold several views which are not generally held.

14672. That is your own view?—That is my own view, certainly.

14673. You are not aware of anything contradicting it?—No, nothing.

14674. You are not aware that many experiments have been made as to the effect of light, especially sunlight, on the tubercle bacillus?—When isolated from the conditions in which they live it is very likely, but I am not aware of any experiments which have been made upon the tubercle bacilli in the natural conditions under which they live.

14675. Of course, directly you have them under investigation they are not in their natural condition?—That is just it. No allowance is made for that.

14676. I admit that they are not in their natural condition; but still are you not aware that experiments done with them in captivity, if I may use the term, do not support that view?—But I do not see how they can support any other view.

14677. Simply that the more light, especially sunlight and air, that you expose them to, the more rapidly they die?—As soon as you isolate them from the natural conditions under which they live, then, of course, they are likely to die very soon.

14678. One other question with regard to this very remarkable case of your own that you bring before us. On the same page you say: "That this was an example of tubercular disease I have no doubt whatever if we are to put our trust in macroscopical appearances and the attendant symptoms." Then, as a matter of fact, the tubercle bacilli were not seen under the microscope in that case?—No.

14679. Therefore you have no evidence that they are present there?—No, certainly not, but that the disease was tuberculosis I have not the slightest doubt, and no one has a right to question my opinion upon that point.

14680. Pardon me, I think everyone has the right?—From the macroscopical appearances I think one can perfectly safely say that a case is tubercle or not tubercle. I think you would bind us too closely if you denied us that privilege.

14681. But I think you admitted that this organism called the tubercle bacillus was at all events associated with a certain disease?—Not necessarily. In many cases of phthisis, in the last stage of the disease, you do not find the bacillus in the sputum; that is a fact.

14682. But you find it in the body, do you not?—I do not know. I did not go so far as that.

14683. On page 15, about the middle of the page, in the second paragraph, you say: "I venture to affirm that not a single case," that is of tuberculosis, "has ever been put on record in which the disease has been thus communicated"?—Yes, I affirm that most confidently.

14684. You mean by that, I suppose, only in man?—I am speaking of the human subject, of course.

14685. But do you mean to say that no case has been put on record in which the disease has not been thus communicated to animals?—I have not said anything of that kind. If you will look at my *précis*, I am speaking of children. I am speaking on the subject of tabes mesenterica.

14686. You say "I venture to affirm that not a single case has ever been put on record in which the disease has been thus communicated"?—By means of milk.

14687. That is to man, I understand?—Quite so. I am not dealing with animals.

14688. Do you hold the same with regard to animals?—I have nothing to do with animals.

14689. Would it modify your opinion at all if you learnt that, for instance, monkeys which have received only a single feeding or so with tubercle bacillus have contracted tuberculosis?—I have nothing to do with that question. I have asked over and over again for the details of a single case in which tubercle or tuberculosis has been conveyed to a child or a human subject by means of milk. I get no answer.

14690. And you put out of account all experiments on animals?—Yes, absolutely.

14691. May I ask who Professor Spina is? I am afraid I do not know his name?—He is an eminent Continental observer. Judging from the name, I should say he was an Italian.

14692. Would you tell me what he has written and where he has published his works?—I think you will find that that is a quotation from Dr. Vicentini.

14693. I wish to be allowed to consult his works, to be referred to his works?—I must try and find that pamphlet of Vicentini's in which you will probably get the reference.

14694. Perhaps you will kindly send the Commission the information?—If I can find it I shall be very happy to do so.

14695. And Kuskow. Charrin's name I do know, but who is Kuskow; perhaps you will kindly send that name to us and reference to his works?—If I can find it I shall be very happy to do so. The work from which I have quoted by Dr. Vicentini is "Bacteria of the Sputum."

14696. And also if I might ask for Dr. Middendorp's work?—That also, I believe, I got from Vicentini.

14697. (*Dr. Gaskell.*) Of what nationality is Professor Middendorp?—German or Dutch. I think he is German.

14698. (*Sir William Church.*) I should like also to ask you why you say on page 16: "Ridicule seems to be the appropriate weapon to use against the theory that plague is conveyed to the human subject by rats through the agency of fleas"?—Yes, I think so.

14699. Why is ridicule the only appropriate weapon? Surely it is not?—I believe that one gentleman has given evidence here already on this question, and he said that one of the precautions which they took in avoiding the bite of the fleas was that they wore top-boots.

14700. (*Chairman.*) Who wore top-boots?—The experimenters, I think. I think if anything could be ridiculous that is ridiculous.

14701. (*Sir William Church.*) I do not remember it. I may not have been present. But why is ridicule the appropriate weapon to use against the theory that malaria is conveyed by mosquitoes?—No, not so much against the theory that malaria is conveyed by mosquitoes. But I contend that that does not explain the origin of the disease.

14702. We have no knowledge of the orgin of anything, have we?—I do not know, but the current idea is that malaria is produced and propagated by the agency of mosquitoes.

14703. Not produced—propagated by mosquitoes?—I will give you a case in point. In the Nile Valley there is a district of about 300 miles in length in which there are no inhabitants, yet it is pronounced to be one of the most deadly districts in the globe in consequence of malaria; but there are no inhabitants by whom the disease can be propagated by mosquitoes from one to another. That shows that we have not arrived at the origin of the disease; and it is well known that when you turn up virgin soil you get malaria as the result.

14704. That is an old belief?—And it is well authenticated.

14705. Quite so. There are explanations why that is the case, are there not: that in the first work of turning up the soil you often cause stagnant water, puddles?—That is not the explanation.

14706. But you do not think that ridicule is the right weapon to use against the belief that malaria is propagated by mosquitoes, do you?—No, I do not say that.

14707. Do you think that there is anything more ridiculous in the possibility of fleas conveying a disease than of the tsetse fly conveying a disease?—That is another thing altogether.

14708. I wish to know why this is ridiculous; why is ridicule the weapon to be used; and whether there is any difference between the possibility of the communication of plague by fleas and the possibility of the communcation of nagana by the tsetse fly?—The tsetse fly is on the same plane as the wasp; it develops its own poison. The wasp does not get its poison from outside, or the bee.

14709. The tsetse fly does not develop its own poison?—I think it does, and the same with the wasp.

14710. Then we will leave the tsetse fly, and I will ask you, do you think that there is such a thing as tick fever?—Very likely. I do not know anything about it.

14711. You think that with the exception of malaria the impossibility of disease being conveyed by infection is ridiculous?—I think in those cases that you refer to the disease is not a disease conveyed by the animal. The effects are the result of the poison secreted by the animal itself. In the case of the tsetse fly, for instance, and in the case of the wasp and the bee, they do not derive the poison from any other source than their own bodies.

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14712. I will put it in another way. You think the explanation that has been suggested for sleeping sickness is incorrect altogether?—I do not know anything about sleeping sickness.

14713. What is your authority for saying that the presence of the bacillus coli is necessary for the maintenance of health?—I took that from the late Professor Kanthack, who read a paper at Liverpool, in which he pointed out that when the bacillus coli was absent from the intestinal track the individual was not in a healthy condition. You will find that in his paper.

14714. When was it published?—It was read at Liverpool before the British Association. I forget the exact date.

14715. Do you mean the British Association or the British Medical Association?—The British Association, because I think it was in the September when Sir Joseph Lister, as he then was, read a paper at Liverpool; it was at the same meeting.

14716. You do not remember the year?—It must have been about 1896 or 1897.

14717. Never mind, I will not trouble with the year. I daresay I shall be able to find Professor Kanthack's paper. But are you quite sure that your statement is right that the typhoid bacillus has never been discovered in water?—I believe so. There may be one solitary instance, but I think it will be a solitary instance which will prove the rule. It was not discovered at Maidstone. It was not discovered at King's Lynn. You know what an outcry there was in the case of Maidstone, and not a single bacillus was found in the water at Maidstone.

14718. I should like to ask you on what grounds you say that in the cases of the epidemics at Maidstone and King's Lynn there exists no proof of contamination of the water by typhoidal matter. Let us take Maidstone?—It was supposed that the disease had been communicated through the occurrence of a case of typhoid fever amongst the hoppers. Now the disease began in July, and the hopping season does not commence until September. The first case occurred at Barming; another case occurred about a fortnight afterwards in the neighbourhood of Barming, and no connection could be shown between those two cases.

14719. Might I not ask you whether there was not only in September, or whatever month you just now mentioned, but also in July, quite a possibility that the spring might be fouled by enteric discharges?—But there was no case of typhoid fever in the neighbourhood, and there had not been.

14720. Well, none was known?—But you cannot affirm anything unless you know it as a fact.

14721. But still you would admit that where you have a source of water supply which is capable of being fouled, it is possible—I do not say that it is the case—that it might have been fouled by a passer-by who did not stay in the neighbourhood?—You have got to prove it.

14722. Quite so; but you admit that the spring was found to be in a condition in which it was possible to be fouled?—No, I do not think it was. I went into that subject very fully in my pamphlet from which my *précis* is taken.

14723. Then on the last page of your *précis* you say that the so-called pathogenic micro-organisms in more than one notable instance not only appear to, but actually do, exert a beneficent influence. What is your authority for that?—A few years ago Dr. George Stoker was treating some cases of chronic ulcer. He had a patient, whom I saw myself, with a large chronic ulcer on each instep. These cases were treated by oxygen gas mixed with air. Mr. Jonathan Hutchinson was asked to see these cases, and he suggested that the one case should be treated with corrosive sublimate solution and the other with oxygen gas. The result of that was that in a short time the one treated with oxygen gas was found to be healing very nicely, whereas that treated with corrosive sublimate solution was making no progress. Then some gentleman went up from some bacteriological institute and took a cultivation from both these wounds. The one treated with

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oxygen gas showed that the wound was swarming with staphylococcus pyogenes; the one treated with corrosive sublimate was sterile. Dr. Stoker found by subsequent observation that the more abundant the staphylococcus pyogenes was the better the case was going on, and if in the course of treatment the case seemed to be getting slack in its progress he would take some of the secretion from the healthy wound and put it upon the one that was not making such progress, on the ground that it was desirable to increase the vitality of the staphylococcus or the streptococcus as the case might be. Then Dr. Stoker brought the case before the "British Medical Association," and he appended to it a microscopical or bacteriological examination. The Editor of the "British Medical Journal" refused to put in the bacteriological examination, and simply said that the bacteriological results were astounding. I say that that is extraordinary evidence in favour of the view that I take.

14724. I do not follow it, but it may be so. It seems to me it is evidence that the staphylococcus or streptococcus, whichever it was, acted as a healthy stimulus to the ulcer?—It is evident, because when there was no staphylococcus there the ulcer was not healing.

14725. And I would suggest that in both cases the rest had a good deal to do with it—the patient not using his legs?—The rest could have nothing to do with it, because they had already been under treatment by rest for a considerable time, so that rest had no part nor parcel in it at all.

14726. (*Dr. Gaskell.*) Who was the bacteriologist?—I do not know. He came up from one of the institutions. I do not know whether it was the Clinical Research Association or not, but he was a skilled bacteriologist and microscopist.

14727. And where is this statement of Dr. Stoker's published?—In the "British Medical Journal." I give it you very fully in this pamphlet of mine (*handing in a pamphlet*). All the facts are given there.

14728. (*Sir William Collins.*) Your work has been chiefly surgical, has it not?—Yes.

14729. Especially in gynecological surgery?—Yes.

14730. Have you ever yourself practised vivisection?—Never. I have never even seen an experiment.

14731. Do you think that you have derived any advantage in your surgical treatment from knowledge acquired by vivisection?—None whatever that I am aware of.

14732. And do I correctly understand you to suggest that in no case has any micro-organism been shown to be the true cause of any specific disease?—I am not aware of any case.

14733. Does that apply equally to such things as malaria and tsetse fly where a cause of animal origin rather than vegetables is suggested?—Quite so.

14734. As well as bacterial disease, so called?—Yes.

14735. I understand you to admit that you find the micrococcus in association with disease?—I believe so. I accept that as correct.

14736. What relationship, if any, do you think subsists between them?—That the micro-organism is the result of the conditions just as you find grouse growing on moors and partridges and pheasants on arable land and salmon in running rivers, and so on.

14737. Do you suggest that any of these bodies, vegetable or animal, which are alleged to be pathogenic in their character, proceed from the body itself, or are they wholly from without?—I think they proceed from the body. For instance, bacteriologists tell us that we are swarming with bacteria.

14738. Do you suggest that they originate *de novo*?—I think it is possible. I do not know. I have never studied that part of the question, but I am quite of the belief that some diseases originate *de novo*. But with regard to bacteria that is another thing.

14739. (*Chairman.*) Do you accept what the bacteriologists say, that we are swarming with bacteria?—I accept it because I have no means of disputing it.

14740. (*Sir William Collins.*) It is not unknown, is it, that fluids which are entirely free from micro-organisms can produce very violent inflammation. Take the case of jequirity, which is obtained from *Abrus Precotorius*. That will produce acute ophthalmia, although it is free from organisms, will it not?—I do not know. I have no experience of that.

14741. Has a great distinction been drawn between the aseptic system of surgery and the antiseptic system?—I draw a very marked distinction between the two, but the current view is that the antiseptic and the aseptic treatment are the same.

14742. Did Lord Lister at one time draw a great distinction between the two?—I am not aware that he did. I do not remember any reference on the part of Lord Lister to the aseptic treatment.

14743. Did Lord Lister include in the antiseptic system such things as frequent change of dressings, free ventilation, dependent openings, and a judicious selection of cases for operation?—I think very likely. Any sensible surgeon would do those things.

14744. Did Lord Lister at one time criticise Sir John Burden Sanderson's work, because Sanderson said that dry air did not contain putrefactive organisms?—I should agree with Burden Sanderson in that. I do not know whether Lord Lister objected to it or had anything to say on the subject.

14745. Was the carbolic spray, in the earlier years of antiseptic surgery, regarded as a most important part of the procedure?—A most important part of the procedure, according to Lord Lister's own showing.

14746. It was finally renounced, as I understand, because, as Lord Lister stated, it was founded upon a theory that was physically impossible?—To affect the microbes.

14747. Did not Lord Lister call attention to the fact that inflammation may occur independently of any atmospheric external influences?—I think so; I believe so.

14748. Did he include in antiseptic surgery such things as the dry dressing of McVail?—Lord Lister included dry dressings impregnated with carbolic acid.

14749. But what was McVail's dry dressing?—Probably like my own, simple.

14750. Was it lint spread with lard?—I should say not. That is not dry dressing. Dr. McVail was in the habit of leaving his amputation cases exposed to the air until the surface got glazed over. That was a peculiarity of his practice.

14751. And some other surgeons at one time did the same?—Yes, but I am speaking of McVail.

14752. Was Mr. Lawson Tait practically on the same lines with yourself?—Almost absolutely.

14753. Was Sir William Savory?—Yes.

14754. Can you mention any other?—Savory, Tait, and myself were the only three who ventured to oppose the antiseptic or Listerian method. It might be interesting to know the result of my reading of a paper at one of the medical societies of London on hyperpyrexia, after Listerian ovariotomy, in which I showed that my best results were with the non-Listerian method. The result of my reading that paper was that I was twice blackballed by that society, so strong was the prejudice at that time existing against my views and the views of Mr. Lawson Tait and Sir William Savory.

14755. I understand, then, that your practice, based upon your views in the matter of ovariotomy has shown an unrivalled success?—Quite so; and it was a case of unsuccess that opened my eyes in the first instance—that is to say, I lost a patient from acute nephritis—carbolic acid poisoning.

14756. That was not a very unusual occurrence at one time?—At one time, I believe, it was not at all unusual. The evidence of absorption of carbolic acid was at one time very conspicuous.

14757. Did Lord Lister say that if the air did not contain putrefactive organisms it would affect his surgical practice in a most important manner?—I do not remember that statement on Lord Lister's behalf.

14758. Does anybody use the carbolic spray now?—I do not think so.

14759. I see that attention has been called in Mr. Paget's book to the alleged relation between the work of Semmelweis and that of Lister. Have you anything to say on that?—I do not think there is any connection between the two. Semmelweis went in for extreme cleanliness after parturition, and I think sanitary arrangements generally, and he would not allow students from a *post-mortem* room to attend upon cases in labour.

14760. Do you claim him as an aseptic or antiseptic authority?—I should call him aseptic, if you use the term at all, but I do not use the term aseptic, because I think it is a ridiculous term.

14761. I see on page 82 of that book of Mr. Paget's a quotation from Haller in 1849, in which he said: "The importance of these observations" (alluding apparently to Semmelweis) "is above all calculation both for the maternity department and for the hospitals in general, but particularly for the surgical wards"?—Yes; that was long before the era of the antiseptic treatment or bacterial observations.

14762. In regard to the question of tubercle, to which you called attention, whatever may have been the pressure upon Koch, is it not the fact that he published a book called "The Cure of Consumption"?—I could not say. I know of nothing but his announcement at Berlin. I do not know of any work that he has written on the subject.

14763. Have you not seen a work entitled "The Cure of Consumption: Further Communications on the Remedy for Tuberculosis by Professor Robert Koch"?—No, I have not seen it.

14764. Are you not aware of the claim there made that "phthisis in the early stages can be cured with certainty by this remedy"?—I believe that claim has been made.

14765. You do not accept it?—I do not.

14766. Do you know of any case of consumption that has ever been cured by tuberculin?—I am not aware of any case that has been cured by tuberculin. I am aware that cases of tuberculosis have been cured. I was taught that in my student days.

14767. As regards diphtheria and the presence or absence of the Klebs-Loeffler bacillus, I see that Dr. Newman, Medical Officer of Health for Finsbury, who has just been appointed Medical Officer to the Board of Education, in his work "Bacteriology and Public Health," says the organism may exist in the healthy throat without producing the recognised clinical symptoms of diphtheria. I understand that you agree with that view?—Yes, quite so; in fact, that is almost universally accepted now.

14768. He also states: "It should be remembered that about 20 per cent. of all cases of diphtheria offer no bacteriological evidence of infection. It therefore comes back to the point of broad judgment and common-sense. The clinical condition is the main fact for guidance, and the bacteriological must not usurp it"?—I agree with that.

14769. Then he says, "Whilst in 72 per cent. of notified definite cases of diphtheria the bacillus may be found, it has been shown that in apparently healthy persons who have not suffered from diphtheria the bacillus diphtheriæ may be present." You agree with that?—Yes, I agree with that.

14770. And that "Hewlett and Murray found 15 per cent. of the children in a general hospital had diphtheria bacilli in their throats"?—Those are statements which I quite accept as *bona-fide* fact.

14771. Do you think that suggests that there is the presence of the alleged cause with the absence of the alleged effect?—Yes.

14772. Or the presence of the alleged effect and the absence of the alleged cause?—It comes to that.

14773. The President of the College of Surgeons when he gave evidence before this Commission alluded to various instances in which surgery, in his opinion, had been assisted by vivisection experiments upon living animals, and amongst them was the treatment of septicæmic infections by sera. Have you anything to say with regard to that?—I have no experience of such treatment.

14774. In regard to the question of Listerism, the President was asked two or three questions by myself, so I will just read them to you and ask your views about his answers. At No. 7845 I asked: "As regards Lister's work, I suppose one would be right in thinking that there have been considerable modifications in the Listerian doctrine between 1876, when, as Mr. Lister, he gave evidence before the Royal Commission, and the present time." The reply was: "I do not quite follow that." I then asked: "For instance, in regard to the use of the spray and drainage, and so forth, there has been a very considerable alteration in technique, if not in the principles upon which Listerism was based?" The reply was: "The technique of the employment of facts which he has laid down, the technique of the employment of the treatment which is based upon the facts which he has discovered." Then again I asked: "Should I be wrong in thinking that Lister laid great stress upon the use of the carbolic spray and drainage?" And the reply was: "Quite so. Those are all modified now. The spray we know is an abolished thing." Have you any further remark that you desire to make in reference to that, arising out of your own experience?—I think Mr. Henry Morris maintained that abdominal surgery was very much indebted to experiments on animals. I entirely deny that. Abdominal surgery is the result of successful ovariotomy, and to that alone is due the success of abdominal surgery generally. It was shown that the peritoneum was not the sensitive membrane which it was always regarded as being. I recollect Sir John Erichsen asking me whether I would treat a knee joint in the same way as I treated the peritoneum, and I said, yes, certainly, I should have no hesitation in treating it just as I should the peritoneum. That was when I was not using the spray or carbolic acid, and experience has shown that I was right.

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14775. Do you recall a case in which Lord Lister, in one of his classical essays, attributed suppuration following an operation on the knee joint to the fact that the spray had been interrupted in its course by the removal of the drainage tube?—I do not remember that particular case, but I remember one case in which the failure of a breast case was attributed to the fact that there was a pinhole in the macintosh covering the gauze dressing.

14776. Then, as regards cancer research, do you think it is idle to look for light from experiments on animals with regard to that?—I am afraid so.

14777. Has any light been obtained at present?—None whatever, so far as I know.

14778. And as regards hydrophobia, I do not think you have mentioned that. Have you anything to say upon that subject?—I should like to say that I know of nothing which is less creditable to the profession than the readiness with which they accepted the claim which has been set up on behalf of the Pasteurian treatment of rabies.

14779. Possibly you would desire to amplify that answer. I do not quite follow at the moment?—I think in the first year there were some 3,000 inoculations. There were only a certain number of deaths, and they claimed that in all the other cases in which death did not occur the cases should be regarded as cures. That is the claim that has been set up by the followers of Pasteur: that every case in which the disease is not manifested after inoculation is a case to be regarded as a cure.

14780. (*Chairman.*) Is that a publication of Pasteur's that you are quoting? Are you quoting any statement of Pasteur's to that effect?—No, not of Pasteur himself.

14781. We have had no evidence of it. We have had evidence about it, but we have had no evidence that such a claim is made?—The claim is made, I said, by the followers of Pasteur.

14782. We have had many witnesses who agree with him as to his method of treatment. I do not think that any of them have made that claim?—We must be guided in this matter very much by statistics.

14783. Statistics are very useful to prove or disprove a claim, but you were saying that the claim was made?—Yes, that claim was made. I could not give you the exact reference to the work in which it was made.

14784. (*Sir Mackenzie Chalmers.*) Who made it?—It is a general claim made by the followers of Pasteur—by the Institute.

14785. (*Chairman.*) A general claim made by the followers of Pasteur is a little vague. At any rate, it was not made by Pasteur. Some people who believe in Pasteur may have made it?—I have not spoken of Pasteur alone. I think much more blame is to be attached to his followers.

14786. (*Sir William Collins.*) I see in 1886 (Question 11659) the cases treated were 2,671, and the deaths 25, or a mortality of 0.94 per cent.?—I should like to read a letter that I have here. I paid considerable attention to this matter in the early period, but of late years I was so disgusted with the whole thing that I have not paid any attention to it. But I wrote quite

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recently to a friend of mine, Dr. Lutaud, of Paris, and he writes me this in reply to my request for the latest statistics.

14787. Is he the author of a work that has been referred to?—Yes, "Pasteur et La Rage." I have the work here.

14788. (*Sir Mackenzie Chalmers.*) What is the date of that work?—1887.

14789. Twenty years ago?—Yes.

14790. (*Dr. Wilson.*) What is the date of the letter?—The date of the letter is the 23rd of April this year: "I was not in Paris during the Easter holiday, and was detained in the country by an attack of 'grippe'; if not, your letter would have been answered long ago. I cannot send you exact statistics for the last two or three years; the numbers I have published at the time of the great Pasteur polemic were demonstrative—viz., that the annual mortality by hydrophobia was not less *since* the Pasteur treatment than it was *before*. For instance, the average number of deaths from 1850 to 1866, date of the introduction of the new treatment, was 30; since the discovery of the new method it was a little higher (about 35 per annum). The system adopted at the "Institut Pasteur" consists in inoculating the virus to everybody having been not only bitten but even licked by a dog without inquiring if the dog was hydrophobic; so they have a lot of customers which are cured of a disease which had not existed. So they establish beautiful statistics to strike the people. The Pasteur Institute is the property of a limited company exceedingly rich, and they can afford to insert advertisements or notes in daily papers; they are regular business people, selling every sort of virus, serums, drugs, even stuff to kill the rats and rabbits. A very rich man, Mr. Ohris, who died recently, has left them 25,000,000 francs. I will try to furnish you with more information, but I am afraid they will come too late." But I have been furnished with that information in the form of a document signed by Dr. Boucher, a well-known man in Paris.

14791. (*Sir William Collins.*) What does that purport to show?—Shall I read it to you?

14792. If you please?—"Before Pasteur's method"—I am reading the translation, but here is the original (*handing in the same*)—"the official statistics collected by Tardieu and Boulay, with the view not of advancing a method, but simply to give an account of the deaths caused by a disease, indicate from 1850 to 1872 inclusive 685 deaths from rabies—*i.e.*, a mean of 30 deaths per annum. After the method when thousands of persons (3,000 patients) were inoculated, whether they had been bitten by mad dogs, or, in the infinitely more general case, whether they were preventively inoculated, the number of deaths, from 1887 to 1890 inclusive, were 154—*i.e.*, 38 per annum. At the present moment it is impossible to get the number of deaths in the whole of France; one entrenches oneself behind the difficulty of picking out the different cases. This excuse has as its true reason the danger there would be for the Pasteurian theories in publishing these statistics, which are more remembered than before Pasteur's method. Yet it is easier to pick out the different cases occurring in the Department of the Seine. The discussions at the Academy, the municipal statistics, render this easy. Here is the result of the comparison between the manifestations of rabies before and after the method. Official statistics before the method, 1876 to 1885: Deaths from rabies 43, a mean of 4 per annum. Ten years after the method, 1897-1901 inclusive: 44 deaths—*i.e.*, 8.8—more than double."

14793. Would that be of French people only?—This applies to France only.

14794. Does it apply to French people only, or does it include those visitors who came to France to be treated?—I do not know. These are official statistics, and it does not specify whether they are French people only, or whether it includes foreigners.

14795. It would be important to discriminate, would it not?—I do not know that it would.

14796. I suppose there was not prior to the Pasteurian period that influx to France for treatment which has been notorious since?—That is so. "I have not the numbers for the years beyond 1901 except the official numbers of the statistics, which are false. One discovers this falseness from a comparison of the numbers furnished by the Academy, and those supplied by Bertillon, Chief of the Statistical Department. Thus:

	Deaths.		Deaths.
Statistics of Bertillon	1897 - - 5	Statistics of the Academy Report to Council of Hygiene	1897 - 6
	1898 - - 4		1898 8
	1899 - - 7		1899 - 9
	1900 - - 9		1900 9
	1901 - 9		1901 - 12

the year 1902 representing one death, after applying to Bertillon 1902 none, 1904 one, express numbers three times less, at least, than the reality." Then he goes on:—"I recall the statistics of Carlo Ruata for Italy. Before the method the mean of the deaths from rabies was about 60. After the method—*i.e.*, from 1887 to 1900—the total of the deaths is 1,193, giving a mean annual rate of 85." That is the result of my application to Luteau.

14797. I gathered from Dr. Martin's reply to Q. 11659 that rabies was regarded as uniformly a fatal disease, and that, according to Bouley, 40 per cent. of those bitten by presumably rabid dogs contracted the disease, whereas amongst those treated by Pasteur from 1886 to 1905 the mortality was in every year less than 1 per cent.?—Because they included cases in which it was not possible to prove that the dogs were mad. That was one of the oldest things I can remember. Youatt maintained or affirmed that of 20 persons bitten by really mad dogs not more than one would develop the disease; that is only a matter of 5 per cent. To put it down at 40 per cent. is a stretch of imagination, I should think.

14798. In hydrophobia has any micro-organism been found as the cause of the disease?—I am not aware of any.

14799. Or has it been claimed by anyone?—Not so far as I know.

14800. In that case is it the fluid that communicates the disease?—It is the saliva of the dog that communicates the disease, and how it happens that the saliva is not the source of the anti-rabic serum I cannot understand.

14801. (*Sir John McFadyean.*) Do I rightly understand you to hold that the experimental method as applied to physiology and pathology has been entirely barren of results?—I have never said so.

14802. But I ask, do you think so?—I suppose I must give a qualified answer to that. I cannot give a definite answer. I have considerable doubts as to the results.

14803. You could not mention to the Commission any case in which you are satisfied that useful knowledge has been obtained by the experimental method in medicine?—I am not aware of any. I have never availed myself of any knowledge obtained in that way that I know of.

14804. Do you think that the same might be said about the other natural sciences, or physics and chemistry. Has any useful knowledge been gained by experiments in those sciences?—Undoubtedly, in chemistry. Chemistry is an exact science. We hear a great deal said about the science of medicine, which is all nonsense. There is no such thing as the science of medicine. Medicine is an art. You might as well talk of the science of painting.

14805. But is chemistry not largely an exact science, because the experimental method has been extensively employed in the investigation of its problems?—Chemistry is undoubtedly an exact science.

14806. Would you tell me why in the case of biology, including physiology and pathology, all methods of investigation except observation should be barren of results?—Because you are not dealing with exact data.

14807. But do you not admit that by experiments you can at any rate limit the data. Would your argument not be fatal against observation as well as experiment?—Not at all.

14808. Can anything be found out about a science which is not exact?—Yes, undoubtedly some things can be found out.

14809. Then you admit that some things can be found out by observation of things as they naturally occur—I mean physiological and pathological facts?—Yes.

14810. How is it that your argument against the experimental method does not apply to observation also. The science still remains an inexact one?—I cannot imagine an inexact science, you know; it is a misuse of the term "science."

14811. Call it anything you like; I cannot find another word handy in my mind that will cover what we both mean. But leaving that out of account you admit that observation has led to the discovery of a great many important facts in physiology and pathology?—Undoubtedly.

14812. And yet this branch of knowledge which you object to being called a science is inexact?—Yes.

14813. Why should one be able to learn something by observation and yet be unable to learn anything from carefully planned experiments?—All knowledge as the result of observation is open to error.

14814. Surely?—Unless you can come to an exact science you cannot rely upon the result of any observation.

14815. You mean to say that even the beliefs which are most firmly held that are founded on observation, may be wrong?—Yes. Take the case of Listerism, for instance. At one time, as you know, Listerism was almost universally practised.

14816. Is this an attempt on your part to discredit observation also?—Yes, it may be taken as such. Everybody, the whole world almost, was impressed with the advantages of Listerism.

14817. I really do not want to go into that?—I take it as an illustration.

14818. Would you forgive me for saying that it does not appear to me to be an illustration of the point which I was trying to elucidate at all. I wanted to learn from you how it came that one might learn a good deal from observation of naturally-occurring cases, but that one could learn nothing, and as a matter of fact in your opinion we have learnt nothing, from observation of experimental cases?—I did not say that we have learnt nothing, but I should like to know definitely what I have learnt as the result of experiment, for at present I cannot recall anything.

14819. You do not come here to give evidence except with regard to diseases of the human subject?—Quite so.

14820. You have no knowledge whatever about diseases of animals?—No, I do not profess to know.

14821. We have had a good deal of evidence laid before us to the effect that things of great practical value with regard to the treatment and prevention of animal diseases have been discovered by the experimental method. You cannot offer any evidence to lead us to discredit that evidence?—No, I am not aware of anything one way or the other.

14822. You make the following statement in your *précis* as explanatory of what the hypothesis was on which the special treatment introduced by Lord Lister was founded; you say:—"The system was founded on the hypothesis that germs, floating in the atmosphere fell into wounds, there developed and multiplied, and produced all the evil effects which sometimes followed surgical operations. In order to combat these lamentable results this system, when it was fully exploited, included operating under an antiseptic spray, and treating all instruments, ligatures, and dressings on antiseptic lines." It seemed to me that in developing your argument you chose to overlook the fact that an important part of the precautions advised by Lord Lister was directed not against atmospheric contamination, but against the contamination that was attached to instruments and to dressings and to hands?—That was not the basis of the Listerian system of antiseptic treatment.

14823. Observe I am quoting from your *précis*?—Yes, I affirm that what I have said there is correct; in fact, you have it from Lister's own address at Berlin.

14824. But I am taking it that you have not misrepresented him?—No, I have not misrepresented him.

14825. You have said that this system, when it was fully exploited, included the treatment of instruments, ligatures, and dressings antiseptically?—Yes.

14826. Does it not appear obvious that it was not directed solely against atmospheric contamination; that the treatment comprised two things? It was directed against what were supposed to be injurious germs in the air; but it appears to have been directed, and I certainly always understood that it was directed against injurious germs that were supposed to be practically ubiquitous and therefore likely to be found on hands and instruments and dressings. Is that not so?—In the early stage of the antiseptic treatment there was no idea of germs or bacteria coming from the skin; it was only of recent years that that was developed, when it was found that no system of antiseptic treatment of the skin could get rid of these bacilli. The staphylococcus albus in the skin is so deeply seated that it is absolutely impossible to remove them altogether.

14827. What do you mean by recent? You said it is only recently; do you mean the last 30 years?—Yes.

14828. That is not very recent?—As it happens, unfortunately, all this matter is ancient history, you know, because it is 30 years since I first began my opposition to Listerism, and that is a quarter of a century ago and more.

14829. I am sorry to say that it is 30 years since I was a medical student myself, but I have a very good recollection that at that time, at any rate, an attempt was made to destroy the hypothetical germs that were on the instruments?—But the main idea was the germs floating in the atmosphere.

14830. Why do you say that that was the main idea, when you admit that the system always comprised the two things?—I believe those were added afterwards. I believe that the first principle was that germs were floating in the atmosphere and got into the wound.

14831. Would you deny that at least 30 years ago, and within five years after Lord Lister's name was associated with this system, he in operating invariably endeavoured to destroy the germs that were supposed to be attached to instruments and hands?—I never got the idea that that had anything to do with Lord Lister's conception in the first place.

14832. But that was especially Lord Lister's practice?—Yes, afterwards; but the first thing that he did was to adopt the spray—that was the basis.

14833. (*Chairman.*) You told me that it was before 1860?—I forget when Lord Lister introduced the treatment. Perhaps Sir John can tell us.

14834. (*Sir John McFadyean.*) No, I cannot tell you. I am really prompted to put these question largely from my own experience. I wanted to know whether you could tell us of anybody who before Lord Lister habitually treated his hands and instruments and dressings with the object of destroying germs that were supposed to be attached to them?—I am not aware of anyone who did it before Lord Lister.

14835. So that the credit of introducing that belongs to Lord Lister?—Undoubtedly; that is the antiseptic treatment as distinguished from the aseptic.

14836. (*Sir William Collins.*) I thought you told me that Semmelweiss did it before?—Yes, cleanliness.

14837. (*Sir John McFadyean.*) You claim to have been rather a pioneer yourself in aseptic surgery?—I had an opportunity of telling the Commission before you came in that in consequence of my opposition to the Listerian method I was twice blackballed by a very distinguished Society in London, and that Sir William Savory and Mr. Lawson Tait and myself were the only men who had the courage to oppose the Listerian method in this country.

14838. But what I meant was that you claim rather to have been a pioneer in teaching that dirt was the thing to be avoided?—Well, scarcely. I think that phrase is due to the late Dr. Campbell Black.

14839. But you quote him with approval?—I quote him with approval, undoubtedly.

14840. Will you tell me what you understand by dirt?—That I have explained already in your absence. My view is that dirt may or it may not contain germs, and that dirt with the absence of germs may poison the wound if it is admitted into it.

14841. What is the chemistry of that sort of dirt?—I cannot tell you.

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14842. It is capable of analysis?—It may be, but I do not know that anyone has ever examined it, and I doubt whether chemical analysis would find out a poison unless it was of inorganic origin. If it was organic the probability is that a chemist could not find it.

14843. It is something so subtle that it is past finding out, you think?—I do not know that it is past finding out, but it would be a very difficult matter to find it out.

14844. Has it not occurred to you that you, or somebody whom you could set to do it, might make himself famous by discovering what is the precise nature of this thing in dirt that is capable of producing the formidable effects with which we are familiar in wounds not treated aseptically?—I have had no ambition in that line to make myself famous by chemical discoveries. I am satisfied to rest upon the results of my practical work.

14845. Do you contend that any sort of what is popularly called dirt is capable of causing suppuration in wounds?—I do not contend for anything of the kind, and I do not see how one can contend for it either. One must prove the absolute product of the application of what is called dirt.

14846. But I understand you to maintain that the great thing is to exclude dirt; that the presence of germs is of little or no consequence; that the dirt must therefore be the noxious element?—To give you an example of that, supposing that a man falls down on the road and cuts his head, and a lot of dirt, take, for instance, sand, gets in without anything else; if you attempt to sew up that wound it will not heal by first intention, simply by reason of the presence of the sand, which is not poison, but which simply produces local irritation.

14847. But may I point out that you are not entitled to draw any such conclusion unless you can give me a sample of this sand or dirt from the street that has not got germs. In discussing whether it is the dirt by itself or the germs that cause the evil, you must not cite a case in which the two are used concurrently?—You can wash a wound and deprive it of all soluble matter.

14848. Deprive it of all the dirt?—Of all soluble matter; but you cannot deprive it of all the sand.

14849. But bacteria are not soluble matter?—But they are washable; they can be removed by washing.

14850. So can the finer particles of dirt?—No; probably the large particles of dirt will be removed, but the small particles will remain, and if those particles be sand you have got a local irritant in the wound, and it will not heal by first intention.

14851. Would you deny that it is possible to inject sand and dirt from the road, provided that it has been freed from its germs, under the skin in considerable quantities without exciting any very serious result?—I think it is very probable that if you inject sand or any inorganic material into a wound it will produce some irritation.

14852. But would you admit that the sand or dirt when treated by any means that can be relied upon to kill bacteria seems to have lost that injurious power which it had in its natural state?—Can you refer me to any experiment of that kind, for I know nothing? There is nothing within my knowledge that would lead me to accept that statement.

14853. You think it is not possible to treat sand by heat, for instance, or by steam, so as to render it absolutely incapable of setting up any serious lesion?—I do not think it is possible, from my experience.

14854. I venture to differ from you. You said that you did not know of any experiments bearing on the matter?—No, I do not.

14855. You have not tried it yourself?—No, and I should not think of trying it either.

14856. On page 6 of your *précis* you say: "It seems very singular that the tetanus bacillus, which is believed to be the sole cause of tetanus or lockjaw, should be found so abundantly in manured or garden soil, or on the surface of roads fouled here and there with horse-droppings." I was not able to follow that reasoning. What is singular about it?—That the disease should be so rare while the presence of the bacillus is so common and so almost universal; that is singular.

14857. But does not that cease to be singular when one adopts the view that the tetanus bacillus is harmless except when it is introduced into the tissues through a wound. What is singular about that theory?—How do you account for it—if you will excuse my asking you a question—for the occurrence of tetanus after an ovariotomy, for instance? Can you explain the like of that?

14858. Certainly?—Do you mean to say that that is because the tetanus bacillus has been introduced into the body?

14859. If you operated, or any of those who hold the same views as you do, I should think it a perfectly natural consequence to happen, now and again at any rate, because you omit the only precautions which can ensure that tetanus will not set in; and even supposing that you adopted the precautions which are generally advised, it is not contended by anybody that there might not sometimes be a loophole which allowed the tetanus spore to creep in?—That is all assumption.

14860. That is the best answer that I can give to your question?—Very good; it must be taken as assumption, but not as evidence.

14861. What is there remarkable, may I ask, in the fact that the tubercle bacillus is not found in the sputum in the early stage of the disease?—I stated that to show that the tubercle bacillus is not the cause of tuberculosis.

14862. Is that the only evidence that you have got to show that it is not the cause of tuberculosis?—I think that is sufficient; it ought to satisfy any mind.

14863. Is it not perfectly natural that the tubercle bacillus should not be found abundant in the sputum, at any rate until the lesions which it has caused begin to break up?—But it is not found at all.

14864. Who said that?—Many people have said so.

14865. But I think that a great many people say that the bacilli are found?—I have read as much as I have seen on the subject, and I have never met with a man yet who has said that he has found the tubercle bacillus in the sputum before necrosis of the tissues has taken place—that is to say, in the early stage of the disease. It is denied by men of considerable authority on the Continent. I do not know whether there are any in this country who deny it.

14866. Yes, I think a good many deny it, and I think myself it is entirely contrary to the fact. You are familiar with the chain of evidence on which it is held that particular bacteria are the actual cause of disease. You are familiar, no doubt, with the evidence which Professor Koch produced many years ago to prove that what has since been called the anthrax bacillus is the actual cause of anthrax?—I have never paid any attention to the question of anthrax; I know nothing at all about it; but if I were asked for an explanation of the occurrence of anthrax I should say that when an individual suffers from anthrax, the active poison of anthrax has somehow been introduced into his body. I hold the same view on that question as I do on tuberculosis, and on other diseases which are attributed to the action of bacillus.

14867. But that is hardly going into the matter as minutely as I wanted. If you are not familiar with the evidence on which it is held that the anthrax bacillus is the cause of anthrax, I will briefly tell you what it is. It is that when you get a case of anthrax you can find this thing which is called the anthrax bacillus generally in enormous numbers, that you can induce this organism to grow outside the body in a great variety of artificial solutions of a comparatively simple character, and you can go on cultivating it and transfering it from one tube to another for years, and whenever you like you can produce what appears to be a case of anthrax exactly similar to the original natural one by injecting a little of this culture under an animal's skin. It dies, and it is just like the original case. It has this anthrax bacillus in its blood. When that can be done repeatedly, will you point out where is the fallacy in concluding that the bacillus is the cause of the anthrax?—In the first place the bacillus has not been freed from the medium in which it lives.

14868. Allow me to interrupt you by saying that it has been freed from the medium in which it lives many and many a time?—Washed and absolutely freed?

14869. You can wash it repeatedly for days, and it will still be found to have the same effect?—And when

it comes to the period of injection, do you inject the live bacteria or the fluid?

14870. You can inject the solid bacteria and nothing else?—Because you know in the case of Koch's fluid there are no living bacteria introduced at all.

14871. Nobody has pretended that there are any in tuberculin, so far as I know, but let us stick to the case of anthrax, or if you like take tuberculosis. You can cultivate this thing called the tubercle bacillus, and can carry it on for years and years thousands of miles away from any case of tuberculosis; you can wash it as long as you like, and you can introduce it into an animal and you set up what appears to be an ordinary case of tuberculosis. What is the fallacy in concluding therefore, when that is done repeatedly, that that thing which is called the bacillus of tuberculosis is the actual cause of the disease?—With regard to anthrax I know nothing at all about it, I have not studied the subject; and in the case of tuberculosis, so far as my reading and observation go, the introduction of the tuberculin was not the introduction of a living bacillus, and I am not aware of any experiments in which a live bacillus has been introduced, freed from the medium in which it lives.

14872. You think that that has not been done in any other instance either?—I do not contend for anything beyond what I know. I am not aware of any experiments to that effect.

14873. Would you deny that cases of tetanus have often been experimentally produced by using the organisms freed from their liquid surroundings?—I could not deny anything if it is stated as a positive fact. I am not prepared to deny what is stated as a positive fact.

14874. (*Mr. Ram.*) In the event, might I ask you, of your being satisfied of the introduction of such an active actual bacillus, and that the consequences were that it produced tuberculosis, or anthrax as the case might be, would that modify the views which you have been expressing to us to-day?—I should have to be satisfied that the bacilli alone were introduced and not the medium.

14875. I am supposing that to be the case. If you were satisfied of that—that they introduced the actual bacillus—would that modify all the opinions that you have been expressing to us to-day?—Probably, yes. I think if that were shown to me to be an actual fact, I would accept it as a fact, but there are so many theories.

14876. (*Sir John McFadyean.*) You contend for the existence of what is called a materies morbi as distinct from bacteria, and you apparently contend that when you get noxious effects from injecting cultures of bacteria it is something in the liquid that is to be blamed?—That is my view.

14877. Has this something in the liquid the power of multiplication?—Probably, as in the case of smallpox.

14878. You think that you can really figure to yourself a non-living liquid material which has the power of indefinite multiplication?—Undoubtedly. Smallpox is an example; vaccine is an example.

14879. You are begging the question?—I do not see it.

14880. In a very flagrant way, because it is contended that in all probability the materies morbi in vaccinia is a bacterium?—You say in all probability, but that is not a fact.

14881. I never said that it was. I said that it was a fact with regard to many other diseases, tuberculosis, anthrax, and so on, and it may yet be made a fact with regard to vaccinia?—But you have no right to calculate upon a possibility. You may calculate to some extent upon a probability.

14882. But I contend that it is a fact with regard to many other diseases. This same thing, whatever it is, has got a far greater power of multiplication in anthrax than in vaccinia or in variola?—I do not know how that is possible.

14883. Are you familiar with anthrax in animals?—I know nothing about anthrax.

14884. Would it surprise you to know that if you inoculate an ox to-day it may be dead in 48 hours, but the thing has multiplied to such an extent that every drop of the animal's blood if injected into another ox would kill it with anthrax?—That may be; that is analogous to the case of small-pox.

14885. Then that involves that during the morbid process what has been multiplying is not the bacterium, but some chemical liquid?—Some liquid. We will not call it chemical or anything else; but it is an unknown liquid.

14886. (*Sir Mackenzie Chalmers.*) Is there any known chemical poison which multiplies in the human body?—No, certainly not. The chemical process applies chiefly to inorganic matter; it does not apply to organic matter. There is something else introduced in the case of organic matter which is absent in the case of inorganic matter; that is vital force—life.

14887. Take the case of a vegetable poison, that is organic?—That is organic.

14888. Is there any vegetable poison that multiplies in the human body?—No. That is a very different thing from poisons existing in the human body. It is a totally different thing.

14889. But there is no instance of either an inorganic or vegetable poison which when introduced into a wound multiplies in the human body?—No.

14890. (*Dr. Gaskell.*) By multiplication or growth in the human body, do you mean necessarily something living?—Yes, apparently. Take the case of small-pox. You know that if you introduce an infinitisimal portion of the fluid under the skin you have an enormous multiplication of the actual poison, which is an exact repetition of the poison that you introduced under the skin.

14891. (*Mr. Ram.*) Which involves something living?—It involves a living subject.

14892. Exactly?—If you put it under the skin of a dead subject there will be no effect.

14892 (A). No, I mean does the multiplication, the growth, involve something living?—It involves the life of the subject.

14893. I do not mean that at all. You say that the multiplication, the growth, is not a chemical process; there is no evidence of connection with vegetable alkaloids. By that word growth, do you necessarily imply that the morbific material must be a living substance?—I do not know what it implies. There is a fact that you cannot explain.

14894. Can you have growth without life?—You must have the life of an individual. In my ignorance I call that a chemico-vital action, because you require a living subject in which this poison may be developed.

14895. (*Sir John McFadyean.*) But I submit that it is not quite fair of you to say that there is a fact which you cannot explain, because a minute ago I took an absolutely analagous case—namely, anthrax—in which there is the same apparent enormous rapid multiplication of something, and I put before you the chain of evidence on which it is held that it is not the liquid that multiplies, but that it is the thing which we call the anthrax bacillus, and you were unable to break down that chain of evidence?—In the case of anthrax I say that I do not know anything at all about it, but I stick to the case of smallpox, in which you have not shown the existence of any organism.

14896. But you also have not shown that there is not an organism there?—It is for you to show that there is an organism. It is for me to deny the existence of it until you can prove its existence.

14897. (*Sir John McFadyean.*) On page 7 of your *précis* you refer to this question of the rapid evolution of the morbid process in vaccinia or smallpox, and you say that it is an extraordinary state of things that this should be due to the action of a special form of bacillus which passes your comprehension and is incredible. That is going far further than saying that it has not been seen. You say that it is not credible. Again, I put to you the case of anthrax, which is one on all fours, and which you agree is not incredible. I have given to you the facts on which it is held to be proved, and you yourself admitted that you could not point out what the fallacy was. If it is true in anthrax, why should it be incredible in the case of vaccinia, if one merely assumes that the thing has been overlooked—that our methods so far have not been capable of detecting this thing—it may be too small for one thing?—But you have no right to assume its presence or its existence until you can prove it.

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14898. Not even by analogy?—No, not even by analogy. Analogy is a very unsafe thing to go by.

14899. Then on page 8 you discuss what happens in a patient affected with smallpox, including the fact that the patient generally becomes insusceptible to a second attack, and you say: "No one has offered even a plausible explanation." Do you think that is quite fair. Have you read the recent literature on protective inoculation and vaccination?—I am not aware that any observations have shown the mode by which this change is brought about or the nature of the change that is brought about as in the case of vaccinia or smallpox.

14900. Remember, please, that you are not the first scientific witness before the Commission?—I do not profess to be a scientific witness. I profess to be a practical witness.

14901. What I want to point out to you is that theories as to how this is brought about have been placed before the Commission?—Quite so.

14902. And it seemed to me to be rather too strong language to tell the Commission that these theories were not even plausible?—In my opinion. I am only expressing my own opinion, and I have a right to express my opinion on any subject.

14903. (*Sir Mackenzie Chalmers.*) I do not want to go into technical details; I only want to know whether I rightly appreciate your argument, because you have gone into some rather technical matters. You said that medicine is not and cannot be an exact science?—Quite so.

14904. It is a practical art?—Yes.

14905. Do you therefore go so far as to say that it has not made any advances towards being an exact science?—It never can, because in the first place we do not know what life is, and we are deprived of one of the most important premisses. It is absolutely impossible that medicine can become a science.

14906. Would you treat it practically then as a handicraft?—It is an art and a very empirical one too.

14907. Is not that almost going back to the old days of surgeons and barbers?—I do not know that it is. We have learnt by experience. We are trading in fact upon the experience of ages.

14908. But we are not making any scientific progress; we are only building up empirically?—Building up facts.

14909. But when we are building up facts, as it builds up facts does not an art tend to become a science?—Not a bit of it.

14910. When you generalise from facts and arrive at even an intermediate law, is not the art becoming a science?—Not a bit of it. I could give you plenty of illustrations.

14911. If I have understood your argument aright, you agree that in certain specific communicable diseases you have concurrently specific bacilli or micro-organisms?—For example?

14912. For example, anthrax and Malta fever?—In Malta fever that is not proved to demonstration yet.

14913. I understood you to say that there were certain specific micro-organisms or micro-cocci or bacilli; that with these specific infectious diseases you do in many cases find these specific bacilli?—Undoubtedly, and I say they may be characteristic of the disease, but they are not the cause of the disease.

14914. They are either concomitants or inseparable accidents, as the logician would say, or they are caused by the disease, instead of causing the disease?—You might just as well argue that a river is the result of trout being in it or salmon.

14915. Therefore you say that cause and effect have been transposed by the bacteriologists?—Yes. In some rivers you will only get trout and salmon; in other rivers you get other fish, such as dace and those fish that live in still water, where salmon and trout will not live. And in the same way you have grouse living on moors, while you have partridges living in arable fields, and so on.

14916. But they are not the cause of the fields?—Just so.

14917. (*Sir John McFadyean.* But you can get the fields without the grouse and the rivers without the salmon?—Yes, that is just it. You have got the disease without the bacillus. That is all right.

14918. (*Sir Mackenzie Chalmers.*) Have you followed carefully the researches of bacteriologists; do you read their literature carefully?—I have got so sick of the matter of late that I have not done so.

14919. For how long have you not done so—for eight or ten years?—More recently than that. I have paid some attention to the matter.

14920. But you have not studied carefully recent researches?—No.

14921. You think it is a waste of time?—Yes, quite so, because my work has been the practical work—the work of a surgeon, and I have never found any benefit from bacteriology.

14922. It is interesting to ask, is there any large body of medical opinion holding the same views as you do?—I do not know. But you know at one time I was in a minority of three against millions.

14923. *Athanasius contra mundum?*—Yes, and I was accused of that the other day in a letter in the "Medical Press and Circular," because I denied the occurrence of tuberculosis as the result of drinking milk. I asked for one single, solitary instance in which it could be shown even that there was a suspicion that the disease had been thus communicated, and the writer said it was a case of Dr. Bantock *contra mundum.* I pointed out to him that that was the position that I occupied some quarter of a century ago when I began my attack on Listerism.

14924. And now the *mundus* has come round to Dr. Bantock in that respect?—On the question of Listerism undoubtedly, and I do not get the credit for the part that I played in introducing the so-called aseptic system or the system of cleanliness; but yet there are some, who are not prejudiced, who admit the part that I played in bringing it about. As you know, Listerism now is as dead as Queen Anne.

14925. I want to come to another point, please. You mentioned the case of smallpox. Do you hold with the protective effect of vaccination?—I do, as the result of experience.

14926. As a mere matter of accumulated experience?—Yes.

14927. Do you see any objection to using animals for the purpose of providing vaccine? Do you think it is justifiable?—I have myself vaccinated a calf, taken the lymph from that calf, and vaccinated children with it, but that is the only thing I have ever done in the way of experiment.

14928. We may take it generally that you have come here on the recommendation of an anti-vivisection society?—Will you allow me to say, with which I have nothing at all to do.

14929. I only wanted to know how far you and they agree except upon one point. Do you think it is justifiable to use animals to prepare vaccine—you do?—I have no sentiment in the matter of experiments upon animals. With me it is a question of utility, and if I can be shown that certain experiments are valuable in increasing our knowledge I have no objection to them. I do hold some objections which perhaps will come out in my examination.

14930. Let me take you to another thing. You have told us a good deal about diphtheria; have you had any experience with the antitoxin?—No.

14931. Do you think its use is justifiable or unjustifiable?—It may be justifiable, but it has yet to prove its usefulness.

14932. Have you seen cases treated with the antitoxin?—I have not, and I should not think of treating a case with the antitoxin.

14933. You would not use it yourself?—No, I should not.

14934. But in the case of people who believed in its beneficial effects, do you think that they are justified in using animals in preparing the antitoxin?—It does not matter to me how the antitoxin is prepared. If it can be shown to be useful I would admit its use, of course.

14935. I want to go one step further. A good many new drugs come into use. Do you think it is justifiable before those drugs are tried on human beings to try them on animals?—Whether it is justifiable or not, I would say that in my opinion it is useless. I am inundated with new remedies every year, a thousand and one—"The Arabian Nights" was nothing to it—and scarcely one of these remedies that are so much vaunted is of any use.

14936. That is very likely?—And these drugs, mind you, as I understand, are sent out on the faith of certain results obtained by experiments on animals, not on the human subject.

14937. Surely among a thousand drugs, you may every now and then get one which is of great use?—As I have said, I think the thousand and oneth is the only useful one; and even in the case of those that are useful their application is very limited. A number of them were mentioned here by a witness—veronal, trional, sulphonal, and some other things with names as long as your arm, but none of those things are of any use. I have tried several of them, and they are absolutely useless, if not dangerous. Sulphonal, as you know, is a dangerous remedy because it is cumulative.

14938. I only wanted to get a general principle from you, to know what your opinion is. You think that researches in pharmacology are useless?—Yes.

14939. And it is better to stick to the old familiar remedies which we know, on the whole?—The longer I live the more I am reduced in the number of my remedies, until I can now count them on the fingers of my two hands. And there are fallacies about old remedies. Many of the old ones you will find, if you investigate them, are perfectly useless. So that as I say I can reduce the remedies that I use now to the number of fingers and thumbs that I have got.

14940. That simplifies medicine, does it not?—Immensely.

14941. Did you read Dr. Cushny's evidence given before this Royal Commission?—I fancy that I did, but I do not remember the trend of it.

14942. He gave evidence on the usefulness of animal experimentation with new drugs, and he drew the conclusion that it was of great benefit. I do not know how far you agree with him?—I do not agree with that. And I think Sir Lauder Brunton gave evidence to the same effect.

14943. He did?—I do not agree with that at all.

14944. Did you read Sir Lauder Brunton's evidence?—I did.

14945. You disagree with his conclusions?—Undoubtedly, particularly on the question of surgery.

14946. On the question of physiology, do you think that physiological experiments on animals are justified or not justified; do you think that we can learn anything from them?—I doubt it very much.

14947. Do you think that we ever have learnt any thing from physiological experiments?—I am afraid not. I am afraid we have not got any knowledge gained by observation beyond the case of Alexis Martin. I do not think we have got beyond that.

14948. That was at the end of the eighteenth century, after the Canadian war?—It was a long time ago anyhow. I think the date has been given in someone's evidence.

14949. May I take it, then, that the very numerous physiological experiments on animals since then have taught us nothing?—I think so; that is my opinion.

14950. You mentioned one other matter that I should like to ask you about; you mentioned the theory that plague was communicated by means of the rat flea as the mechanism of communication?—Yes.

14951. Have you read the two reports of the Plague Commission?—No, I have not.

14952. I am sorry that you have not, because the arguments to a mere layman seemed sound, and I should like to know where the fallacies were?—Well, there was one piece of evidence given before this Commission quite recently which freely excited my risible faculty to a very large extent. It was that when the experimenter was engaged in collecting fleas from the rats he wore top boots. I think when it comes down to that we have got the *reductio ad absurdum*.

(*Sir John McFadyean.*) The Commission must be devoid of a sense of humour, then.

(*Witness.*) I think that is in Dr. Martin's evidence.

14953. (*Sir Mackenzie Chalmers.*) Did you read his account of the experiments themselves, and the way in which he tested his theory?—No, I am afraid I did not, for as I say, I am rather sick of the thing.

14954. I may take it that you did not read the actual experiments?—No. I quoted the evidence obtained in India, where it was shown that patients who had died from plague presented no bacilli in the body. I stated that Mr. Hankin, Chemical Examiner and Bacteriologist for the North-west Provinces and Oudh, reported to the Plague Commission in 1899 that there was no doubt that cases of plague occurred among human beings in which no microbes were visible at the time of death; and I think that an actual observation like that is worth any amount of theory.

14955. There were also experiments made on the opposite side by Dr. Charles Martin on one day, were there not, where he excluded the flea and the animal did not take the disease; but where the flea got access to the other animal, the other animal did take the disease?—But it is absolutely impossible to ascertain all the conditions that surround a case of that kind, and unless you can do that, you cannot rely upon the observations.

14956. As you have not read the observations, I do not think there is any use in discussing it?—No, I do not think so.

14957. (*Mr. Ram.*) Do you think that the fact that it is impossible on certain occasions—say, in these cases of plague—to discover the bacilli in the patient, warrants you in saying that there is no such thing as a bacillus?—No, but what I maintain is, that it does not warrant the supposition that the bacillus is the cause of the plague. That is my argument.

14958. If there is a failure in certain cases to discover the bacillus, that does not preclude the discovery of the bacillus in other cases?—Certainly not.

14959. I think you have told us that many years ago you formed a strong opinion against what I may call the bacillus theory?—Quite so.

14960. And that for several years past, at any rate, you have not even read the researches made by others in the matter?—Quite so.

14961. And you have never yourself performed any experiments with the view of ascertaining the presence of bacilli or otherwise?—Never; I have never bothered my head about it.

14962. You know that we have had witnesses before us who have spent the whole of their lives in such experiments as these, and many witnesses of great distinction have laid before us evidence which, in their opinion, at any rate, warrants them in thinking that they have discovered the actual bacillus in the case of certain diseases capable of communicating the disease. Do you ask us to reject all that evidence in favour of the opinion of one witness who has never experimented, and has not lately even read the researches about the matter?—I ask you to accept my opinion for what it is worth. I am a practical man. I have been engaged in surgery for more than a quarter of a century, and my views are the result of practical work and observation in that work, and as the result of that I have come to the conclusion that the bacilli play no part in the causation of disease. They may be associated with the disease, and they may even be a characteristic of the disease, but they are not the cause of the disease.

14963. I think you told me just now, very candidly, that if you could have brought to your knowledge the fact that the undoubted bacillus by itself, apart from the surrounding fluid, on being injected into an animal caused the disease, that would lead you to modify all your evidence?—I would not say positively that it would.

14964. Then what is your attitude of mind with regard to that?—I cannot say. I should have to know all the circumstances, all the conditions surrounding the experiments, before I could give my adhesion to any particular doctrine.

14965. The case which I am putting to you is the case which has been very frequently put to us—that of the bacillus of some disease separated from any surrounding fluid, washed repeatedly, isolated, if I may so say, so as to be the bacillus pure and simple, injected into the blood of an animal, and causing imme-

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diate symptoms exactly identical with the disease of the person from whom it originally came?—Well, I have considerable doubt as to whether that has been done or not—whether the bacilli have been deprived of all the material by which they are surrounded. I say I have considerable doubt as to the absolute success of the cleansing process.

14966. But when you say you have doubt about it for many years, at any rate, you have not taken the slightest pains to find out what has been done in the matter?—No, I have not, and I do not profess to give any opinion on that subject. I have been so satisfied with the evidence that I have had, that I have ceased to take any trouble in the case of bacilli.

14967. Your evidence and such experience as you have had (and I know that it has been very large indeed, and very successful) have been entirely on the human subject?—Yes.

14968. And you have made no sort of experiments with the view of ascertaining the existence or non-existence of any bacillus?—Quite so.

14969. Now, with regard to this aseptic or antiseptic treatment, you have said that you were one of the pioneers of that which is practised so much now—namely, the protective measures taken by being cleanly?—Yes.

14970. By being cleanly, do you mean anything more than merely the use of water?—And soap.

14971. Against what do you guard when you are cleanly?—Dirt; and that is the only explanation I can give you.

14972. And what in dirt is it that you desire to guard against?—I do not know, nor does anybody know.

14973. You think that if you were not cleanly you might infect the patient with some disease with which you had been recently in contact?—Yes, undoubtedly.

14974. There is a something which some people call germs and other people call bacilli——?—Not generally so.

14975. May I finish my question? Am I right in thinking that there is in dirt a something which some people call germs and some people call bacilli, and of which you say that you do not know what it is, against which you wish to guard?—Undoubtedly.

14976. And against which, if you did not guard, you might infect the patient?—Quite so.

14977. You told us of the case of some surgeon who in old days went to a man who I think you said had been operated on for hernia. He inserted his finger into the wound; the wound was suppurating. He then merely wiped his finger on a cloth or towel, and he afterwards inserted the same finger unwashed into the body of another patient?—Yes.

14978. In your opinion was that a dangerous thing to do?—Under the circumstances it was not a dangerous thing to do, because the two wounds were in the same condition; but if he had gone to the first case and just wiped his finger and then gone and performed a fresh operation and allowed some of that material to get into the wound, then I should look for disaster. But the two wounds were in the same condition.

14979. So that the only safeguard was in the condition of the patient and not in the care of the surgeon?—Yes.

14980. Supposing that the second patient to whom he had gone was a patient with a clean uncontaminated wound, what would he have given to him?—Inflammatory mischief, followed by suppuration and goodness knows what.

14981. Why would he have given it to him?—Because he would have introduced some poisonous material apparently into the wound and otherwise dirt.

14982. He would have wiped off, by the process that you have told us, anything in the nature of fluid?—Not all of it. He would get some of it under the nail, for instance.

14983. And you think that that fluid might produce very serious consequences?—Yes.

14984. Why is it easier to suppose that the fluid would cause mischief than to suppose that in the fluid there would be a bacillus that would cause mischief?—You must prove that the bacillus was there first of all.

14985. I do not think that that is quite an answer to my question. I should be glad to have an answer to it?—Will you kindly repeat it?

14986. Certainly. Why is it easier to suppose that the fluid, as you have just told me, would cause mischief to the second patient, than to suppose that it was a bacillus communicated by the finger which was the cause of mischief?—Because the fluid is visible to the eye and you know the fluid is there, but you do not know that there are any bacilli in it; and, as we know, in the case of a chronic ulcer the application of the active staphylococci is of benefit to a wound that is not healing up well.

14987. But are there not thousands of cases in which things which are invisible to our eyes are most undoubtedly potent factors of either good or evil?—But how can you speak of them as being potent factors if you do not know that they are there? You only suppose that they are there.

14988. If you take a tiny drop of what you have been speaking to us about—from a vaccine pustule—and you put it into the body of an animal or a man, it produces vaccine?—It reproduces itself; it does not produce septicæmia.

14989. And you suggest that it is because we can see this tiny drop that it produces it. What does it matter whether we can see it or not?—It does not matter; all you want is to know that you put it there. It does not matter whether you see it or not. But you know that you put it there. You take it on the point of your lancet and introduce it.

14990. You are so confirmed in the view that you hold that there is no such thing as a bacillus being the cause of disease, that you think there ought to be no further research into the matter at all?—I am very much of that opinion.

14991. That is not the method by which medicine and many other sciences in the past have made advances?—I am not aware of the influence which these experiments have had upon the advance of practical work.

14992. But you have told us that you have not latterly, at any rate, tried to inform yourself?—No.

14993. Still, you think that no research as to the nature and method by which disease is communicated is advisable?—I would not go so far as that.

14994. Then how far do you go?—I would simply go to the limitation of experiments. I am not an opponent of vivisection. I might say that I should be almost sorry if vivisection were abolished, because it may be useful yet. It has yet to be proved that it has been of material service, but it may yet be so. The only opposition that I should offer to vivisection would be that operations should not be performed before a class—that experiments of which the results are already known ought not to be repeated; and that experiments with a view of ascertaining the action of remedies ought to be very much curtailed. That is the extent to which I should go.

14995. I am much obliged to you for the answer. I think it helps us very much, having regard to what we here have to investigate.

14996. (*Dr. Gaskell.*) On page 3 of your *précis* you give Lord Lister's words: "Hence I was led to conclude that it was the grosser forms of septic mischief rather than microbes in the attenuated condition in which they existed in the atmosphere, that we had to dread in surgical practice." Do you intend us to believe that by those words Lord Lister meant something that was not living germs?—I do not know what he meant. You must apply to Lord Lister himself for the meaning of his words.

14997. I heard the lecture myself; I was there?—This is taken from the report of his lecture as published in the "British Medical Journal."

14998. I know it. I was there at the time in Liverpool and heard it?—But one might make a mistake in one's recollection of a thing; but one cannot make a mistake in copying down the exact words of the address, and that is what I have done here.

14999. But I want to be quite certain that you do not want us to believe that that term, "the grosser forms of septic mischief," in any way implied that Lord Lister had given up the germ theory?—I cannot answer that question. I do not know what his ideas were. He gave up the idea that the spray could be of any use in affecting germs floating in the atmosphere.

15000. But in saying "the grosser forms of septic mischief" he was putting that against the next sentence, "the attenuated condition" of microbes in the atmosphere?—He said: "Hence I was led to conclude

that it was the grosser forms of septic mischief, rather than microbes in the attenuated condition in which they existed in the atmosphere, that we had to dread in surgical practice." What is the meaning of "the grosser forms of septic mischief"?

15001. Throughout the whole of his lecture was it not the case that he dilated upon the influence of the germs in producing suppuration and disease, which were "the grosser forms of septic mischief"?—I forget what he said throughout his lecture. That was the part that interested me, because it had a direct bearing on my own practice.

15002. But nobody who listened to that lecture could doubt for a single moment that Lord Lister meant throughout to hold on to the germ theory of disease?—At the same time I should just like to point out to you that Lord Lister certainly said at Liverpool that the condition was one of great importance; that it was not the germ or the bacillus alone, but that it was the condition necessary for the action of the bacillus.

15003. There was another point, too, that I wanted to be quite clear about, which you did not seem to me to make quite clear when Sir William Church asked you a question. You said, speaking of the microbes of tuberculosis, that we now know that air, light, and sunshine favour their growth. I do not understand about that "now know"?—As the result of experiments on sewage we now know that if you expose the sewage matter to the air it favours the action of the microbes.

15004. Then you refer in that "now know" to microbes in sewage, and you are not referring to the microbe of tuberculosis?—But it applies throughout.

15005. Does it apply throughout?—Yes.

15006. (*Sir John McFadyean.*) By analogy?—Yes.

15007. (*Dr. Gaskell.*) You have no experience that there is some difference, that some microbes like light and some microbes like dark?—I am quite aware that some flourish more in light and some in dark.

15008. And are you not aware that some will be actually killed out by exposure to light and sunshine?—No.

15009. You are not aware that there is a great difference in microbes in that respect?—I am not aware that they are killed off, for instance, when they get into a river and are exposed.

15010. (*Sir William Church.*) I understood you to say in answer to me that you distrusted those experiments in which bacilli were exposed to light and air, because they were done under unnatural circumstances?—Unnatural conditions.

15011. And I ventured to use the term that the very fact of their being in captivity was unnatural. Therefore, if you consider that an unnatural circumstance, you doubt all these experiments?—Yes.

15012. (*Dr. Gaskell.*) You have not seen any experiments of the kind?—No, none.

15013. They are very striking?—To some minds.

15014. I wanted to be clear, if I could, with respect to this fluid that accompanies the microbes, and which, when injected, reproduces itself, and so causes disease. That I understand is your position?—No, I have not said that.

15015. That it is something in the fluid?—I have not said that. The only instance that I have given of a reproduction of itself is in the case of smallpox. I have not said anything about the fluid of diphtheria reproducing itself.

15016. What I wanted to understand more clearly than we have yet obtained, was what is meant by the words "reproduces itself." All bodies can be put into two categories, either living or non-living. A living body we know reproduces itself. I do not understand what you mean by a non-living body reproducing itself, therefore when you use the term "reproduce" do you mean a living substance or a non-living substance?—You mean an organism or a substance.

15017. A living substance or a non-living substance?—I have made use of the word reproduction in the case of smallpox for this reason: that when you introduce an infinitesimal portion of the contents of a vesicle under the skin you have an enormous multiplication—call it multiplication or reproduction, whichever you like—of the poison; it is incalculable.

15018. I do not understand what you mean. If it was a salt in water, of any kind whatever, would there be that enormous reproduction?—Certainly not; I doubt whether there is any reproduction in the case of snake poison. We do not know the action of snake poison; we do not know how it acts upon the blood, whether it is multiplied or whether the initial dose is sufficient to destroy the blood and kill the subject; but we do know in the case of smallpox, because we can get any quantity of poison possessing precisely the same properties. There is no analogous condition, that we are aware of, to that of smallpox.

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15019. Do you not rather mean by its reproducing itself that it may possibly be a spreading of the small thing rather than an increase in quantity? Snake poison is the spreading possibly of the initial poison that is put in?—Spreading throughout the body.

15020. Carried about in the blood and fluids?—Yes.

15021. Do you not mean that that is what happens with each vesicle?—No, I do not think they are at all analogous. I think that there is no resemblance between them. Smallpox and vaccinia, which are probably the same thing, stand by themselves; there is nothing else like it in Nature.

15022. Now I have got it. Then it is a process of reproduction which is dissimilar to any other in Nature?—Undoubtedly, but no one can explain the nature of it.

15023. I understand you to suggest to us that if you have a fluid in which certain organisms, bacteria or microbes, are found, in your opinion the mischief is done by the fluid parts of that fluid rather than by the microbes themselves?—Undoubtedly.

15024. How would you test it? If you separated the fluid from the microbes would you expect the mischief to be done by the injection of the fluid part or by the injection of the part that contains the microbes?—I should say by the injection of the fluid part most likely.

15025. Are you aware that there is such a thing as a Pasteur filter which prevents these small microbes passing through?—I am not aware that there is a filter which will prevent microscopical objects from passing through.

15026. It is so; it has been shown again and again?—Well, of course, if you say that it is so, I am not prepared to say that it is not so.

15027. In that case if it was possible to do that you would expect the fluid part to give the disease rather than what is left behind?—Yes, that is what I should expect.

15028. You are not aware that that experiment has been done over and over again with just exactly the opposite result?—I am not aware of that.

15029. Would you tell me what is a trypanosome?—I do not know anything about it.

15030. You have never heard of it?—I have already said that I do not know anything about sleeping sickness or the cause of it.

15031. But you said that the tsetse fly developed its own poison?—Yes.

15032. Did I rightly understand you to mean that in the case of both nagana and sleeping sickness it was the poison of the tsetse fly that gave the disease?—I am not aware that the tsetse fly is the cause of that sleeping sickness. I know that it has the effect of killing horses and cattle in certain districts, but beyond that I am not aware that it has any influence upon the human subject.

15033. In the case of killing horses and cattle, I understood you to say that it was the poison that the tsetse fly developed that killed them. Is that your opinion?—In certain districts of Africa the tsetse fly kills horses and cattle, but I have never heard that the tsetse fly produces any effect upon man. If you read Gordon Cumming and Livingstone, and all those earlier explorers, who tell us everything about the tsetse fly, we never hear anything of sleeping sickness or any disease communicated to the human subject.

15034. Is it your opinion that it is the poison produced by the bite of the tsetse fly that kills those animals?—I believe so most thoroughly.

15035. You are not aware of anything to the contrary?—No.

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15036. In fact, you know nothing about trypanosomes?—I will not allow that I know nothing about them, because I have read extensively the writings of Gordon Cumming and Livingstone and other explorers who have told us about the ravages of the tsetse fly on horses and cattle, but not on human beings. Human beings were not attacked at all.

15037. If you or any of your friends were bitten by a mad dog, would you go to Pasteur?—Not a bit of it.

15038. You would not give him a chance?—No.

15039. You gave us some statistics with regard to hydrophobia. Those were all general statistics were they not?—No, they are very specific.

15040. They are statistics as to the number of deaths in certain times?—I think those statistics are specific, because they refer to particular times, and to a particular district of France.

15041. But they were general statistics, they were not case statistics?—They were not case statistics. I do not consider that the giving of cases is a statistical statement at all.

15042. When you first performed surgical operations for ovariotomy would you have considered it fair to you that the general statistics as to deaths from ovariotomy should apply also to your case?—No, certainly not.

15043. Would you not have felt very much aggrieved if it had not been a case there of case statistics rather than general statistics? Would you not have considered it unfair unless the person who drew up the statistics had said, Dr. Bantock has had so many cases of ovariotomy, and has had only so many deaths?—General statistics, lumping together the statistics of half a dozen operators, would be absolutely useless. What you wanted to ascertain in the earlier stages of the operation was what was the success obtained by any particular operator, and what was his method of operating.

15044. Is not that what you want to obtain with regard to the Pasteur Institute and hydrophobia?—I do not see the analogy.

15045. And not the number of deaths that took place in a certain year throughout France?—I think if you can show that there was a certain number of deaths during a certain number of years before the introduction of the Pasteurian treatment, and that the deaths had not been reduced over a similar number of years after the introduction of the method, that is a fair comparison.

15046. Would you like that sort of comparison to have been made in your own case with ovariotomy. I should not have liked it if I had been you?—I should not like my cases to be compared with the results of anybody else certainly, but I am quite open to have my results in a certain number of years compared with the results of another number of years. I think it would be very unfair to credit me either with the successes or the failure of any other operator. I must stand on my own feet.

15047. (*Dr. Wilson.*) With regard to that, of course, in dealing with cases of ovariotomy you cannot compare any results at all with the results obtained or supposed to be obtained by the treatment of a disease like hydrophobia?—Oh no. I do not see how you can talk of them in the same breath or the same week.

15048. Returning to the effects of bacteriology on surgery, I do not think your contention has been made quite clear. Your contention is that bacteriological views rather led surgeons astray in the first instance?—Undoubtedly. If the bacteriologists claim that the study of bacteriology has had any influence upon surgery, then I say that that influence has been a gigantic mistake in the form of Listerism.

15049. Was the term Listerism applied to what is known as antiseptic surgery?—The name was given to the method almost immediately after its introduction.

15050. And am I right in defining antiseptic surgery in this way, that antiseptics, carbolic acid say, were used as a spray to prevent the wounds becoming infected by the microbes contained in the air, and that antiseptics were also used for instruments, dressings, and ligatures? Was that antiseptic surgery?—That was antiseptic surgery as we understood it when Lord Lister invented it.

15051. And it was discarded eventually, just because the use of the antiseptics which were supposed to kill the bacteria injured the wounded tissues of the body?—Undoubtedly that was the sole reason why it was discontinued.

15052. And nowadays, of course, the spray is never used, nor are antiseptics?—No.

15053. And you, Sir William Savory, Mr. Lawson Tait, and others, from your own painful experience by trying this antiseptic method, revolted against what is called Listerism?—Quite so, and I might instance the late Dr. Thomas Keith, who never improved his statistics by one iota after he had adopted the Listerian method; and the result upon himself was that after a long operation under the spray he invariably got an attack of hæmaturia, and he ultimately died from kidney disease.

15054. But Listerism, or antiseptic surgery as I should call it, was advocated most strongly at the time by leading surgeons of the day?—All over the world.

15055. I may quote this passage from your pamphlet giving an extract from an address by the late Sir William MacCormac—he was then Mr. MacCormac: "The lecturer said, 'other methods I would term inexact. The Listerian method is, in contradistinction, an exact method'"?—That is so.

15056. That is in using the spray, and so on?—Yes.

15057. "'Founded on a special theory, and carried out in all its details, in almost precisely the same manner in each case: that the access of ordinary air is very generally the cause of inflammatory or putrefactive changes in wounds," and so forth"?—Yes.

15058. Those were the words used by Sir William MacCormac?—Yes.

15059. So that the consensus of opinion, as it is called, in those days was that the spray must be used for successful surgical operations?—Quite so.

15060. And then, afterwards, the system which was introduced by Mr. Lawson Tait, yourself, Sir William Savory, and others, known as clean surgery, came to be generally adopted?—I may say that the only two operators were Mr. Lawson Tait and myself. Sir William Savory had delivered a lecture at Cork upon the subject, but I do not know how far he carried out his views in practice.

15061. And on account of your strong protest you were blackballed because you referred to it?—That was after I strongly opposed the Listerian method, and advocated the practice of cleanliness; but I did not use the word aseptic.

15062. (*Chairman.*) I daresay it was so, but it would be very difficult for anybody who has been blackballed to say precisely why it was?—I know, my Lord.

15063. I have no doubt that it was an operating cause?—Yes, there is no mistake about it.

15064. (*Dr. Wilson.*) And you define aseptic surgery simply as clean surgery?—Clean surgery.

15065. (*Chairman.*) I should like to ask you a further question. I understood you to say that if you could imagine yourself converted, or perverted, you would say, to the opinion of those bacteriologists who believe and have satisfied themselves that a bacillus is the cause of a certain number of ascertained diseases, then you would think that the pursuit of bacilli for the purpose of discovering and identifying them as the cause of disease by means of experiments on animals would be quite justifiable if you can imagine yourself of their opinion as regards the cause of disease?—I should like to answer that by saying that at present I cannot conceive of my conversion to the doctrine.

15066. But might I assure you that you are not committing yourself to any admission by saying that, but simply putting yourself in the position of agreeing with probably nineteen-twentieths of the experimental physiologists of this country: if you could imagine yourself reduced to that, what would you say?—If I could imagine myself; but I cannot.

15067. Then I will put the question in another way, though I should have thought that you might with ease have allowed your mind to be sufficiently elastic as to follow me; but as you cannot do that, take the case of those who do strongly believe it, do you see any reason why they, holding that opinion, should not endeavour to follow out that line of discovery by experiments on animals?—I see no reason. I see no objection, and I see no reason to object to that.

15068. And also in the same way in the discovery of

remedies by those means—holding those opinions?—Quite so.

15069. Whether these diseases are caused by a bacillus or by a fluid is a thing which cannot be proved either way, you say. Supposing that by experiments on animals you could prove that it was not the fluid but the bacillus, or that it was the fluid and not the bacillus, would you think that experiments for the purpose of ascertaining that very important matter and making it perfectly clear, justifiable?—I should have no objection to experiments of that kind, but I do not think that they ought to be continued indefinitely.

15070. I will not ask you to repeat the conditions that you applied; you applied those conditions that you mentioned before?—Yes.

15071. You said that ridicule was the only answer to be given to those who thought that the plague was produced by the rat flea, and you gave us a reason for that that one witness had said that the doctors who were engaged in examining into the plague question in India wore top-boots to prevent their being bitten by the fleas?—Yes.

15072. That is not all that Dr. Martin, the witness in question, said. His answer is at No. 12111. He is asked, "Incidentally if the rat flea is the ordinary way of communicating the disease, how is it that doctors continually handling these animals in transferring the fleas from one to another, were not bitten?" His answer is "I imagine they were occasionally bitten." And then he added, "For one thing, they wore top-boots," but he goes on to speak of other precautions. I do not know whether you have read the evidence?—Yes, I have.

15073. I suppose that if these fleas do convey the plague it is very desirable not to be bitten by them?—Undoubtedly.

15074. And if there are fleas frequenting rats upon earth floors in a house in India, would you not think it was a very prudent thing to avoid contact with the fleas hopping about; would you not protect your legs?—Well, they will not convey the disease.

15075. But supposing you believed that the rat flea did convey it, would you think that it was a ridiculous thing, if you were going in the course of your duty into a place where you knew there had been plague and where you thought rat fleas were likely to be, instead of putting on a pair of light shoes to put on a pair of top boots?—I should think the protection very insufficient.

15076. It is better than shoes, is it not?—I do not know. The fleas would not attack you on the legs only.

15077. Of course they might fall off the ceiling, but they would be more likely to be on the dusty floor, would they not?—Yes, but I do not accept the doctrine that plague is due to rats.

15078. I know you do not, but you said that one of the reasons why you thought that ridicule was the only answer to that suggestion was that these doctors wore top boots when they went into places where the floors were likely to be frequented with fleas. That is not a very formidable reason, is it?—No, but I probably had other reasons in my mind when I reduced it to two lines.

15079. Dr. Martin goes on to say, you know, that other precautions were taken when they were likely to come in contact with the fleas.

15080. (*Sir William Church.*) The name of an old colleague of mine has been mentioned frequently to-day, Sir William Savory. Of course, he did not accept Listerism in full at first, but are you aware what Sir William Savory's views were with regard to, if I may so call it, the germ theory and suppuration?—I had no communication with Sir William Savory, except some little time after I was black-balled by the society.

15081. Do you know anything more of his views than what you have gathered from the lecture he read in Cork?—No more, except in conversation with him.

15082. And were you in the habit of conversing frequently with him afterwards?—Not frequently, occasionally.

15083. (*Sir John McFadyean.*) If I may ask you one more question, do I rightly understand you to contend that the term Listerism or antiseptic surgery, as introduced by Lord Lister, should be held to cover only the use of the spray intended to destroy atmospheric germs?—And all the other arrangements that he devised.

15084. Do you admit that he also was largely instrumental in introducing methods directed to destroying what he supposed to be the germs on instruments?—That is part of the system.

15085. I think that any person would have gathered from some of the answers which you gave to Dr. Wilson that the aseptic surgery which is now practised universally in London and all over the civilised world is practically what you call clean surgery and claim to have introduced rather than what Lord Lister introduced?—Undoubtedly.

15086. At what period did you begin to apply antiseptics to your instruments?—When I began to practise.

15087. What was the antiseptic that you employed?—Carbolic acid.

15088. Have you given up the use of things intended to destroy the *materies morbi* on instruments?—Absolutely, for the last quarter of a century.

15089. But you do not wish the Commission to believe that that is the sort of surgery that is practised in the London hospitals?—I do not know what the practice is.

15090. (*Chairman.*) I asked you to describe exactly how operations were performed when antiseptics were in vogue and then when the aseptic treatment came in, and you described to me that in the first case first of all the spray was used, which has been abandoned since, and that the hands of the operator were washed with some antiseptic, and that then antiseptics were used, some of them of an irritating kind like carbolic acid, upon the wounds in dressing. I asked you then to describe what was the aseptic treatment, and you said that it was the same, with the exception that the spray was discontinued and also the bringing of anything like irritant antiseptic such as carbolic acid into contact with the tissues; but that with that exception the proceedings were the same as heretofore?—That is so. The distinction between the two is as plain as a pikestaff.

15091. But it is rather different from merely saying soap and water.

15092. (*Sir John McFadyean.*) You really do not want us to believe that in the great hospitals of London at the present time anything approaching your system of clean surgery is practised?—I never contended that.

15093. They would not be content with washing their instruments and their hands with soap and water?—I never contended anything of the kind.

15094. Is that a part of Listerism?—It is not a part.

15095. You say that you began to direct your attention to instruments when you began Listerism?—Yes.

15096. Surely that is a part of Listerism. If not, I am asking who deserves the credit of having introduced it?—Allow me to say that many eminent surgeons do not use any antiseptics in connection with operations at all; that is to say, they keep their instruments in water which they call sterilised.

15097. Hot water, you mean—boiling water?—Just so, water that has been boiled.

15098. Do you want to deny the use of the adjective "aseptic" or "antiseptic" to boiling water?—I say that the use of the term "aseptic" is nonsense, because you cannot say that you have performed an aseptic operation. You can say that you have performed an antiseptic operation, but you cannot say that you have performed an aseptic operation, because you have to wait for the result.

THIRTY-FOURTH DAY.

Tuesday, 12th November 1907.

PRESENT:

The Right Honourable the Viscount SELBY (*Chairman*).

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Capt. C. BIGHAM, C.M.G. (*Secretary*).

Mr. J. N. LANGLEY, M.A., D.Sc., F.R.S., called in; and Examined.

Mr. J. N. Langley, M.A., D.Sc., F.R.S.
12 Nov. 1907.

15099. (*Chairman.*) You are Professor of Physiology in the University of Cambridge?—Yes.

15100. And in that capacity you have both to experiment on animals, I presume, and also to teach with regard to experiments on animals?—Yes.

15101. And do you give demonstrations before students?—I generally give three or four in illustration of my lectures in the course of the year.

15102. You have given us the heads of the points on which you desire to speak. I believe first you desire to say something about your personal experience that experiments on animals under the existing Act rarely cause pain?—On that point I wish to state the evidence which has come before me; it is almost entirely in connection with the University of Cambridge. In such a matter it seems to me better to give one's personal experience. I have had experience of the physiological laboratory in Cambridge ever since it was founded, and in that time all the experiments within my knowledge have been performed under the influence of anæsthetics. There are two classes of experiments which are carried out; the first are those simply under a licence in which anæsthesia is always maintained throughout the whole of the experiment and the animal is killed. In those I do not see how, short of gross carelessness, there can be any pain inflicted; and such carelessness, in my experience, does not occur. The other class of experiments come under a certificate; an anæsthetic is given, and then the animal is allowed to recover from the anæsthetics; the pain inflicted is that which occurs in a surgical operation, the experiment is done under the aseptic treatment, and it is done, moreover, on an animal that is healthy. An animal that is healthy recovers from an ordinary skin wound or a surgical operation with extraordinary rapidity. I myself have seen animals in an hour or two after the skin has been sewn up take their food in the ordinary way, walk about perfectly content; cats will purr. What I would call the more ordinary physiological experiment which comes under this certificate really does not inflict anything that can be called pain.

15103. What do you call the more ordinary physiological experiments?—Those are the more frequent ones.

15104. You mean that they may be severe?—Those that are severe I should put under the second class, which I would now speak of. In some of those experiments, possibly—such, for example, as where there is a section of the spinal cord, undoubtedly there is some pain during the process of recovery. That pain is less, I think, than in a human being, but it does occur and is of the same nature as in man. So that what I mean with regard to the class made under the certificate is in the first place that the great majority are quickly recovered from and do not inflict pain, although they come under this certificate which implies that they do. Then, secondly, there are some operations which do inflict pain, but the pain in that case is never, I think, as great as is common in a surgical operation on man. And on the whole, so far as my experience has gone in the Physiological Laboratory at Cambridge, the total amount of pain that has been inflicted is exceedingly slight. There are very few cases where I could have said that the animal had been suffering pain after any operation where the anæsthesia under the certificate has been allowed to cease. I may also mention that during the whole surgical part of the operation the anæsthesia is strictly maintained, that there is no question about doing anything at all on the animal unless it is anæsthetised, and properly anæsthetised. All the skin stitches, for example, are inserted under anæsthesia; nothing is done to an animal except in that condition; then it is put aside and slowly recovers from the anæsthetic.

15105. Do you make any distinction as regards the keeping up of the anæsthesia, where the animal is going to be allowed to come to after the operation finally, between the treatment of an animal and of a man?—None.

15106. You would keep the anæsthesia on just as completely and as long in the case of an animal as you would in the case of a man?—Absolutely.

15107. Does that include the sewing up of the wound and the dressing? In the case of a patient, I suppose you would put them comfortably up in their beds before allowing them to come to?—Yes.

15108. Is that the same process, leaving out that last element?—Yes, entirely the same. Everything that is done to the animal is done under anæsthetics. It is then put aside, and after everything is over it slowly recovers from the anæsthetic. In fact, generally myself I have given a little morphia or a little chloral, or something to prevent it from coming to until some time after the operation, because an animal

will stand more anæsthetic than a man; if it dies there is not the same serious mischief done, of course; so that on the whole one really gives more and prolongs the anæsthetic more in an animal than one does in man.

15109. You say that is your experience in your laboratory?—That is my experience in my laboratory in Cambridge.

15110. But you have studied elsewhere also?—I did work in Heidelberg for about three months, but there were not many experiments going on there at the time, and that is, of course, outside the operation of this Act.

15111. I was going to ask you whether your experience extended in that sort of operation outside Cambridge?—I have seen them in various laboratories outside Cambridge, and they have all been conducted, so far as I have seen, in exactly the same kind of way.

15112. (*Sir Mackenzie Chalmers.*) Just to clear up one point, it has been suggested to us that under Certificate B, it is a frequent course simply to administer the anæsthetic for the purpose of a cutting operation only, and that other exploiting and the painful sequelæ of the operation immediately connected with it are done after the animal recovers. Have you ever known a case of that sort?—It has never even occurred to me. The thing seems to me so foreign from anything that I can imagine is likely to occur in the laboratory that I hardly know how to answer the suggestion.

15113. It would not occur to you to construe that Certificate B as allowing that?—Certainly not; it never occurred to my mind that it could be done. Then with regard to the anæsthetics that are used, no one, I suppose, has any doubt that nearly all the anæsthetics which are used are sufficient to maintain anæsthesia practically indefinitely. I should think that could be taken now as an established fact.

15114. (*Chairman.*) Although it may be taken, amongst scientific men, as an established fact, there are other people who do not understand it to be so. You say that for an indefinite time the anæsthesia may be prolonged. In a case where you are going to give an ultimately lethal dose, but intend to operate in the meantime on the living animal, how long a time can intervene between the giving of the dose which produces anæsthesia and the death of the animal?—Of course, one has never carried on an experiment in that way. All that one can say is that one has never seen any sign of a tendency to recovery of consciousness in consequence of lapse of time; but generally it is the other way, that the anæsthesia becomes deeper and deeper, and the animal more and more immobile the longer the anæsthesia is kept up, and I have seen for something between eight and ten hours' complete anæsthesia maintained.

15115. (*Sir Mackenzie Chalmers.*) With or without artificial respiration?—Without artificial respiration.

15116. (*Chairman.*) So that if you required two, three, or four hours for an operation on an animal, and it was intended it should never come out of the anæsthesia, would you have any difficulty in keeping it under anæsthesia, yet still alive for the period?—Absolutely none—that is quite certain.

15117. (*Mr. Ram.*) When you said the animal would become more and more immobile, did you mean more and more incapable of reflex action?—Yes.

15118. Never at any time capable of conscious action?—Never at any time.

15119. (*Chairman.*) You were speaking about anæsthetics?—Of morphia I have very little personal experience. I have not done many experiments under morphia, but in those which I have done I am quite confident that no pain was inflicted. The animal in those cases lay perfectly still; when severe things like cutting the skin were performed, they did not cause any movement. I purposely tried this in consequence of a doubt which has been thrown out with regard to the anæsthetic properties of morphia. This was an animal that was not fastened, so that there could be no question that if it wanted to get up and run away it could do so; and there was no movement. As I say, I have not done many of those experiments, so that I have not great experimental experience in the use of morphia, but within my personal experience I am confident that pain was not inflicted by the operations of cutting the skin, or by the operations of cutting a nerve which would, without the morphia, have caused very considerable pain. Another point that I should wish to call special attention to is this question of the difference between reflex actions and ordinary voluntary conscious actions. It is, of course, known in man that during anæsthesia in the absence of pain and absence of consciousness movements may occur. I suppose that I need not enter into that; it does not come specially within my province; but, of course, one can say something about it on general grounds if it is considered desirable. But I should take it that it would be accepted that the experience of anæsthetists has shown that there is a degree of anæsthesia which allows movements in man to occur which are not accompanied by consciousness.

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15120. Or by pain?—Or by pain. What I wish to point out is that in animals this limit of range between the disappearance of pain and the disappearance of reflex action is far wider than in man. That is partly, mainly I suppose, because in man the forepart of the brain, which is concerned with consciousness and with pain, is so much more developed relatively to the rest of the nervous system. The rest of the nervous system is really a mechanism for carrying out a number of movements that is controlled by the fore-brain, which is concerned with pain and with consciousness. Now, the greater the normal working of the forepart of the brain, the less is the automatic, the intrinsic, power of the lower part of working by itself. In a very low vertebrate as the frog, all the mechanism of movement is carried out by the spinal cord and lower centres—it can be carried out purely by reflexes. The more the higher centres of the brain, which we may call those of the fore-brain, are developed, the more the lower centres cease to act of themselves. So that a suppression of the higher leads to a greater suppression of the lower. Or, in other words, when in man you suppress the activity of the forepart of the brain, then there is a greater diminution in the ordinary power of activity of the lower parts; they go so far hand in hand. The lower you go in the scale, the less effect is produced by suppressing the higher parts, those concerned with pain and with consciousness. Now anæsthetics are substances which have a special action upon the fore-brain. That is really what is meant by an anæsthetic; it is a chemical substance which acts primarily upon a particular portion of the central nervous system, which is that of the fore-brain. Anæsthetics amongst themselves differ very greatly as to their power of acting upon the lower centres. Chloroform, as I have mentioned, first abolishes pain; then it abolishes consciousness; after that there is a rather narrow limit of increase of dose at which it begins to act upon the lower centres, but for some time after nearly all the ordinary reflex actions have gone, respiration, which is part of the mechanical apparatus, which is part of the working of these lower centres, still goes on. And, moreover, it can be influenced by decreasing the amount of oxygen in the blood; it can be influenced by various effects upon the nerves, so that the mechanism is obviously working in the respiratory centres just as it also works, but to a less degree, in the whole of those lower centres of the spinal cord. The danger of chloroform is in the narrow limit between its acting on the higher and on the lower centres. If you give a little too much, it not only destroys pain and consciousness, but it stops the action of the rest of the brain and of the lower centres. If you give a little too much, it stops respiration; and remember in man respiration lasts a little longer than most of the other reflex mechanisms. Morphia is immensely more selective. Morphia picks out the pain centre, and it leaves these other lower mechanisms for a time practically intact. Moreover, it not only does that, but it stimulates a certain part of these lower centres, quite special ones, and they are those which do not give rise to pain in normal life. In an animal under morphia this selective action can be shown (it is obvious) in this kind of way. If a small point of the skin is taken between the fingers and pinched hard, nothing results; if a knife is taken and the skin is cut there is no movement; but if you give a loud rap on the table close to it, causing vibration, then there will be a more or less convulsive start of the whole animal. That is not a painful sensation.

15121. That is caused by the action upon the animal's nerves?—Yes, it is caused by the action on its lower mechanism. The evidence that is really produced by the mechanism is that as you increase the

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morphia the special reflex to a certain extent increases, and it continues (when it is pronounced) after the whole of the fore-brain has been removed. It would continue in a portion, in the hind limbs for example, after not only the fore-brain, but the whole brain, and the top part of the spinal cord, had been removed, so that there is little more (in that case, of course, artificial respiration must be kept up) than a fragment of the nervous system with the hind limbs depending from it. In that state, if you gave a sharp rap causing vibration, the hind limbs start in exactly the same kind of way that the fore-limbs and body do in the intact animal under morphia.

15122. Is that an experiment that would produce the same results in man as it would in any animal?—I believe that you would get the same sort of thing in man if the observations were made in the same conditions. In man many people have described the convulsive action which occurs near the point of death with large doses of morphia. It is well known, in fact, that there are certain twitchings of the muscles, and that these ,in certain particular cases, spread into real convulsions with excess of morphia. It is in a state of deep coma that this occurs. In fact, not uncommonly death results from the convulsions which are produced in coma. The convulsions are, I am certain, the same kind of thing as this increased reflex that is produced in the lower animals. In man, what is done to keep a man alive when an excess of morphia is given is to take him and shake him. He is walked up and down the room. It is old fashioned ; it is not done as much now, but it was an empirical experiment, that to save a man from morphia you marched him up and down, shaking him at intervals and shouting at him. So far as any painful stimulus was concerned, he was in a state of profound stupor, and took no notice ; but the shaking produced some kind of evidence that all this lower reflex mechanism was still reacting. I am quoting this not so much on the action of morphia itself as an anæsthetic, as in relation to the degree of movement that can be obtained after complete loss of consciousness. What I want to insist upon is that each anæsthetic has special properties, that the nervous system consists of groups of nervous centres performing different functions. Anæsthetics form an incoherent group of chemical bodies which have this in common, that they act first on the central nervous system in the part where pain and consciousness lie ; after that there are endless differences in the other effects ; they may, like chloroform, rapidly sweep through all the rest of the central nervous system and destroy the function of that too ; they may, like morphia, linger in the higher nervous system and only slowly proceed to abolish the lower. Further movements which are reflex, and which may be produced in anæsthesia, are the movements which are produced by a diminution in the amount of air, or a diminution in the amount of oxygen. In animals in a state of deep anæsthesia, if asphyxia is produced ; that is, if the supply of air is cut off, you may get more or less struggling movements which diminish with the depth of anæsthesia, but in perfectly complete anæsthesia it will cause rather strong movements ; the reflex action upon this respiratory mechanism is seen though to very much less extent in man also. The evidence that in animals it is reflex is shown by removing the obstruction. Almost immediately the air gets into the lungs and the blood gets oxygenated, the movements cease and the animal is perfectly quiescent.

15123. Is it the case also that the animal has become insensible?—Absolutely insensible. The anæsthetic is given first ; it is given to a stage in which the animal could be tossed about without any indication of movement. Then the trachea is stopped, the breathing is stopped by stopping the inlet of air, and movements will begin. The air is allowed once more to get to the lungs, and the movements stop. The trachea is stopped, once more, *i.e.*, the supply of air is cut off, and the movements begin again, and so on ; it will go on in this state of anæsthesia constantly with the regularity of the ticking of a clock.

15124. In ordinary suffocation not arising from anæsthetics, does a man become unconscious before he dies?—Yes.

15125. There is a space?—Yes.

15126. In that space does he struggle?—He struggles early, and for some time after unconsciousness is produced ; unconsciousness comes on rather early in suffocation, so far as can be judged by external appearance in man and by what is said by those who recover, but the movements in suffocation go on practically up to death, and usually there are rather slow laboured respirations at long intervals at a time when consciousness must have long gone. It varies greatly in different individuals. In the case of an animal that is anæsthetised, and then is asphyxiated, there is a regular series of ways in which the nervous mechanism works, and the last are inspiratory gasps which occur at an interval of perhaps 10 or 20 seconds, long drawn, and then they diminish and slowly die out. During that time and long before that the animal is completely unconscious. I have mentioned above methods in which various movements are obtained in anæsthetised animals, and which in the description of experiments in physiological accounts give rise to great misconception. There are many people who think that when in the account of an experiment it is said that the animal moved, or that the animal struggled, it implies a return to consciousness. That is not the case. It is perfectly certain that all the varied kinds of reflex movements and the general movements of the body may be noticed long after all pain and consciousness have gone. As a last example a small reflex movement may be produced by slight tension of the limbs, sometimes when the feet of an animal are fastened there may be a rhythmic movement of the hind limbs.

15127. You mean while the animal is under anæsthesia, but fastened, to counteract any movement?—To steady it and prevent it from rolling over.

15128. In order to prevent reflex movement?—Yes. merely to steady it. If the cords are relaxed to a certain extent the movements stop. If you gently tighten them the movements begin. If you relax them again they stop, and so on. It is the effect of slight tension ; moreover, that occurs with a portion of the spinal cord only, without any brain and without any medulla in the animal, in which practically everything has been removed, and I have not the faintest doubt that that would occur with artificial circulation ; if you could remove all the front part of an animal, and take only the posterior part, and inject arterial blood through it, you would get the same regular movements steadily occurring on a particular kind of tension.

15129. That, I think, is what you wish to say about anæsthetics?—Yes.

15130. You, of course, have formed some opinion as to the value and importance or the reverse of using experiments on animals for the purposes of physiological and medical knowledge?—On general grounds it seems to be almost self-evident that as physiology is the study of the mode of action of the living organism and of its various parts, that they must be studied living, because the function ceases when life ceases. Anatomy, the use of the microscope, observations in health and disease, offer suggestions as to functions, but these must necessarily be tested by experiments, for otherwise they leave room for endless conjectures. To take a single example. The functions of certain nerves can only be determined by actual experiments on animals. It is impossible to discover where a vaso constrictor or a vaso dilator nerve runs except by trying it. There is no microscopical difference, there is no anatomical difference. The only way of noting the difference between a vaso dilator and a vaso constrictor nerve is to try and stimulate the nerve and see what effect is produced.

15131. (*Sir Mackenzie Chalmers.*) That is not necessarily a sensitive nerve?—No, a peripheral nerve, a nerve removed from the centre and running to the periphery of the organism, as to a muscle or gland, will serve for experiment.

15132. Is that necessarily a sensitive nerve?—Not necessarily.

15133. (*Chairman.*) In point of fact, was the distinction between those nerves known before experiments on animals?—It was not known before experiments on animals, I think.

15134. Was it known that there were two sets of nerves, apparently the same, but having different functions or action?—So far as I know it was not known. I have not discovered any reference to it before the experiments of Claude Bernard.

15135. It is now known by those means, I under-

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stand, that there is that distinction, and you can discover which nerves act in a particular manner?—You can discover which nerves cause dilation of vessels, and which cause them to contract. And in the same way nearly all we really know of the functions of the different parts of the body has been derived from experiments on living tissues and living organs and living animals. With regard to the applicability of the results obtained on anæsthetised animals to unanæsthetised animals, it has been said that observation made on animals under anæsthesia are not applicable to ordinary life, because they are abnormal. The use of the word abnormal in that sense is purely misleading; it depends on a juggle with words. It seems to be supposed that anything that can by any chance be labelled abnormal must therefore be inapplicable to normal life. It is obvious that that is not the case, because one of the ordinary meanings of the word "normal" is "average."

15136. I do not understand why the words normal and abnormal are used. The question is rather, is it not, whether you cannot necessarily apply to human life a conclusion which you may safely draw from animal life? You would not say that if the life of an animal, or its construction, differed in some respect from that of man, it was abnormal?—No, I take it that one objection that has been made to experiments is that the anæsthesia modified so much the whole character of the living tissue, that you could not apply the results to the normal unanæsthetised animal.

15137. I see. I thought you were arguing simply from animal to man?—No, the relation of animal to man is, I think, the second line of argument; but the first, which is more general, is that you cannot use any result obtained under anæsthesia. Of course, that depends upon a purely verbal juggle with words. The abnormality consists solely in the alteration in pain and consciousness. As I have indicated, the nature of anæsthetics is, to begin with affecting certain parts of the nervous system, and it is only subsequently that they go to other parts of the nervous system, and it is subsequent to that that they proceed to the muscles and nerves and the various glands in the various parts of the body. So that, in fact, nearly all the parts of the body are normal, and in the ordinary condition; their state is one which allows conclusions to be drawn as to the condition in ordinary life, except as regards the particular circumstances of pain and consciousness. The animal is abnormal. The tissues are not abnormal; they are perfectly normal. So that in that way all the muscles, the blood vessels, and viscera—nearly the whole body except just this part of the brain connected with pain and consciousness—is in a condition which allows you to definitely state what is the condition in ordinary life. With regard to the applicability, then, of experiments on animals to man, it is to be noted that the tissues of man and the organs resemble very nearly those of all other vertebrate animals, so that there is a probability that the function of any one tissue or organ in any one vertebrate would be approximately the same as that which it has in other vertebrates. Further, if you find a function in a number of different animals, the probability, then, is very great that there will be a similar function in man, and the more animals that you can show have the same kind of function, the more it is necessarily applicable to man. As a matter of fact, a similarity, and often a very close similarity, has been shown over the whole range of physiology to exist between man and the vertebrates. The normal working of the parts of the human body differs in most cases only in detail from that of the higher mammals, so that experiments on animals give to a large extent positive evidence with regard to the condition of man. But they do more than that, because they clear away a number of erroneous hypotheses. If we look back to the history of medicine, it is clear that treatment is always open to a number of erroneous modifications, until a definite reason is given why a particular method of treatment should be applied. As soon as a reason can be given, then a particular line of treatment is rapidly adopted. It satisfies the human mind to have something which it can positively say is the cause of a particular thing. Until that can be given, so long there is room for numerous conjectures, numerous side issues, and numerous different forms of treatment. That is really indicated at the present day very largely by the different treatments in nutrition. One will recommend the constant feeding on meats; another constant feeding on this or that diet. These different treatments which are considered to be useful in particular functional processes mark the fact that we do not know the scientific reason for the dietetic methods, and, in consequence, all the possibilities of treatment are open and have followers; so that the greater the number of possible ways of accounting for a fact, the greater is the number of erroneous methods of treatment. Physiology, I think, then, aids medicine and surgery in those two ways, in that it brings positive results which are in the highest degree probable for man, and then that it clears away a number of erroneous hypotheses, and gives some firm ground for explaining current practice, and so leads to the adoption of the practice which is really the best and the most useful for the purpose.

15138. Are experiments upon animals, in your opinion, necessary in order to correct a great many errors which arise from hypothesis or suggestions?—I think they are absolutely necessary. The usual method is to disprove first one hypothesis, and then to disprove another, and gradually to arrive at truth in that way, but each step in the process, I think, is valuable in its application to the ordinary treatment of medicine and surgery.

15139. Of practical value?—Yes, of practical value. One case of a particular advance in surgery which I should like to bring before the Commission is that of the effect on the surgical treatment of diseases of nerves and of severed nerves, which has been brought about by experiments on animals.

15140. Do you mean so as to enable you to know how you should treat a case of an accidentally severed nerve in a man?—Yes. I think that has come from experiments on animals. The earlier relation I may treat quite briefly. Animal experiments disproved the old medical and surgical belief which came from Galen that nerves could not be healed at all. That was current till about 1770, and then Cruikshank and a number of other observers, by experiments on animals, which went on to 1822—they were, in fact, on dogs—showed that nerves could heal when joined together. That was adopted in surgical practice, and it became the custom then to join up the central end with the peripheral end of a severed nerve, and in a very short time it was found that the difficulty in certain cases was not to cause nerves to join, but to prevent their joining too rapidly in particular instances, as, for instance, in neuralgia. A modification of the treatment was introduced in the case where a large portion of a nerve was cut out. That, again, so far as I have been able to discover, was first introduced by Philipeaux and Vulpian in 1869, who in dogs introduced a portion of a foreign nerve between the central end and the peripheral end of the cut nerve, and found that the piece introduced could aid and help regeneration. That also is a constant practice in the surgical treatment of nerves.

15141. Were all those experiments made without anæsthetics?—I should think the earlier ones were made without anæsthetics, but the last that I have mentioned, those of Philipeaux and Vulpian, I think were experiments under anæsthetics. The earlier ones undoubtedly were without anæsthetics.

15142. They would be very painful operations, I presume?—That depends upon the nature of them. The simple connection of a severed nerve I do not think would be necessarily painful.

15143. Some of them, at all events, would be painful operations?—Yes, some of them would be.

15144. (*Sir Mackenzie Chalmers.*) They were foreign experiments?—They were made in France. The earlier experiments of Cruikshank were published in the Transactions of the Royal Society in 1789, I think; they were by an Englishman, in 1770.

15145. Can those operations be done now under anæsthetics?—Yes.

15146. (*Chairman.*) They could not be done then under anæsthetics?—They could not; there were no anæsthetics discovered at that time. The two main points then were, first, the actual union of a nerve after having been severed, and then the introduction of a piece of nerve. A further development then was in joining the central end of one nerve to the peripheral end of a different nerve, which is necessary in certain operations. For example, when a facial nerve is injured in the skull, as it is sometimes by an abscess, or by other causes, such as bullet wounds

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and so on, the central end cannot be got at—it is too deep down and in the bone—for the purpose of joining on to the peripheral end. What is done now, consequently, is to take the central end of another nerve and to join it on to the facial nerve, and that, it is found, produces recovery from facial paralysis more or less complete. This operation is one which can be traced with remarkable closeness immediately to operations which were made on animals. There was first a series of experiments which showed that the thing was possible—that is to say, physiological experiments which were purely of a scientific nature. The first of these was a very early one by Flourens, in 1824-1828, who performed the operation of the cross-union of nerves in the wing of a fowl. He took the nerves which run to the upper portion of the wing, and joined them to those which go to the lower portion, and *vice versa*, and he found that in no long time the fowl could use these nerves, and could fly—that they had actually joined crosswise—that the central end of one nerve had joined on the peripheral end of another and different nerve. Then physiological experiments on other nerves were made by Bidder in 1842, and by Philipeaux and Vulpian in 1863. That then led to some operations in France by Desprès in 1875. Further experiments on animals of a more recent kind are those of Rawa in 1885, Stefani in 1886, Howell and Huber in 1892, Cunningham in 1898, and Kennedy in 1901. All of these joined nerves which were approximately of the same kind. Schiff and Reichert in 1885, and myself in 1898, tried nerves of a different character; those in my case were apparently of a thoroughly different nature, and it was found that there also the central end could join the peripheral end. So that it was an established scientific fact that almost any motor nerve could join the peripheral end of another motor nerve. It took a little time before this purely scientific physiological deduction was translated into actual practice; that was done in France, Germany, America, and England with a few years; in nearly every case the deduction for the operation in facial spasm and facial paralysis in man was made as the result of the experiments mentioned above. Up to about 1898 facial paralysis in man was regarded as incurable, and certain kinds of facial spasm were also regarded as incurable. Nobody thought of doing anything except ameliorating the methods of treatment. Nobody supposed for a moment that they could be healed. The first successful operation was by Kennedy in England. The operation was performed in May, 1899. The paper giving an account of it was read before the Royal Society on November 22nd, 1900, and published in the Philosophical Transactions, 1901. The case was that of a woman who was admitted to the Glasgow Western Infirmary on account of chronic spasms and incessant muscular twitchings on the right side of her face, which had lasted ten years. As a result of that operation, the tonic spasm disappeared, and control over the muscles of the face was regained. Kennedy definitely states that he performed these operations because of the general physiological result that was known, and because of the experiments on animals which he made himself directly for the purpose.

15147. Was that a new operation on man?—Yes.

15148. And was it successful?—Quite successful.

15149. What was the condition of the person before the operation?—In Kennedy's case all one side of the face was in constant twitching, a very uncomfortable thing; there was tonic spasm which gathered up the corner of the mouth across the face.

15150. Was it disfiguring?—Most disfiguring; it would practically prevent the subject entering into a great many of the ordinary occupations of life.

15151. And after the operation did these spasms entirely disappear?—They entirely disappeared.

15152. And the face resumed its normal appearance?—Recovery is rarely absolutely complete; there is generally some sign left. But control over the face and control over the muscles was restored—that is to say, all this drawing of the face and the mouth to the side disappeared.

15153. But there are some traces left of the disfigurement?—I think in all these instances there will be some traces left. But Kennedy said that what induced him to try the experiment—which was, of course, rather a daring thing to do on man—was this: "Being convinced that voluntary co-ordinated movements had been restored after nerve-crossing in the dog, I decided to treat the case by dividing the facial nerve and grafting its peripheral segment on to the spinal accessory nerve, in the hope that the same result of nerve-crossing would result in man as had proved to take place in the dog."

15154. You have in print before you, I believe, the remaining evidence of the success of these operations?—I have. Subsequently several cases of facial paralysis, treated by joining the spinal accessory nerve with the peripheral end of the facial were described by C. A. Ballance, H. Ballance, and Purves Stewart ("British Medical Journal," May 2nd, 1903). In this paper Mr. C. A. Ballance mentions that he first performed the operation in 1895. The result was not known for some years, as the patient was lost sight of. Mr. Ballance informs me that he felt justified in making the operation "by the fact that the cross-union of nerves had long been known by physiologists to be a successful operation." In France the first operation for facial paralysis was performed by Faure [Faure and Furet, "Gazette des Hôpitaux," 71 An., 8 Mars., p. 259, 1898]. Furet suggested to him [cp. Bréavoine (op. cit. infra)] that the hypoglossal nerve should be joined to the facial. The choice of the hypoglossal may reasonably be attributed to this nerve having been used in the well-known experiments on the cross-union of nerves by the French observers Philipeaux and Vulpian.* On discussing the question, they came to the conclusion that the spinal accessory would probably be a better nerve to take than the hypoglossal. The operation was performed early in 1898. Nine months later Faure ["Revue de Chirurgie," T 18, p. 1098, 1898. Improvement in this case was noted at a later date, and in 1901 (Congès de Chirurgie, Paris, 1901) Faure describes it as a partial success] reported that no recovery had taken place. He argued, however, that success would be obtainable in proper conditions, since it had been shown that different nerves of the same nature can become functionally connected. The failure of the operation could not, however, but raise some doubt as to its feasibility, and this led Barrago-Ciarella [Barrago-Ciarella, "Il Polichnico," 1901: "Per consiglio del mio maestro, che vivamente ringrazio, ho voluto tentare la risoluzione del quesito che Furet e Faure vollero sciogliere, limitando pero questa mia prima ricerca al fatto, se cioè nella procurata paralisi facciali mediante la sezione del nervo alla sua uscita dal forame stilomastoides con l'immediata sutura del moncone periferico di questo all'estremita centrale dell'accessorio, resecata al momento del suo ingresso nella faccia interna dello sterno-cleido-mastoides, sia possibile ripristinare la perduta funzione dei muscoli innervati dal 70"] to test in dogs how far return of function could be brought about when the spinal accessory or the vagus was joined to the peripheral end of the facial nerve by primary suture. He made three experiments on dogs, and found that recovery of motor power occurred in each case. At Faure's suggestion, Bréavoine [Bréavoine, "Travaux de Neurologie Chirurgicale," Vlme. An., p. 92, 1902] in 1901 drew up a statement of the observations made in various countries up to that date: "Dans le but de faire connâitre en France les heureux resultats de cette opération, tant sur les animaux que sur l'homme." In America the first operation was performed by Cushing [Cushing, "Annals of Surgery," May, 1903] in May, 1902. The patient was a man of thirty years of age, in whom the facial nerve had been severed by a bullet wound. "Motor paralysis was complete on the right side of the face and in the platysma as well." "Bell's sign, the rolling of the eyeball on attempted closure of the eye, inability to pronounce distinctly the labials, lacrymation, and the other many discomforts attending on the condition of facial palsy, were present." As a result of the operation the discomforts disappeared, the muscles were saved from atrophy, and voluntary control over them regained. The operation was suggested to Dr. Cushing by experiments made upon cats. He says (p. 4) the "suggestion for such an operation came to the writer through cognizance with Langley's experimental work on the transference of nerve function after anastomosis between the vagus and the sympathetic." Other similar operations have recently been made by Korte in Germany, by Dr. Tubby in England, and probably by others elsewhere. It may

* *Faure and Furet refer to Lebierant, who refer to Philipeaux and Vulpian.*

safely be predicted that the operation will alleviate human suffering and distress all over the world. But there are several questions still to be settled before it can be definitely ascertained what is the best method of operation. Thus it is probable that if the whole spinal accessory were cut through, the central end of the trapezial branch joined to the facial, and the remaining central branch joined to all peripheral branches of the spinal accessory, the maximum of independent voluntary control would be regained, with litle or no permanent paresis of the muscles supplied by the spinal accessory. The further experiments, if not made on animals, must be made on man. Facial paralysis in an animal is much less distressing than in man, since a large factor in man, the consciousness of a distorted face, is absent in animals. Since I wrote the pamphlet from which this evidence is taken, *i.e.*, last year, my attention has been called to further operations which have been suggested by these operations, so that the effect is accumulating.

15155. Operations on the nerves?—Operations on the nerves in man.

15156. Had these operations ever been attempted before on man?—No. The operations which are now being carried out are those on birth palsy. When a child is taken out by instruments, it sometimes happens unavoidably that the nerves of the brachial plexus or elsewhere are torn, and that leads to permanent paralysis, which is called birth palsy. This has suggested that one of the central ends of a sound nerve of the brachial plexus should be joined on to the peripheral end of a nerve which has been injured; that has been done, and with success; the child has been saved from lifelong paralysis in that way. There is another disease, called acute anterior poliomyelitis. This is a disease of a certain number of cells in the spinal cord, which causes partial paralysis. There again the nerve which has these unsound diseased fibres is joined on to the central end of a portion of a sound nerve and that has also led, so far as it has been carried out, to satisfactory results.

15157. When you speak of the central end of a nerve, what exactly do you mean?—I mean the end that is connected with the spine. Then again in athetosis, which is a continued spasm of certain muscles, the flexor muscles of a limb are sometimes very much stronger than the extensors, and cause various twitchings and tonic spasms. What has been done has been to take the nerve from the flexor side and join it on to the extensor side, so that the excess of action which goes to the flexors is diverted to the extensors, and a balance is fairly maintained. That also, so far, is a success.

15158. How do you regulate the extent of the operation in accordance with the extent of the excess?—That can only be done, a little empirically, by estimating how much stronger the muscles are on one side than on the other, and taking the corresponding portion of a nerve to join across; the one is grafted on the other. A portion of the flexor nerve might be cut out, and the extensor nerve drawn across and joined on to the flexor nerve.

15159. Has that been done?—The operation has been done by Spiller, Frazier, and Van Kaathoven, and the account of it is to be found in the American "Journal of Medical Science," 1906.

15160. Is that of practical benefit to the person who has it done?—It is.

15161. Is the defect from which he was suffering a serious one?—Yes, very serious. These operations are of the very greatest benefit to people on whom they are performed.

15162. It is not a very common operation, is it?—I can hardly say how common they are. At present there are certainly not a great many operations being performed; it is entirely recent.

15163. In your view, are all these operations the consequences of experiments on animals?—I could give you a statement of Dr. Frazier, who wrote the paper in the "American Journal," and who is partly responsible for these operations.

15164. As you have given us the reference it will be enough, I think, if you can say that he does or does not attribute them to experiments on animals?—He attributes them to experiments on animals.

15165. And that will be seen on reference to his paper?—Yes. Lastly, there is the operation for partial hemiplegia, which is allied to these, and which hitherto has been quite intractable to treatment.

15166. Has that been remedied by operations?—In the few experiments which have been made the operation has distinctly benefited the patient.

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15167. It may be said that these operations might probably have been discovered without experiments on animals, although, in fact, the possibility of them was discovered by that means. Does it occur to you that such might have been the case?—It is impossible to say that they might not have been discovered in the next century, but they would not have been discovered in this. One can only judge by seeing the progress of past inquiry, and the progress of past inquiry when it has been confined to man without the co-operation of experiments on animals has been so slow that I think we may legitimately say that without experiments on animals, these things would not have been discovered within any reasonable time.

15168. If they were discovered?—Exactly. It only means that experiments must be made on man or on animals, it is a choice between the two. No doubt, if experiments were made on man the same kind of things could be discovered, because, as I have already indicated, I think that many of the higher mammals are so closely similar, that what is applicable to one is applicable to another.

15169. Have you had any experience of the use of curare?—I have had experience of the use of curare for particular experiments, and having obtained a knowledge of the way to administer anæsthetics, I am confident that there is no difficulty in keeping up the anæsthesia whilst curare is given.

15170. And what seems to be very important, is there any difficulty in the operator knowing that the effect of the anæsthetic is complete throughout the whole time of the operation?—He must start with complete anæsthesia, and he must know from past experience the amount of the anæsthetic which it is necessary to give in order to maintain it.

15171. Must he start with the anæsthetic before he gives the curare?—Yes, before he gives curare the anæsthetic must be given.

15172. And then he must satisfy himself that the animal has arrived at complete anæsthesia?—Yes.

15173. And with a dose that will last for a certain time?—Yes, and which must be repeated. For instance, with chloroform, the chloroform is given at the intervals, and in such doses as his previous experience has shown will maintain complete anæsthesia.

15174. Assuming that he uses adequate care, the care that you have a right to expect from a skilled anæsthetist, would he have any difficulty?—I do not think he would have any.

15175. But he would have to rely on the dose and on his watchfulness?—Yes.

15176. That is to say, he could not see from any signs in the patient anything that would satisfy him?—There is the state of the blood pressure which will indicate, to some extent, the reflexes on the vascular system.

15177. Would pain felt cause an increase of blood pressure?—It would cause a rise of blood pressure, but I should myself always rely on the previous experience of the anæsthesia—how it is produced and the depth of it, and see that it is maintained in just the same way as if the curare were not given.

15178. Is that the precaution you have seen adopted when the curare has been used?—Yes, that is the mode. An anæsthetic is always given in my experience.

15179. Are there any operations on animals to which you can point in which you would say that the use of curare was necessary?—There are two operations which I can mention, which I have myself performed. One is in investigating the origin of the class of nerves called sympathetic nerves, from the spinal cord. These nerves issue from the spinal cord with nerves to the muscles, and in investigating where they originate, the nerve is severed from the spinal cord, and the end which is not connected with it, but connected with the periphery, is stimulated; that is to say, stimulation in a live unanæsthetised animal could not give any pain. That experiment would give extremely uncertain results unless curare were given, because the musclar movement which is caused by the other nerves mixed with the sympathetic nerves obscures the action of the sympathetic nerves themselves. They must be seen by themselves in order

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to be distinctly appreciated—for example, whether they cause constriction of blood vessels. If a muscle is tight and contracted, of course, it causes blanching or squeezing of the blood out of the blood vessels, and so there is a pallor which might be attributed to the sympathetic nerves, but which is really due to the other class of nerves. Then another kind of experiment that I have had occasion to try is that of showing certain nerve fibres going to the sphincter of the stomach, which cause dilation of the sphincter, which cause it to relax instead of causing it to contract. That experiment was done on a rabbit, and in the rabbit there are fibres running to the sphincter which do not belong to the sympathetic system, but which are of the same character as the muscles of the limbs. Those must be put out of action by curare before you can determine whether the sphincter contracts or dilates, or is inhibited when the nerve is stimulated. One could have got no satisfactory proof of the presence of inhibitory fibres in the sphincter of the stomach unless curare had been given. When curare is given the proof is absolutely clear and one or two experiments settle the question.

15180. We have had suggestions made that even if experiments on some animals are permitted they ought not to be permitted on dogs. Have you anything to say on that point?—In all experiments on animals, of course, one would naturally prefer, for various reasons, sentimental reasons, to avoid the use of higher mammalia. The same sentiment which makes one hesitate to experiment at all applies with increasing stress the higher you go in the scale of animals, ending, of course, in man himself; but experiments on dogs I think sometimes are absolutely necessary. Of course, on general grounds it is clear that physiological laws must have a broader basis the more different kinds of animals are investigated, and to remove dogs from the category would be to limit the general application and determination of physiological laws. Professor Starling has already given a number of instances in which dogs are required for experiments, and I need not repeat those. I would only mention two which are of immediate interest to me in my own work. One is in relation to the sympathetic nerve fibres which run to the lips and to the gums. In no other animal can that be properly investigated, since vaso-dilator fibres in this region are much more developed in the dog than in any other animal, and it is necessary, I think, to perform experiments on dogs in order to arrive at any satisfactory result.

15181. With what object?—To find out the general relation of the vaso-dilator nerves in the sympathetic system. It is the only case, as it happens, which is definitely known, which can be shown, in which there are vaso-dilator nerves in the sympathetic system at all; and the following out of that problem may lead to very interesting and very important results. At the present moment, of course, one cannot definitely say what is likely to come from it.

15182. It is research really?—It is research. And the second instance also is a piece of research work of the same kind, that is to say, as to the presence of vaso-dilator fibres in the posterior roots of nerves. These may be of great physiological importance, but there is so little known about them at present that it is almost open even to doubt their existence, and it is certain that the investigation of them, so far as one can see at present, must be performed on dogs, and that no other animal will adequately meet the case.

15183. As regards teaching by means of demonstration experiments on animals, you practice that to a certain extent yourself, and therefore I presume that you approve of it—that you think it necessary?—I approve most strongly of giving a certain number of demonstrations to students. I think they ought to see in the first place an experiment, under anæsthesia, so that they may judge of it themselves, and form a clearer idea of the working of the body. They cannot see it in any other way. It is always recognised that wherever you can show a thing, the fundamental truth of it is much better appreciated if it is seen rather than if it is simply read about, and in my experience the students take the greatest, but a serious interest in the demonstration, and I do not understand at all how anyone who has been familiar with students attending those demonstrations can say that they have any demoralising influence whatever.

15184. Have you seen any symptoms of it at Cambridge?—Absolutely none.

15185. Your classes almost entirely consist, I presume, of those who are either going into physiology as a profession, or are going to practice medicine as a profession?—Yes.

15186. About how many would there be in a class?—About 100.

15187. Would only a certain number of those be present at demonstrations?—They would all be present at demonstrations—*i.e.*, the demonstrations are open to all.

15188. Could they all see the demonstration?—If it is a case where they cannot all see, it has to be repeated in two sections.

15189. (*Sir Mackenzie Chalmers.*) That is to say on two animals?—On two animals.

15190. (*Chairman.*) Have you seen any sign of heartless conduct on the part of students—exhibitions of jesting, I mean?—I think it is quite the reverse; that they are all extremely anxious to assure themselves that the animal which is there has no trace of pain. One of the things which I generally show in a course is some of these reflexes during anæsthesia, because as a rule they are not satisfied on the first view that the movement can be carried out in a wholly anæsthetised animal. They always press home the point, and that is usually shown to them in such a manner that they can satisfactorily convince themselves of it. I am sure that there is no levity of demeanour; that they are extremely impressed with the necessity of avoiding the infliction of any unnecessary pain.

15191. You think that the public opinion of the students would be quite against any exhibition of levity?—Entirely. It would, I think, be impossible.

15192. Have you any suggestions to make with regard to amendment of the present Act?—I have some suggestions to make on the lines that I think that if precautions have been taken to prevent severe and prolonged pain, and to prevent any unnecessary pain, it should be the object of the Act rather to encourage than to discourage experiments on animals. I think in the first place that the interference with research, which is caused by the necessity of taking out certificates in addition to a licence, should be lessened. I would suggest that after a licence has been held for, say, ten years, it should ordinarily be endorsed with permission to make experiments under any of the certificates, a return, of course, being made of such experiments as are performed. In the next place, I am inclined to think that the injection of non-irritating substances under the skin, and feeding experiments when they are not calculated to give pain should be excluded from the Act altogether. I think it creates a good deal of prejudice to see that a large number of experiments are made, many of these which are not experiments of a painful nature at all. That experiments merely involving a prick of the skin should be massed together as experiments requiring the sanction of an Act of Parliament does not seem to me to be a satisfactory thing. Then again, I think the necessity of taking out two certificates for an operation on a cat or dog, that is to say, B and E E, or, in the case of a horse, ass, or mule, B and E E and F, should be got rid of, and that one certificate should be substituted. I should, by the way, add the monkey to the same class as the cat or dog.

15193. (*Sir Mackenzie Chalmers.*) Do you mean that the ordinary Certificate B should include the right to experiment on dogs without a special certificate?—If you want to experiment on a dog, there should be a separate certificate, that you should not have to get Certificate B first. Now one has first to get B and then E E.

15194. (*Chairman.*) You would have Certificates A, B, and E E all on one piece of paper, you mean?—So far as I understand the Act you cannot do an experiment on a dog without having certificate B.

15195. (*Sir Mackenzie Chalmers.*) You might have A and E E?—Yes, but that means two.

15196. You might have A and E E or B and E E?—Yes, but you must have two. If anyone is researching and only wants to do experiments on dogs, why should he get Certificate B, which is for experiments on other animals?

15197. Certificate B covers animals generally which are to be experimented on with anæsthetics, and then are to be allowed to recover?—Yes.

15198. Then, if you want to go into the particular category of dogs you get Certificate E E as well?—Could you not go at once to the category of the dog without the intermediate stage of animals generally?

15199. If you had Certificate E E only, the persons who give the certificate would not know whether you want E E plus A or E E plus B?—The certificate might be headed A plus E E; then you would obtain it direct, instead of getting two sets of signatures and having two certificates to fill up. There are various ways in which it might be done.

15200. (*Chairman.*) You mean that it would save time and trouble in applications?—It would save time and trouble to the people who have to sign. It is rather hard, for instance, to take up the time of the President of the Royal College of Surgeons in giving signatures, if it is unnecessary.

15201. And it would save time in getting the certificate, and not waiting for it?—Yes.

15202. Are there any other alterations that you would suggest?—In the certificate for teaching purposes I think that the word "absolutely" before "absolutely necessary" should be struck out, because it is so much a matter of argument what you can say an experiment is absolutely necessary for. It comes to this, that for teaching purposes at present, only experiments which are absolutely necessary can be performed, and the word "absolutely," I think, throws an unfair onus on the holder of the certificate. If it comes to a court of law he does not know what view will be taken as to the meaning of "absolutely" in that sense. Because from the physiological point of view there is nothing that is absolutely necessary. There are things which are advisable, and there are things which are more or less necessary, but I doubt whether in any scheme of education, short of learning to read and write, you could say that anything is necessary, if it is pressed home to a point.

15203. You prefer something nearer to the word "desirable"?—I think if you say "necessary," and leave out the "absolutely," that would be adequate. People would then understand it; they could say it is necessary to their functions. There are many things which you can say are necessary, but I think there are very few things which you could say are absolutely necessary.

15204. (*Sir Mackenzie Chalmers.*) The Act assumes that certain experiments will be absolutely necessary, or it would not have made that provision?—Yes, it is an alteration in the Act which I am suggesting.

15205. The Act assumes that certain experiments must be absolutely necessary, because it makes those exceptions, and leaves you to determine what they are?—I suggest that that is throwing an unfair burden upon the licensee.

15206. (*Chairman.*) I understand that your view is that the condition should not be that you have to prove the necessity, but the desirability in the interest of science, or some words of that sort?—Yes.

15207. (*Sir Mackenzie Chalmers.*) What word would you substitute?—I should leave out "absolutely."

15208. You would say "necessary"?—Yes.

15209. But not "absolutely necessary"?—Not "absolutely necessary." It seems to make too much of a different grade of proof required in one case than in the other.

15210. (*Mr. Ram.*) It would eliminate a large number of cases if you were to keep the word "absolutely"; there are cases which you might say would be necessary?—I think it would remain purely arguable. Then a point which has probably been brought up already is that an animal which has been operated upon under anæsthetics and is allowed to recover should be allowed to be kept in any place, provided that notice of a change of place is given to the Inspector, and that the Inspector has the right of entry to the place, wherever it is. That is really in the interests of the animals; especially in the case of dogs; that is to say, if they are kept in the physiological laboratory it not infrequently happens that the place is less satisfactory than would be some place outside a town.

15211. (*Chairman.*) You mean for their recovery?—For their comfort.

15212. Do you mean by reason of the dog associating the place with the operation that it has undergone?—No; it is for their comfort and well-being.

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15213. (*Sir Mackenzie Chalmers.*) You mean that your laboratories are not good places in which to keep the animals; they are necessarily rather dark and rather confined?—Yes.

15214. (*Mr. Ram.*) And you would give them a happier life elsewhere?—Yes, much happier. I have known people, for instance, who would like to take them home and see after them there, but they are not allowed to do it.

15215. (*Sir Mackenzie Chalmers.*) To give them fresher air, in fact?—Yes.

15216. (*Chairman.*) I see what you mean. You would take, I suppose, their address before they left?—Yes. Then I am inclined to think that whilst the animal under anæsthesia under a licence should, as now, be killed, of course, before recovery, anyone holding a licence ought to be able to experiment on that animal during the anæsthesia, because it would save another life.

15217. When it is going to be destroyed?—Yes.

15218. (*Mr. Ram.*) Under a lethal dose?—Yes.

15219. (*Sir Mackenzie Chalmers.*) Anyone holding a certificate, you mean?—Yes, because it merely saves another animal being killed, and, so far as that goes, I take it that is an advantage. If the animal is properly under anæsthesia, I cannot see that there is any objection to anyone who is properly licensed to do an experiment beginning at that stage instead of beginning a little earlier.

15220. (*Chairman.*) Would you have the second operation that it is to be subjected to described, as well as the first?—I think that anyone who does an operation must make a return of what he has done, but that in counting up, in the summary which gives the number of experiments, it should be counted as one experiment, because the number of experiments is to be considered as the number of animals, and you want to get the statement of the number of experiments to coincide with the facts of the case as to the number of animals which have been experimented on.

15221. (*Sir Mackenzie Chalmers.*) Or, rather, with the number of times that anæsthetics are administered. Surely it is two experiments, if you put an animal first under an anæsthetic and experiment upon it, and then let it recover, and then do a subsequent experiment?—Yes; I am talking, of course, of an animal experimented upon under a licence, and never to recover.

15222. (*Chairman.*) You mean when a dog under a licence has been given a dose of anæsthetic, from which it is never to be allowed to recover?—Yes.

15223. Whilst it is under that anæsthetic you would, if it was desirable in the interests of science, perform more than one operation?—Yes.

15224. (*Sir Mackenzie Chalmers.*) There is nothing in the Act to prevent that at present, is there?

15225. (*Chairman.*) I rather think that a witness has objected to that as illegal. (*To the Witness.*) Is there anything else you wish to say?—The last point I am not quite clear about, but apparently under the Act, a technical infringement of the Act of a very minor character may really mean an action at law with a threatening of a penalty of £50 and imprisonment. In an Act of this kind, which is to prevent pain, but at the same time to forward science and humanity, I think that no merely technical infringement should be liable to such a penalty—that it should be excluded from the Act and dealt with by the Home Office. Some of these infringements are really comparable to, say, in a Government Office, coming at 11 o'clock instead of 10. There may be ignorance of a thing, but no one would think of threatening a public employee with a fine of £50 or imprisonment for six months because by chance he came at 11 o'clock instead of 10 in the morning.

15226. (*Sir Mackenzie Chalmers.*) Practically that is secured, because no prosecution can be instituted against a licensee under the Act for anything done in contravention of the Act except by leave of the Secretary of State?—I am aware of that, but could it not be made a little more clear? I think that there are persons who are anxious to take any chance infringement of the Act, of however technical a nature, and

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claim of right that under the Act, as it stands, there can be a prosecution, and therefore the Home Office ought to prosecute. It seems to me that people ought not to be put in that position.

15227. I do not think that the Act contemplates the Home Office prosecuting, because the Home Office is the authority that gives leave for a prosecution.

15228. (*Chairman.*) I think what you mean is that there might be pressure brought to bear upon the Home Office to allow a prosecution?—Yes.

15229. And you think that a prosecution in such a case ought not to be allowed?—That is so.

15230. It is not a penalty of £50 necessarily; it is not exceeding £50. However, I see what your point is. Is that all that you wish to state?—That is all.

15231. (*Sir William Collins.*) How long have you held a licence?—Since the beginning of the Act, so far as I can remember.

15232. Since 1876?—Yes.

15233. What certificates do you hold?—Certificates B, C, and E E.

15234. (*Sir Mackenzie Chalmers.*) You do not do experiments under Certificate A?—Never.

15235. (*Sir William Collins.*) And I think you told us that your experiments have been uniformly painless?—Uniformly painless.*

15236. What animals have you employed?—A variety of animals—birds, rabbits, cats, and, rarely, dogs—very rarely dogs.

15237. Could you give us any idea of the number?—Of course, they are all in the return. Last year I should think none on any of those animals which I have mentioned. All my experiments last year under Certificate B have been on frogs. Perhaps something between twenty and forty experiments, including all kinds under a licence and under certificates, in a year, including frogs, rabbits, cats, and dogs—the whole thing.

15238. And did I rightly understand you to say last year none except on frogs?—Last year and this year. (The details are in the return that I have made). I have done very few experiments except on frogs. The number varies from year to year according to the line of experiments that one is carrying out.

15239. Have you employed any dogs this year?—Two.

15240. Do you employ dogs for demonstration purposes?—No.

15241. Do you think it necessary to do so?—For some experiments I think it is necessary. These demonstrations that I give are generally on the cat and the rabbit.

15242. Do you give a complete course of physiology?—Yes, I give a complete course of physiology. Perhaps my demonstrator may show an experiment on a dog. He does some in addition to those that I do.

15243. You do not yourself employ dogs for demonstration?—In the laboratory they are used sometimes for demonstration, but it is not very common.

15244. For class demonstrations?—Yes. I should think, perhaps, one a year.

15245. For what demonstrations?—For the flow of chyle, because that can hardly be seen in any other animal than a dog, to show that after feeding there is a large amount of material passing from the various parts of the body, absorbed from the intestine and flowing into the blood, and that it is milky. The amount of the flow is a very impressive thing in absorption.

15246. (*Mr. Ram.*) Would the dog be under a lethal dose of anæsthetics?—Yes.

15247. (*Sir William Collins.*) Professor Gotch, of Oxford, told us that he did not use now dogs at Oxford for his vivisection experiments?—No doubt the practice varies in different places.

15248. I was anxious to get from you whether you think that it is essential to employ dogs for purposes of demonstration to students?—It is certainly essential if they are to understand how the lymph flows in the body that they should see it. You can explain it, but that is a thoroughly different thing from seeing that particular experiment.

15249. Is that the only experiment that you show on a dog to a class?—So far as I remember, it is the only experiment that we show to a class. We might have, for instance—I do not know that it has been done, but it might be done for the more advanced class—we might have an experiment on the vaso-dilator nerves to the lips or gums.

15250. As a matter of fact, have you demonstrated that before a class?—Not to my elementary class. Whether I have done so to the advanced class I should hardly like to say. I should have to look up my records.

15251. You have drawn a distinction between painful and painless experiments. We have been told that the Home Office returns formerly made such a distinction, but that of late years that distinction has been given up, and I understood for the reason that it was sometimes difficult to make such distinction. What do you say in regard to that?—The degree of pain is, of course, always a relative thing. I do not think that there is any severe pain in the operations of which I have any personal knowledge. There is discomfort when a wound heals; there is certain to be discomfort; and it is rather a matter of nomenclature where discomfort passes into pain. I have indicated the difference, so far as I could, by saying that some of my experiments were clearly not painful, and some of them were of the nature of a severe operation in a hospital, and those I should think possibly at times do cause pain, as they do in a human being.

15252. Your work is limited to physiological research, I understand?—Entirely.

15253. You have suggested certain possible amendments in the Act, but I did not quite gather whether you suggested that the Act itself was undesirable or should be repealed?—I would much rather have the Act than no Act. I think it would not be fair to the animals to allow anyone to experiment upon them without control. I think it is a very natural desire that there should be some control of experiments, which might, if carelessly conducted, inflict pain, and I thoroughly approve of there being some control.

15254. And you desire that anæsthesia should in all possible cases be employed?—Most certainly.

15255. And you believe that, at any rate within your own experience, it is so employed?—I believe that it is so.

15256. We were told by a lecturer on physiology, in answer to Question 14115, that the Vivisection Act was entirely opposed to the advancement of physiology, and when he was asked whether that was an opinion held by others engaged in physiological research besides himself, he replied "Yes, I think so." What do you say in regard to that?—It cannot, I suppose, be denied that the working of the Act has to some extent interfered with the advancement of physiology, because there is the difficulty and trouble of getting certificates. In research of any kind the way in which it is carried out is really to go red hot on ideas as they occur to you. If you have to wait for three weeks to get a certificate, a particular idea which occurs to you is probably never carried out, and you go on with the humdrum and immediate things which are in your line of work. If you have everything ready, then some idea which may be very fruitful you can test by observation, you can test it if you can do so at the moment, but if it has to be put off you probably never test it. So far as that goes, I suppose it would be said to interfere, but I certainly do not think that the interference with the advance of physiology has been a really serious one. Physiology has, in fact, advanced very greatly in England during the operation of this Act, and, I believe, with a diminution in the amount of pain which has been inflicted.

15257. May I take it from that that you are not of opinion that the Vivisection Act is entirely opposed to the advancement of physiology?—I should not say that it was entirely opposed. I should say that it was only partially opposed to it.

15258. A great deal of your research work has been in regard to the nervous system?—A good deal of it.

15259. And you have drawn a distinction between

* *This refers to actual experiments and operations the state after an operation is referred to in Q.* 15251.

reflex actions and conscious action in your evidence to-day?—Yes.

15260. I do not think you gave us any description or definition of what you mean by reflex actions?—If you take a sensitive plant, and you touch it, the leaf shuts up; that is one form—the simplest form of reflex action; so if you take a fragment of the nervous system, separated from all the rest, you get similar little movements of a simple character in response to external stimuli, and if you take a little more of the nervous system, or get a little more complex piece of the nervous system, you get a little more complex movement; so that one follows the reflex actions up from the plant, where they occur in a very simple form, right through the animal kingdom, and it is on the basis of comparison of the behaviour of the simplest forms of life with the most complex that I should draw the distinction between reflex actions and those which are accompanied by consciousness. Eventually consciousness can only be judged of by ourselves. We cannot have any thorough knowledge of anything conscious except as it occurs in man, and in man conscious pain only occurs when he is in possession of certain parts of the central nervous system. If those go there is no longer any power of appreciating pain. Thus you can get an absolute lack of pain where you have a body of which a portion and the nervous system is cut or where there is a lesion of part of what is called the internal capsule; this gives complete anæsthesia on the opposite side of the body. The reflex actions can then build up the mechanism out of the simple cell, but except with certain physiological structures as a basis, there can be no consciousness and no pain.

15261. Is reflex action ever conscious?—It is never conscious.

15262. So that an essential part of reflex action is that it is unconscious?—That is a necessary part of a definition except that reflexes may in their operation also set in action a portion of the brain which is concerned with consciousness, so that there may be another chain set in action, but the reflex in itself goes on in its integrity entirely and absolutely independent of consciousness.

15263. Then when you speak of reflex action, *per se* it is implied in the definition that it is an unconscious act?—It is implied. The term, of course, is often used loosely.

15264. We have often had the term "incomplete anæsthesia" mentioned in the course of the evidence; have you any definition which would attach to that?—If it was incomplete, I should usually say that sufficient anæsthetic had not been given; but those words are not used with very great scientific accuracy.

15265. It is not a recognised physiological term?—I do not think we use it in pure physiology with any frequency.

15266. I understand you to say that the margin between the disappearance of consciousness and the disappearance of reflexes under anæsthesia was different in animals, and different with anæsthetics.

15267. Is that margin very much greater in the case of animals than in that of man?—Much greater. With chloroform you have to watch very carefully to prevent the respiration stopping, which is part of the reflex really, and not conscious; it does not give rise to ordinary sensation unless you attend to it.

15268. Would the teaching to students of anæsthesia in animals be of use in their carrying on such practice in the case of a man, or is the difference in this margin so considerable that it would make it unwise to use such instruction?—I think that would depend upon the intelligence of the student. With a good student I think it would be a great advantage to him, but with a student who would go simply by rule of thumb, it might be rather dangerous.

15269. One witness suggested to us that hypnotism might be employed in lieu of anæsthetics on animals. What would you say with regard to that?—I have done a good deal of hypnotism, but I never succeeded in hypnotising an animal or a lunatic.

15270. The lecturer on physiology at Guy's Hospital said, at Question 14149: "I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of hypnotism, which I offered to show to the Commissioners. That, I think, is one very important point. These animals pass into a condition, so far as one can see, comparable with hypnotism. If you give them anæsthetics you are introducing a complication which you could remove, and therefore, without anæsthetics, you actually save life and actually diminish the infliction of pain." What do you say with regard to that?—You can produce a condition which is similar to that of hypnotism in man in animals, but in an animal that condition cannot be maintained for more than a short time; in certain animals it cannot be maintained for more than a few minutes, and it is always uncertain; you could never rely upon keeping it up. It is not a condition in which you could operate on an animal. In a dog, so far as I know, it has never been produced at all. It is done with pigeons and with other birds and with frogs, but no serious operation could, in my opinion, be performed on an animal in a state of hypnotism. I should be extremely interested to see the condition of an animal where anyone would suggest that you could really substitute hypnotism for anæsthesia.

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15271. The same witness, in answer to Questions 14119 to 14121, gave the following evidence: "Did I rightly gather that in your opinion anæsthetics are unnecessary?—(A.) Yes, I am convinced that they are unnecessary, not only in the case of animals but of men. (Q.) For vivisection and for surgical operations?—(A.) For vivisection and for surgical operations, and I mentioned the case of midwifery. (Q.) Would it be going too far if I were to express your opinion as being that pain is a good thing in itself?—(A.) I say that pain is a beneficent and protective mechanism. I have written that in an article which I have recently published, and I am prepared to defend it. It is not a new view, of course, at all, it is recognised by surgeons, I believe, that a certain amount of pain is a protective mechanism." Is that a view that is generally held by physiologists?—There are two points there, the one with regard to the application of hypnotism to animals, the other with regard to the beneficent action of pain.

15272. Hypnotism was not mentioned in the last questions that I read to you?—In the development of the race pain, of course, has been of value, but what we are all hoping to do at the present time is to diminish the amount of pain in the universe as much as possible. Its beneficence has narrow and restricted limits. In the struggle for evolution, no doubt some excess of pain was almost necessary for the selection of certain individuals from others, but as things are now, I think it can only be said that the less pain there is in the world the better it would be. That seems to me a perfect truism.

15273. Then are you, or are you not, of opinion that for vivisection and for surgical operations, anæsthetics are necessary?—I think that they are entirely necessary.

15274. The return of movements in an animal or in man under an anæsthetic might, or might not, be indicative of returning consciousness, I gather from what you have told us?—The first movements which return after complete anæsthesia will always be unaccompanied by consciousness. The later movements may or may not be accompanied by consciousness.

15275. But in ordinary surgical anæsthesia is not the return of movement not infrequently a reason for administering more anæsthetics?—Yes, it is the beginning sign that more is required, but not that consciousness has returned.

15276. In regard to surgical operations for reuniting a nerve which has been severed, and other experiments to which you referred, do I rightly understand you to say that they have all been dervied from vivisection?—Certainly not. The main leading things have been derived from vivisection, and they have suggested operations on man. The two have been going on concurrently ever since the beginning of the work.

15277. Do you trace the ordinary union of a divided nerve directly to vivisection experiments?—The adoption of it by the surgical profession as a practice was due to the advocacy of it by prominent physicians and surgeons on the basis of vivisection. People had tried the thing long before. I suppose that from the time of the Greeks you will probably find some evidence of surgical operations on the nerves, but up to the end of the last century there were many different views about it, and the prevalent medical view was that it was impossible to cause regeneration of the nerve; and the thing which turned the tide and really directed attention was the early experiments of Cruickshank, Houghton and Fontana.

15278. What date do you put upon them?—From 1770 to 1822.

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15279. You said that the erroneous opinion of Galen had been refuted by these experiments, as I understood you?—Yes, that is putting the thing in a concise fashion.

15280. Should I be wrong in thinking that Galen's opinions, whatever they may have been worth, were not derived from experiments upon animals?—I am afraid I could not offer any opinion as to where Galen derived his opinions from; I know so little about him.

15281. I see that the last Royal Commission says that Galen, in his writings, describes with painful particularity a great number of different kinds of tables which he had on which animals might be fastened, and severe experiments, which he describes, might be performed upon them. Apparently Galen's views, whether right or wrong, were derived from experiments on animals?—Does it say that this particular view of his was derived from experiments on animals?

15282. I was merely asking, as you referred to Galen, whether your attention has been called to that particular passage in the Report of the last Royal Commission?—It may well be that Galen's attention was directed to other points.

15283. When was the first successful reunion of a divided nerve for surgical purposes?—I think it would be impossible to say.

15284. Have you made any researches to ascertain that?—Yes, but the facts are scattered in so many medical papers; there are thousands of papers (of course, there were fewer in the earlier times), that it would take years to settle that point, I think.

15285. Was it a well-understood procedure thirty years ago?—Yes.

15286. Is the regeneration of nerve fibre fully ascertained now?—There are some theoretical conclusions which are still very much in dispute as to how exactly it takes place, but the general facts of regeneration are well established now.

15287. Are there not two rival schools known as the central and peripheral schools respectively?—Yes.

15288. Are you able to decide between them?—I have my own opinion, but I do not suppose that I should convince the other people.

15289. Are you a centralist on a periphalist?—I am less certain than most people, but so far as I go, I am a centralist.

15290. The researches of Ballance to which you have referred rather tended in the opposite direction, did they not?—They did.

15291. Have both schools appealed to experiments on animals for verification?—Yes, both of them.

15292. (*Sir Mackenzie Chalmers.*) I think you are a pure physiologist?—Yes.

15293. You are not a medical man?—No.

15294. Is there a school of physiologists springing up in England who are not medical men—who go in for physiology as a profession, so to speak?—Partly as a profession, and partly because they are men with independent means, who are interested in research for its own sake, and are able to live independently, and are not bound to go into the medical profession.

15295. The question has been raised from this point of view; it does not apply to the man who is chosen as the professor of a great university; but on what conditions ought licences to be granted to people who wish to practice physiology and have not had a medical training?—If they have had a scientific training I should have thought that that was perfectly sufficient. If they have been through courses of physiology in a laboratory, and are working where others are already conducting research, I think every precaution would be taken that there would be no misuse if a licence or certificates were given them.

15296. Do you think that you would have the same guarantees for care and humanity as you would have in the case of a member of the medical profession?—Naturally you cannot have exactly the same guarantees at the beginning as you can after experience; but I think that the guarantees are adequate. I think it would be exceedingly rare that there would be any misuse of the power.

15297. You think that the present authorities who recommend for a licence or grant a certificate have the means of ascertaining the character and experience of a man before they send in their recommendations?—One of those who signs the certificates, I take it, in nearly all cases knows enough to be able to state that the experiments will be conducted with care and proper precautions, but not necessarily both of those who sign.

15298. Another point that has been raised before us is this: It has been suggested that senior students who are going eventually to be teachers of physiology should have licences authorising them to perform experiments on animals under the supervision of the recognised head of the laboratory. Is that important or not, in your opinion?—The object being to fit them for research, or to fit them for study, or to fit them for a medical career? They are rather different aspects.

15299. It may be one or the other. Every one has to begin performing experiments. The question is whether it would be right that a, so to speak, partial licence should be given to a man who is a student to begin, not on his own account, but actually to do the operations himself under the supervision and teaching of a professor?—Yes, I think so, if care is taken that he is supervised.

15300. It was suggested by one witness that the proper precautions would be these: That the animal should be anæsthetised by the professor, that in an anæsthetised condition it should be handed over to the student manually to do the operation, with the professor supervising and seeing that the anæsthesia was kept up?—I think that for many students it would be extremely useful, and if it were conducted in that way I do not think that there would be any danger of any pain being inflicted by lack of knowledge or carelessness; but I do not think that I would give indiscriminate licences to students merely to practice.

15301. No, certainly not. But do you think that, under those precautions, there would be any real, substantial advantage gained? Do you think it is at all necessary?—I think there would be an advantage gained.

15302. I want to ask you one or two questions about Certificate B. You have told us that not merely the initial operation, but the whole of the operation, was performed under anæsthetics?—Yes.

15303. And then the animal allowed to recover?—Yes.

15304. Was it your practice, or is it within your experience, that when the animal is allowed to recover there are any supplementary operations ever required?—None, in my experience.

15305. It is not necessary again to put the animal under anæsthesia; so far as you have been concerned it was not necessary to do any further experiments upon it?—I can conceive that there might be such a case. I cannot recall any case of my own* in which it was necessary to give anæsthetics again, or to do anything with the animal until it was killed under the anæsthesia.

15306. If, after an animal has been allowed to recover under Certificate B, it is found to be suffering, what do you do with it?—It is killed. With regard to your former question, one might, for instance, in nerve surgery, want to give anæsthesia again, and to cut the nerve again, and leave it to recover a second time. That is to say, having observed regeneration, it might be necessary, in order to see how much fibre had regenerated, to cut the nerve and then observe the tracks under the microscope after they had been altered by the process of degeneration; so that there a second operation and a second giving of anæsthesia would be necessary.

15307. As you construe the certificate, if that had to be done, anæsthesia would be applied again as a matter of course?—Yes.

15308. It has been suggested to us by some witnesses that certain animals, having very much more acute sight and hearing than human beings, have also a more acute sense of pain. Is there any justification for that suggestion?—Every-day experience of the behaviour of animals which have large gashes and skin wounds is directly opposite to that. I should say that that is a question on which every one can form his own opinion.

15309. Physiologically, is there any connection between either the senses of sight, hearing, or smell, and the sense of pain?—Physiologically, all the apparatus for discerning pain is much less in the lower animals than it is in the higher.

15310. You may have increased sensitiveness of

* *On reference I find that I have, in a few cases, given anæsthetics a second time, in investigation on nerve degeneration and regeneration.*

hearing and of sight quite independently of that of the perception of pain?—Quite.

15311. In your experiments I suppose that you use general anæsthesia, not local anæsthesia?—I generally use the A.C.E. mixture.

15312. Taking the A.C.E. mixture or taking chloroform by itself, does the animal suddenly pass from acute sensitiveness to pain to absence of pain, or is it a slowly progressive diminution?—It is a very gradual process, beginning with diminution of the power of feeling pain, and going on to disappearance of pain, and then a gradual diminution of consciousness, and so a gradual action on all the rest of the nervous system. It is gradual throughout.

15313. There is no sudden crossing of the gap between consciousness of pain and unconsciousness of pain?—No, none.

15314. Have you by chance read Miss Hageby's evidence of some physiological demonstrations which she saw in London University?—I have looked through her evidence. I cannot say that I have read it.

15315. Her impression was that the animals there were imperfectly anæsthetised, because she saw certain movements. Can you form any opinion yourself as to whether those movements were reflex or conscious movements?—From what I have seen myself in Professor Starling's laboratory, I should feel certain that they were reflex movements. But I have not read the exact account with sufficient care to give evidence upon it, and can only answer upon general grounds.

15316. You have told us that your own experiments have been chiefly on nerves. There are many nerves, I suppose, which are wholly unconnected with the sensation of pain, which you can stimulate without producing pain?—Certainly.

15317. But are the nerves themselves which are not the subjects of pain so inextricably woven up with other nerves which are there, that you cannot stimulate them without causing pain?—If a nerve that is severed from the central nervous system is stimulated, it cannot by any possibility cause pain. The majority of stimulations in experiments on animals are stimulations of the peripheral end after it has been cut away from the central end.

15318. (*Mr. Ram.*) And that has no sensation?—It has absolutely no sensation. It is impossible that it should have any sensation.

15319. (*Sir Mackenzie Chalmers.*) We have been told by some witnesses that in operations on animals which involve opening the intestines, the intestines themselves are very slightly sensitive. Is that so?—In ordinary cutting operations they are always insensitive, and it is difficult to find that they react at all in such cases as cutting with the knife. On the other hand, it is known that when they are in an inflammatory condition, they are painful.

15320. For instance, the intense pain of peritonitis?—Yes, that, I take it, is one of the most painful diseases, but there are numerous instances in which the intestine is cut under very light anæsthesia, just over the border of loss of consciousness, when it produces no indication at all that severe pain such as might cause a return of consciousness is produced, there is nothing there to indicate that the section or suture of the intestine is capable of giving rise to pain.

15321. Then in these insensitive internal organs, how do you get intense pain of cramps, the cramps of cholera, for instance?—In that case, they seem to be sensitive to a particular kind of stimulus which one may regard as a special stimulus of the intestine; thus a certain degree of strong contraction causes pain, but cutting and burning are not painful as a rule in the viscera.

15322. Are the same nerves sensitive both to heat and cold and to pain?—No, I think they are a different set. That is one of the questions on which there is considerable difference of opinion still as to whether where the stimulus increases in intensity in a nerve which gives rise to cold, say, or to heat, and then it passes into pain; or whether there is a different form of nerve fibre which comes into action when pain is concerned. That is a matter which is not definitely settled, but in which the balance of opinion is in favour of a specific different kind of nerve.

15323. (*Sir William Collins.*) Do you mean a morphological difference of fibre?—No doubt it is connected with morphology—that one kind of nerve cannot give rise to pain under any circumstances, it only gives rise to cold; and that a nerve which gives rise to pain cannot under any circumstances give rise to cold, it only gives rise to pain.

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15324. (*Sir Mackenzie Chalmers.*) The two are so closely interwoven, then, that you cannot separate them?—They run together, and it is difficult to separate them. You can get little points of the skin which only feel cold, and other little points which only feel pain or pressure.

15325. It was suggested by a witness last week that in certain cases a slightly painful operation is preferable to the use of anæsthetics. I suppose that often in mankind that is very common?—I should think it was common, and I think that it is so in some experiments on animals—all those very slight experiments. It is obvious that the puncture of a needle is perfectly trivial, whereas the giving of anæsthetics is generally accompanied by discomfort.

15326. It depends, therefore, upon the nature of the operation whether an anæsthetic should be given or not?—Yes.

15327. But I suppose you would say that as an animal cannot speak for itself, the safe thing is to give an anæsthetic whenever there is a cutting operation?—Yes, that must be regarded as a guarantee for the sake of the animal, because the infliction of discomfort, after all, is not a serious matter, whereas the casual infliction of pain is.

15328. A human being can be asked whether he would rather have the slight pain of an operation or the discomfort of an anæsthetic, whereas an animal cannot?—That is so. Where the operation is clearly not painful, as in a simple puncture with the needle, I think it is obvious that anæsthetics should not be required.

15329. That is to say, under Certificate A?—Yes.

15330. You mentioned showing certain what I may call advanced experiments to certain advanced students. Do you allow any of your students to see some of your research experiments, or only the demonstrations?—Occasionally they may see research experiments.

15331. And that obviates the necessity of having a Certificate A?—Yes.

15332. (*Mr. Ram.*) There are a few points that I want to ask you to be good enough to tell me about, please. In the nerve regeneration operations that you have told us of, would the process of healing be necessarily painful?—No.

15333. Sometimes?—I do not think that the process of healing of a nerve is ever painful unless there is inflammation, or unless there is sepsis.

15334. If you use an antiseptic there is no pain?—There is no pain, I think.

15335. You spoke of a certain number of operations in which, when the animal recovered, there would be discomfort, and of a certain other number in which the discomfort would pass into pain. What sort of operations would those be; can you indicate a specimen?—Operations on the central nervous system, I think, must generally be accompanied by some kind of pain, but certainly not always.

15336. Then the number of cases in which during recovery any pain is liable to be felt is, in your opinion, very small?—I think it is small. In the removal of a portion of an organ, for example (supposing it were endeavoured to discover with how little of an organ an animal can live with) there may be pain. I think an experiment like that, to the minimum of organ left, will probably cause some pain. Some of the experiments made in the extirpation of thyroid, which led to the introduction of thyroid treatment, I think caused pain.

15337. And would that be pain merely while the wound was healing, or even after recovery, in consequence of the altered state of the organ?—In the case of the central nervous system, it would be while the wound is healing. In the case of the removal of an organ, it might be the result of the actual removal—in the alteration in nutrition that was brought about.

15338. Then probably in that latter case the animal would be in some discomfort or pain as long as it lived?—In some cases I think it might be.

15339. What would you do yourself in your own practice in such a case as that. Should you consider yourself justified in keeping the animal alive indefinitely?—Not if it were in severe pain.

15340. You would judge of the amount of pain?—Yes.

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15341. And you would put the animal out of pain if you considered the pain severe?—Yes.

15342. That, I suppose, is a matter which must be left, at all events in the present state of the law, to every operator to decide?—I think so, but, as I have said, I have never seen such a case, and I think they are rare.

15343. But if you found such a case you would your self put the animal out of its pain?—Yes. Such experiments as those are not made generally for scientific purposes, but are directed to the study of some disease which it is desired to elucidate in man.

15344. And to that end it is necessary to allow the animal to live?—It is necessary to allow the whole disease to work through.

15345. Have you ever found the inspection which at present exists under the Act any nuisance or hindrance to you?—No, I cannot say that I have found it any nuisance. The Inspector comes at irregular times and walks round the laboratory and asks to see everything that is there in the ordinary course. That takes up an hour or so in the morning.

15346. That is the only trouble?—Yes, that is the only trouble.

15347. Save in so far as there might be a hindrance to you in your work and in occupying your time, should you mind having the inspection increased?—No. Of course there is a limit. It would be a great hindrance not to be able to experiment unless someone was there.

15348. I was not upon that?—But if by an increase of inspection you mean that more people could come in at any time they liked I have no objection whatever to that.

15349. Do you think that it would be any hindrance to scientific research if an animal which had undergone an operation which might cause pain and survived was to be inspected periodically by the Inspector?—No, I think there is no reason why the Inspector should not inspect as much as he chooses.

15350. Now, with regard to morphia, if I understand you rightly, morphia not only does not prevent, but actually at times occasions reflex action?—At times it occasions reflex action; that is to say, it does not in itself occasion it, but it puts some of the lower parts of the nervous system into a condition in which they react more readily than if they are in a normal condition.

15351. Morphia given in sufficient quantities is an absolute anæsthetic?—I think it is an absolute anæsthetic.*

15352. But an animal under absolute anæsthesia from morphia would be more likely to show reflex action than under any other anæsthetic?—Most animals if you give them enough go into convulsions under morphia.

15353. Is that also the case where morphia is used in conjunction with other anæsthetics?—No, it is not the case.

15354. The other anæsthetics control the reflex action?—Yes.

15355. Now with regard to curare, in your opinion, are there certain cases in which if the necessary object is to be attained, curare must be employed?—I think it must be employed.

15356. Those cases, some witnesses have told us, are very few. Do you agree with that?—It depends upon the line of research. Taking the whole field of research they are very few; but any particular line of research might want many.

15357. It would depend upon what the object of the operation was?—Yes.

15358. And it is principally, is it not, in operations dealing with nerve investigation that curare would be necessary?—Yes.

15359. When curare is used, I rather gathered from your evidence, that many of the indiciæ of pain are absent?—They are.

15360. Therefore, as you expressed it, if I understood you rightly, the operator, in order to be sure that the animal is not suffering when under curare, must be certain that the dose he had previously given of anæsthetic is sufficient to obviate all possibility of it?—Yes.

15361. In the hands, therefore, of a careless or inexperienced administrator of anæsthetics, or operator, the presence of curare might involve suffering unperceived by the operator?—I think it would be gross carelessness, because the operator must be aware of the necessity of giving continuous anæsthetics.

15362. Do you see any objection to its being enacted that there should be a separate certificate to authorise the use of curare?—No. I think, of course, that all increase of certificates is so far a drawback that it tends to prevent people from doing experiments which they otherwise would do; but if it tended to alleviate public opinion on the question I should not in any way object.

15363. It was on that ground that I was suggesting it to you?—In itself I think it would be unfortunate, but there are many things which are unfortunate which have to be done, because of public opinion, which does not understand the thing thoroughly.

15364. For the alleviation of what I would venture to call a sensible, reasonable, public opinion, you, having regard to that, do not see any objection to having an additional certificate to authorise the use of curare?—Of course, I would rather that there were not one, because I do not think that curare is abused; but I should offer no strenuous opposition to it if it is considered advisable on what I may call the politic ground. I should only object if I thought that it might be taken to imply that curare was misused at present.

15365. Do all the animals that have curare given them die under the anæsthetic?—Yes.

15366. In no case does the operation admit of recovery?—No. There are instances recorded in the early experiments of Waterton, who discovered the poison. In practice they die under the anæsthetic.

15367. Having regard to the fact that the cases in which curare is used are comparatively few, taking the whole field, and that there is this risk of which you have spoken, do you think it would be any impediment to scientific research if it were enacted that the Inspectors should be present in all cases where curare was used?—I think it is an impracticable thing to have an inspector present when all experiments are going on, because it means that research is so retarded that it becomes practically impossible to carry it out.

15368. You observe that I limit my question to cases where curare is used?—Yes, but one never knows exactly when you want to use it in a particular kind of experiment, and the Inspector is probably not on the spot. You have to arrange weeks beforehand. In the meantime some committee meeting or something happens, so that either you cannot do the experiment, or something happens to the Inspector and he cannot come. I think it would be very difficult. And then, although in a general way I acknowledge the risk, it is on theoretical grounds. I have no reason to suppose that it is a practical thing.

15369. You have suggested as one of the alterations or improvements of the present Act the elimination of non-painful experiments, such as feeding experiments, which do not involve pain, and sub-cutaneous injections, and so forth. Could you give us any kind of definition of those experiments? Have you thought of any expression which would serve to include and express such non-painful experiments as that?—"Experiments not calculated to cause pain" I should have thought would have covered them. The Act is for those calculated to cause pain.

15370. You think that some such general expression as that would be broad enough—I should perhaps say narrow enough?—Yes—not calculated to give more pain than is caused by the prick of a needle.

15371. (*Chairman.*) Supposing that the injection causes fever?—I think I suggested the injection of non-irritating substances.

15372. (*Sir Mackenzie Chalmers.*) That would not cover the case of a substance which afterwards produces a painful disease, as, for instance, plague?—But that requires a special licence for it afterwards. But it would not require Certificate A to perform an

* *By this I mean that morphia has produced complete anæsthesia in the cases and under the conditions in which I have used it and seen it used. Beyond this I can only offer an opinion based on the statements of others; my own experiments do not cover the whole ground.*—J.N.L.

operation on an animal under anæsthetic. The experiment in that case would be one for the investigation of plague. Those experiments for the investigation of a transmissible disease seem to me to require a special certificate. The injection is an incident.

15373. (*Mr. Ram.*) The initial stage in giving an animal a sub-cutaneous injection of plague serum, say, will no doubt cause no pain to the animal, but the subsequent condition produced thereby might be very painful?—If anyone is going to investigate plague or some disease he should have a certificate, I think, that it is for the investigation of the disease, but not for the injection of a substance under the skin. It is rather striking at the wrong point.

15374. I quite follow your point. I was trying to see whether you could help us to formulate anything to carry it out.

15375. (*Sir William Collins.*) You mean a minor operation that is not followed by any morbid sequelæ?—Yes.

15376. (*Mr. Ram.*) Such as injecting a mouse or guinea-pig with milk supposed to be tuberculous?—Yes.

15377. The operation itself would not be painful at all?—No.

15378. I just throw it out in case you might be able to assist us.

15379. (*Chairman.*) One way might be to give power to the Home Office to classify certain operations as not being painful within the meaning of the Act?—Yes.

15380. (*Mr. Ram.*) I dare say if you think hereafter of any definition—you see the difficulty—you will let us know, if you can assist us to formulate anything to meet what you propose?—With pleasure.

15381. There is only one other matter, I think, I need trouble you upon. I think either in your *précis* or in your pamphlet you referred, but only made some allusion, to infantile paralysis. Is it to be hoped that these experiments with regard to nerve regeneration may be of assistance with regard to the discovery of the causes and perhaps finding the cure for infantile paralysis?—The causes are generally of a surgical nature, and are in many cases well known; but the experiments will be of benefit for the cure of the disease, or, at any rate, for the alleviation of the condition. In other words, the paralysis will be diminished or almost abolished.

15382. That is what I wanted to get at. It is pretty widespread, is it not?—There is a good deal of infantile paralysis.

15383. And when it comes in it is apt to maim or perhaps blast the whole life?—Yes.

15384. Do you think that the experiments which have been and may be made in these matters, will be likely to assist in curing or alleviating infantile paralysis?—I think so, certainly.

15385. Has anything been done in that way at present?—It is just beginning. Some experiments have been made, those that I have quoted by Frazier and others.

15386. (*Dr. Gaskell.*) I understood you to say that in demonstrating to your class you made a special point of administering anæsthetics?—Yes, they are generally shown the anæsthesia, the method in which it is produced, and the condition is pointed out.

15387. And stress is laid upon the actual condition that the animal is well under anæsthesia in consequence of the dose that has been given?—Yes. In any particular demonstration the question of the anæsthesia might be entirely left out if it has been done on a previous occasion. There would probably be one demonstration in which attention was specially called to the anæsthesia, and in a subsequent one it would be taken for granted.

15388. Have you read the past evidence before this Commission at all; have you read Mrs. Cook's evidence?—I read it just after it appeared, but I have not looked at it since.

15389. As she has brought in your name, I should just like to have a statement on your part. She says at Question 1785: "In the second volume of the 'Journal of Physiology,' page 267, will be found an account by J. N. Langley, M.A., from the Physiological Laboratory at Cambridge, of an experimental operation on a dog. The dog was narcotised with morphia. I believe I shall have expert support if I say that morphia is not an anæsthetic. The proceedings by which this dog was most cruelly tortured lasted between three and four hours"; she then describes the experiment. Have you anything to say with regard to that statement?—That is an example of the misrepresentation on which so much of the anti-vivisection opposition has been founded. There was absolutely no torture; the animal was perfectly quiet and entirely unconscious throughout. The action of morphia is not understood, of course, completely, and until we completely understand it we shall not be able to avoid this kind of misrepresentation. But there is no kind of doubt that morphia, just as it abolishes the severe pain of neuralgia, peritonitis, and other diseases in man in a small dose, can in a larger dose prevent pain in animals.

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15390. There was no evidence whatever of anything in the nature of the animal being cruelly tortured or any sign of pain in that animal?—There was none.

15391. Then there is another statement: "In the 'Journal of Physiology,' volume VI., is an account of how a cat was alternately suffocated, cut, and its nerves exposed and irritated with electric shocks, without intermission, for over two hours and a half. It is stated that chloroform was given at the beginning of the operation; but it is not possible that the animal was kept in a state of insensibility for more than two hours and a half." That statement we have had put before us again and again of the impossibility of keeping an animal anæsthetised for so long a time as two and a half or three hours. Would you just tell us whether there is any truth in that?—It must be the revival of some statement made somewhere by someone who did not know what he was talking about—that animals cannot be anæsthetised and kept under anæsthesia indefinitely. I say undoubtedly that you can do it for many hours. I have seen animals untied and unfastened without producing any kind of movement in deep anæsthesia for hours together.

15392. (*Chairman.*) Was that operation on the cat one of yours?—Yes.

15393. (*Sir Mackenzie Chalmers.*) As regards that particular cat, you have no doubt whatever that during the whole of the operation it was absolutely unconscious?—I have no doubt of it whatever. If you notice the reference to nerves, all the nerves, as I have mentioned before, are those which are stimulating the peripheral end, which could not give pain.

15394. (*Dr. Gaskell.*) You suggested that when a licence has been held for ten years permission to go on experimenting should be endorsed, and no certificates required. I presume that you mean by that that during those 10 years the licence has been used continuously?—Yes, I mean that guarantees have been given of earnest work, and that the work has been conducted under conditions which have satisfied the Home Office.

15395. Do you think that as long as ten years is necessary?—No, that was merely a suggestion. I do not specify any particular term.

15396. I should like to know whether you have seen Dr. Pembrey's evidence, to which Sir William Collins referred when he was questioning you?—I have not.

15397. After the remark which Sir William Collins mentioned at Question 14115, that the Vivisection Act was entirely opposed to the advancement of physiology, the next question is: "Is that opinion held by others engaged in physiological research besides yourself," and he answers "Yes, I think so." You do not agree with that?—I certainly do not agree with that.

15398. You do not consider that Dr. Pembrey would be representative, in that respect at all events, of English physiologists?—No, I think not.

15399. Considering the question of the organisation of an animal in connection with the sensation of pain, do you think that there is any need to include cold blooded animals in the Act as at present?—I think they certainly must suffer very little, but I think again, as a matter of public policy, it is rather difficult to press for their exclusion. I do not think it would be understood. If they were excluded, and people could make experiments on them, I am not at all certain that that would satisfy public opinion. I think it would be better to have a separate certificate for cold-blooded animals, like fish and frogs, which will certainly suffer very little pain rather than to exempt them altogether from the Act. But if by your question you mean whether I think that they suffer much pain, I certainly do not think that they do. The organisation of the part of the brain

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that is connected with pain in man is so small in those animals that it cannot have any very large function.

15400. Do you think that those cold-blooded animals being included in the Act somewhat hampers physiological research in this country?—I think it does, because a good many experiments could be made on frogs, and sometimes on fish, and if they require a certificate it prevents some of them from being undertaken; and, as a side point, I think that rather tends to divert attention to the higher mammals instead of concentrating it on the lower—that there might be fewer experiments on the higher mammals if there were more liberty for experimenting on the cold-blooded animals.

15401. Do you not feel yourself that in the future the study of what I may call comparative physiology is likely to come more and more to the front?—Yes, I think that is going on every day.

15402. And is not the physiology of fishes at present hardly worked at at all?—It has not been much worked at.

15403. Would you therefore recommend this Commission to consider that question of excluding those cold-blooded animals from the Act?—I certainly regard it more as a matter of public policy than of pain inflicted on the animals whethered they are included or not. It is a balance in which the amount of pain inflicted on the animals whether they are included main factors to be considered are the general public opinion upon the matter, and the degree of hindrance to research which is caused by the present Act, and that is a balance which I have not very carefully thought out.

15404. Do you not think that public opinion is mostly confined to dogs and cats, and warm-blooded animals generally?—Yes, largely. I think it is obviously absurd that cold-blooded animals should be classed under the same return of the number of experiments made with cats and dogs.

15405. Even now, at the present moment, I presume that you cannot make experiments upon tadpoles without a licence?—No, you cannot.

15406. (*Mr. Ram.*) In giving demonstrations to your students, do you instruct them as to the method of giving and the duration of anæsthetics?—In the first demonstration generally the method of giving the anæsthetic is described. After that usually the experiment only is mentioned and described.

15407. Do they see the animal put under anæsthetics?—In some cases they see the animal put under anæsthetics in the lecture room. Generally it is a question of time if that is not done, because everyone is very busy, and it takes some time.

15408. And during the demonstration, I take it that it is necessary to keep on the anæsthetic to ensure its being operative, so to speak?—Yes.

15409. And they see that?—Yes.

15410. Do you instruct them as to the necessity of keeping up the anæsthetic, and producing perfect anæsthesia the whole time?—Yes.

15411. On that, may I ask you another question? We have heard the phrase several times, "incomplete anæsthesia." Anæsthesia, according to its proper meaning, means that there is no perception of suffering at all?—No perception of suffering.

15412. Therefore, incomplete anæsthesia is a misnomer?—Yes, it is a contradiction in terms; but the meaning of anæsthesia is very varied.

15413. But anæthesia means no perception of suffering at all—that you have no ἀισθήσις. In nerve practice, anæsthesia means that there is no sense of touch, but that there is pain. Analgesia means no sense of pain. There are at least half a dozen interpretations, including Claude Bernard's.

15414. Anæsthesia means that which deprives you of all sense of pain?—Yes, that is the popular view.

15415. (*Sir William Collins.*) Is that the sense in which you have used the word in your evidence to-day?—I used it in the popular sense of absence of pain.

15416. (*Mr. Ram.*) To that extent you think that the term "incomplete anæsthesia" is a misnomer?—It is a contradiction of terms.

15417. (*Sir William Collins.*) You stated, I think, that when an animal was completely under anæsthesia, if you rapped on the table close by you produced convulsive movements?—When it is under morphia alone.

15418. Only under morphia?—The difference with morphia is that it acts very slowly on the lower nervous centres, and some of them it excites and makes more active. It has a more gradual action throughout on the lower centres from the higher to the lower, which is rapidly gone through under chloroform.

15419. That is not the case under chloroform?—No.

15420. Is that the case with man under morphia?—I have never seen an exactly parallel case of man under morphia, so that I am unable to speak from experience.

15421. (*Sir Mackenzie Chalmers.*) There was a statement made by Miss Hageby about morphia, I think, with regard to some of your experiments—it is at Questions 9266 to 9268. Will you read it, and see whether you have any comment to make upon it?—(*After referring.*) The particular experiments which are mentioned here by Wood and Cerna I have not any acquaintance with; but if the statement is intended to imply that a dog is not affected by a moderate amount of morphia infinitely less than that given on that occasion, one can only meet it with a flat contradiction. There is an experiment here by Claude Bernard, which I know, in which it is said that a dose of 30 grains was given, but Claude Bernard expressly states that 50 millegrammes * is sufficient to send an animal into profound stupor, into a condition in which cutting operations on the skin and nearly the whole of ordinary vivisection could be performed with the animal completely untied, and without any movement. That is given in Claude Bernard's "Lessons on Anæsthetics." He also mentions in his experiments the increase of excitability by a non-painful stimulus, such as by rapping on the table and by sounds. The reason why Claude Bernard gives such contradictory accounts of the action of morphia, apart from the reason that he did not know everything about it, was that he relied upon some false experiments of his own. He concluded that the increased excitability from morphia depended upon the brain, and that you did not get this increase of movement when the spinal cord alone was present. That was shown by Kölliker to be erroneous, and it is the easiest thing in the world to show that it is wrong—that this increased excitability comes from the spinal cord and not from the brain. It can be shown in the frog. A frog can be decapitated and morphia injected under the skin; in a short time the same kind of convulsive start that you have in the dog by a slight tap is produced in the frog, and this is of the same kind as is produced in man. But there are many things in the action of morphia that we do not understand; that is a second reason, apart from his erroneous experiments, why Claude Bernard makes so many contradictory statements. A third reason is that in his experiments he is making observations on each experiment as it occurs. It is not a systematic account of the action of anæsthetics; it is a description of an experiment done before an audience, with the experimenter making his deductions as he sees the things done—not in the quiet of a laboratory. Moreover, the account of the experiment was taken down by assistants; he did not write a full account himself, so that the whole form of the account is one that naturally leads to misconceptions and to contradictions.

15422. At any rate, you have no doubt that a sufficient dose of morphia is an absolute anæsthetic?—It is an analgesic.

15423. Are there cases of idiosyncrasies in animals as there are in man?—There are great differences; that is why, in experimenting with morphia, you have to take each animal and watch it.

15424. Morphia ought only to be given, then, by a very skilled physiologist; it is not so safe as chloroform or A.C.E.?—No, it is not so safe. There is a longer interval between the disappearance of pain and the disappearance of consciousness with morphia than there is in chloroform.

15425. (*Dr. Gaskell.*) Still, the disappearance of pain is easily produced; that is the main point. Pain goes quickly with morphia.

* *This is the dose mentioned for a young dog; more is required for an adult dog.*

Mr. G. Sims Woodhead, M.A., M.D., called in; and Examined.

15426. (*Chairman.*) You are a Master of Arts, a Doctor of Medicine, and Professor of Pathology in the University of Cambridge?—Yes.

15427. And you are one of the gentlemen whom the Vice-Chancellor has nominated to give evidence on behalf of the University?—Professor Langley is one, I am the other.

15428. You hold a licence, I believe?—I do.

15429. How long have you had one?—For about 24 or 25 years.

15430. You have performed during that time operations on animals?—A very large number.

15431. And you have had charge, I believe, of the work, or the principal part of the work, in pathological laboratories in the United Kingdom?—Yes; I put that in simply to indicate that I have had a somewhat wide experience.

15432. So that, at various laboratories, you have had responsibility and experience?—Yes.

15433. And for the last four or five years also you have been a member of the Royal Commission on Tuberculosis?—I have.

15434. During the time that you held a licence, have you been in the habit of performing operations in connection with pathological research?—Yes, largely in connection with pathological research.

15435. And also in teaching?—Yes.

15436. And in connection with the diagnosis of disease and the testing of antitoxins?—Yes.

15437. And what has the bulk of your work been in connection with?—In connection with the diagnosis of diseases, especially in connection with diphtheria and tuberculosis; and also in connection with the preparation of antitoxins. In addition to this, I have done work on the healing of wounds, the organisation of blood clots in vessels, the action of variolus vaccine, and the manufacture and testing of toxins and antitoxins, especially in connection with diphtheria.

15438. In connection with all those matters, have you made experiments on animals?—Yes, a very large number.

15439. And you have probably formed a very definite opinion one way or the other as to the value of experiments on animals in such matters?—A very definite opinion.

15440. What is your opinion?—My opinion is that a great deal of the work that has been done recently could not possibly have been done without having recourse to experiments on animals.

15441. Does that apply to all these different inquiries that you have just been mentioning?—Just so—to each one.

15442. Has your work on the Royal Commission on Tuberculosis confirmed you in that view?—Entirely. It would have been impossible even to undertake the work if we had not been able to use experiments on animals.

15443. Have they carried out, or caused to be carried out for them, experiments on animals for their own satisfaction during the last four or five years?—Yes, on a very extensive scale.

15444. Have they reported yet?—They have made two preliminary reports, and have issued four or five volumes of records of experiments.

15445. They have not made a final report yet?—No.

15446. But they have reported upon their experiments to a large extent?—Yes.

15447. And they have reported the evidence?—They have put forward the evidence, and their conclusions so far as that evidence will allow them to go up to the present time.

15448. I gather, then, that they attach importance, as a body, to experiments on animals?—Very great importance.

15449. Have you examined the question of the antitoxin for diphtheria by experiments?—Yes. In fact, I undertook the study of the question for the Metropolitan Asylums Board when they first commenced the work, and I carried it on for some four or five years. I saw the commencement of that work.

15450. About when was that?—1895 and 1896.

15451. Did you report to them upon it?—Yes.

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15452. At that time were they using the antitoxin, or were they desiring your report before they used it?—They did begin to use it; but they could not get a sufficient supply, and they could not determine the strength of the antitoxin. So they asked us to determine first of all whether it was possible to measure the strength of the antitoxin; and, secondly, they asked us to prepare the antitoxin for them for the treatment of their patients.

15453. And were you to report upon the value of the antitoxin?—We did report upon its value; we were asked to prepare the antitoxin for them, and then I reported first of all that from what I could learn I considered that it was useful; then we were asked to prepare antitoxin for them, and we undertook its preparation.

15454. Was this at Cambridge?—No; this was at the laboratories of the conjoint Board of the Royal College of Physicians and Surgeons in London, where I was.

15455. Were you at that time in charge of the laboratories of the Royal College of Physicians?—Yes.

15456. Then when they asked you to prepare it, did you do so?—I did.

15457. Did you watch the results of the antitoxin of diphtheria?—Very carefully indeed.

15458. Have you done so since the years from 1895?—Yes, I made reports from 1895 to 1898. The work was then taken over by Cartwright-Wood. I also collected the statistics for 1899; but I then left for Cambridge, and have not had an opportunity of analysing all the statistics since that time. But from the year 1894 to 1898 I kept a careful record, which I now have before me.

15459. You did not keep statistics of that kind afterwards; but I suppose that you kept your eye more or less upon the question?—Yes, pretty carefully.

15460. What is the result of your experience, first up to 1898, when you were keeping this record?—I can give the statistics very shortly. In 1894 very few cases were injected, and the percentage case mortality was 29.6—practically 30 per cent.

15461. In what hospitals?—In the whole of the hospitals under the Metropolitan Asylums Board. I have the figures from the separate hospitals, which I can give you immediately. In the first antitoxin year, 1895, the case mortality had fallen to 21.5, in 1896 to 18.3, in 1897 to 17.7, and in 1898 to 13.1.

15462. Do you attribute that to the antitoxin? It is quite possible that there may have been a decrease in the virulence of diphtheria, as there often is in diseases?—Yes, that is possible. But we have two hospitals which rather took away that ground. In the Northern and Gore Hospital, where they had a large number of cases of scarlet fever—that is to say, cases that suffered from scarlet fever, and afterwards suffered from diphtheria, diphtheria bacilli being demonstrated whilst in the hospital—the case mortality amongst those was very high indeed; but when those cases were treated with the antitoxin—exactly the same set of cases—the mortality fell from something over 45.1 to 1.94 per cent.; or, if we include the similar cases at the Gore farm, 1.19.

15463. Was that between the same years, 1894 to 1898?—Yes, within that period; the fall was so tremendous that it could not be accounted for by anything else.

15464. There was a fall of rather more than 50 per cent. in the other cases?—Yes; but here it falls from over 45 to something lower than 2 per cent. In one set of figures it was between 1 and 2 per cent. In this set the cases were treated at once with the antitoxin. If the same set of cases had been allowed to go for some time untreated with the antitoxin, I have not the faintest hesitation in saying that there would have remained a very high mortality.

15465. You are speaking from those statistics. Have you yourself practised in any diphtheria hospital and watched the cases?—No, I have never watched any

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series of cases in a hospital during the antitoxin period; but, unfortunately, I have had to watch some cases of children of friends. When I was hospital surgeon and hospital physician, of course I had from time to time cases under my charge; but that was before the antitoxin days. But, from seeing cases treated with antitoxin, and comparing them with those that I had seen in the pre-antitoxin period, I am perfectly satisfied, quite apart from statistics, that the antitoxin treatment is of great value. Although I have never practised since I held resident appointments in the Royal Edinburgh Infirmary, I have taken this question up, whenever any of the children of my friends have been attacked by diphtheria.

15466. Then you used the antitoxin in those cases?—Yes, and the results, to my mind, were far more striking than any statistics. The patients in most cases were exceedingly ill, suffering from acute toxic symptoms, which are very marked in diptheria, but in a few hours after the administration of a full dose of antitoxin their condition was quite altered. You see a patient almost livid, breathing heavily, with a very high temperature, in great discomfort, and unable to sleep. You give that patient a dose of antitoxin, and in three to four hours he is breathing easily, the skin is acting, and the patient is comfortable, and he goes to sleep. That, to my mind, is even more striking than any statistics that one can bring forward.

15467. Then you are satisfied of the benefits of the antitoxin in the case of diphtheria?—Yes. I may mention, perhaps, in passing, that in our experiments on animals carried out in connection with the testing of the potency of the antitoxin, an enormous dose of toxin may be neutralised by a dose of antitoxin, and you may see that animal recover without turning a hair, if one may put it in that way, never losing its appetite, never losing in weight, but simply going on as would a normal animal into which no toxin has been introduced.

15468. (*Mr. Ram.*) What would happen if you did not give it the antitoxin?—It would die in from 24 to 48 hours.

15469. (*Chairman.*) I gather that you are thoroughly satisfied of the value of the antitoxin in cases of diphtheria?—Absolutely.

15470. Do you think that there is any possibility of improving the antitoxin and so decreasing the mortality?—I think we have not reached finality in it. I believe, for example, that the antitoxin may be prepared in a slightly different way. We have already results from experiments on animals which indicate that. I believe that in future, instead of using the toxin only, we may use the proteids of which the organisms are made up in order to obtain something in addition to the antitoxin, and that instead of it having an antidote merely to the poison of the diphtheria bacillus, we shall be able to obtain an antidote to the specific action of the diphtheria bacillus itself. This action is local, and not so important as the toxic action; but I believe that a combination of the antidotes to two will probably be more valuable than the single antitoxin; everything is tending to prove that. I might say, in connection with the use of antitoxin in the hospitals, in order to show that it produces not merely a diminution in the virulence of cases, that in the same hospitals the percentage mortality varies very greatly in different years. For example, in the Northern Hospital the percentages were 45.1 in the pre-antitoxin year, in the Western Hospital, 37.1, in the Eastern, 30, in the South-Eastern, 29.7, in the South-Western, 28.5, and in the North-Western, 26.9. Then we find that in the last antitoxin year, 1906, in the Northern Hospital it had gone down from 45.1 to 1.94, in the Western Hospital it had gone down from 37 to 24; in the South-Western it had gone down from 28 to 22; in the South-Eastern from 29 to 22; and in the Eastern from 30 to 19. Although there was a difference in those hospitals in the different years, in all of them there was a downward tendency. This cannot be due merely to the general conditions in the hospitals. Moreover, we found that the more antitoxin was given in any hospital, and the earlier it was given, the greater was the diminution in the mortality; everything else being constant, the greatest fall in the death-rate took place where most antitoxin was given in the earliest stages of the disease, because this latter, of course, is a most important factor.

15471. You said that you kept this question of the diphtheria antitoxin as a remedy under view, although you were not engaged in preparing the statistics, but what do you say as to the use of it and the belief in it by those who attend diphtheria cases now?—All one can say is that our medical men are using more and more antitoxin every year. The worse the cases are the more anxious they are to give the antitoxin; I said before the very best results are always obtained when the antitoxin is given at an early stage, before the poison gets a thorough hold of the patient and damages the tissues. If you can give it early the death rate from diphtheria should be practically nil. The only cases that should be allowed to die are those that do not come under treatment until too late, or the few very virulent cases in which the poison seems to be so plentiful and so active that the patient is, as it were, stunned.

15472. That you believe to be the result of the use of the antitoxin?—Yes.

15473. And the antitoxin, I understand you to say, cannot be successfully tested and applied without experiments on animals?—It cannot be made, of course, without bringing some animal, usually the horse, in, and it cannot be tested without the use of guinea-pigs or some similar animal; we always use guinea-pigs. And here, of course, come in those cases of which Professor Langley has been speaking this morning. In a great many of those cases there is absolutely no suffering of any kind beyond the prick of a needle. The antitoxin neutralises the toxin so completely that you cannot tell that anything has been done to the animal; there is no swelling, no loss of appetite, and no loss of weight; the hair does not fall off, and the animal remains in perfectly good condition, and there is absolutely no pain. It is stated, of course, in our Return to the Home Office that so many thousands of experiments have been done in connection with this work, but probably in nine hundred out of every thousand there has not been the slightest discomfort even, except that produced by the prick of a needle.

15474. (*Mr. Ram.*) What is the poison which is given to the animals that are infected?—The diphtheria poison; it is the diphtheria toxin, the antitoxin being used to neutralse it.

15475. Are all animals subject to diphtheria?—Not all, but you can poison nearly most of them with the diphtheria toxin. You can poison the rat, but you must give it an enormously greater dose than is necessary to kill a guinea-pig. Animals differ greatly in this respect, and you have to determine what the requisite dose is in each case.

15476. (*Sir Mackenzie Chalmers.*) In preparing the antitoxin the horse is used?—Yes.

15477. (*Chairman.*) Have you had any experience in watching the carrying of disease by mosquitoes?—I have seen a limited number of experiments with mosquitoes, but I have followed the question very closely in connection with malaria and yellow fever, but we have of course no opportunity of studying it practically in this country.

15478. We have had many witnesses who have been engaged in it at first hand, so that it will not be necessary to ask you about it unless there is anything that you can shortly state as to your view of the value of those experiments?—I think that many of the experiments recorded are valuable as indicating how much we really owe to the method of experimentation on animals. In the case of yellow fever, although observers were not able to trace the yellow fever infective agent from man to animal, or from man to man directly through experiments, they have been able, by applying the methods which have been used in connection with other diseases, to demonstrate that the yellow fever virus is carried by the mosquito; and so satisfied were they of this, that, as we know, a member of the American Commission submitted himself to infection by that special method. Volunteers offered to live in a house with all the dirty clothes from yellow fever patients and to remain there for a considerable length of time; all came out quite unaffected. It has previously been proved that the carriage of certain organisms from the blood of birds is affected by mosquitoes, and it was suggested that this might be the method by which the infective material of yellow fever is carried from a patient suffering from yellow fever to a healthy individual. As the result of submitting himself to such infection by a mosquito—stegomyia fasciata—Dr. Carroll, a member of the Commission, and several other volunteers, contracted yellow fever. Dr. Lazear died of yellow fever.

15479. In both cases did they take it from

mosquitoes?—I am not quite sure that Dr. Lazear contracted the disease as the result of an experiment, but in Dr. Carroll's case it was undoubtedly so, and a number of the volunteer patients were infected in the same way. They simply applied in this case, on themselves, of course, the methods already proved to be of value when animals had been used. In connection with the bird parasites it was proved that they could be carried by mosquitoes, that they passed through certain phases of development in the mosquitoes, and then were transmitted to other birds. It was argued from this that it was possible that the same thing might hold good in malaria and in yellow fever, and it is now proved, I think, as clearly as anything can be proved, that that is the case. That was simply the application of the experimental method to man instead of to animals.

15480. There was no toxin that they had to administer to themselves in that case?—No.

15481. I think that the only other point on which you wish to give evidence as to the carrying of diseases through other animals is that there is still some uncertainty about its effect as it passes through the different animals?—Yes, it has been objected that different animals must be affected in a different fashion by the various diseases, and that undoubtedly is the case. We know, for example, that certain animals are affected by tuberculosis, and that other animals are not affected by it in the same degree. We know, too, that certain pus-producing organisms are very virulent for one species, but that if you pass that pus-producing organism through another species, through a whole series of animals of this second species, it gradually loses its virulence for the first, but gains in virulence for the second. We know that there are very great differences in immunity and susceptibility to various specific infective diseases, but, of course, the organisms producing them can be studied as they pass through the different animals, and I think it is very important that we should know all the phases through which these disease-producing organisms pass outside the human subject. If we cannot trace these organisms, and trace their effects outside the human organism, then we cannot tell from what point we may be attacked by these various disease-producing organisms. That has been our difficulty in connection with the work of the Royal Commission on Tuberculosis. If we can once determine exactly how disease-producing organisms act in different animals, we shall be in a much better position to understand the conditions under which we may eliminate the danger from those diseases.

15482. That, I suppose, would involve a considerable amount of experimentation?—Of course.

15483. Are the Royal Commission on Tuberculosis engaged on such experiments?—Yes, at present.

15484. Who is the Chairman of that Commission?—Sir Michael Foster was the Chairman, but he unfortunately died last year, and now the Chairman is Mr. Henry Power, of the Local Government Board.

15485. (*Sir William Collins.*) Did I rightly understand you to say that we have learnt something with regard to vaccine by experiments on animals?—The only thing, I think, that we have learnt by experiments on animals with regard to vaccine is that in all probability it is an exceedingly minute organism.

15486. Do you think that you have identified the organism?—No, I think it is ultra-microscopic. That is the opinion that I have formed. At one time I performed a large series of experiments on rabbits. Rabbits and monkeys are the only convenient two animals outside the bovine and the human subject on which one is able to raise vesicles.

15487. Was not a prize offered by the Grocers' Company for the identification of the cause of vaccine?—Yes.

15488. Has that prize ever been obtained?—Not as yet.

15489. Is that all that we have learned with regard to vaccine by experiments on animals?—Yes, I think so, except, of course, Jenner's original experiments on cattle, which are so well known to you.

15490. They were rather observations, were they not, than experiments?—That is the case.

15491. Then you dismiss the protozoal bodies which have been alleged to cause vaccine?—I do not say that I dismiss them, but I have seen no fully satisfactory evidence that they cause the vaccinal condition.

15492. At any rate, any micro-organism which may be the cause is at present invisible, and has not been identified?—Yes, I took a large series of vaccines, and I found that I could obtain the best results from those vaccines in which there were no organisms which I could cultivate or see under the microscope, in fact, that the best vaccine was that which contained no organism such as we usually recognise as the cause of one or other of these active processes.

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15493. Not even the small bacillus which has been alleged as the cause?—I could not find that bacillus, and I came to the conclusion that, like the pleuro-pneumonia organism, and perhaps that of yellow fever, it is ultra-microscopic. It will pass through the pores of a very fine filter, but we have no microscopic appliances at present which will enable us to see it.

15494. Is it the same with regard to sheep pox and cattle plague, which were brought before the last Commission by Mr. Simon in 1876?—Did he refer to pleuro-pneumonia of cattle, or foot-and-mouth disease?

15495. The examples on which Mr. Simon thought that he had obtained exact scientific knowledge were sheep-pox and cattle plague?—If he referred to pleuro-pneumonia I do not think we have been able to find any organism yet that we could identify.

15496. If we refer to cattle plague—rinderpest?—There again we are in the same position, I think.

15497. No organism has been identified?—Not that I am aware of.

15498. And with regard to sheep-pox?—I do not know; I have not studied it.

15499. It was claimed, was it not, at one time, that an organism had been discovered, but it was subsequently repudiated?—Yes.

15500. With regard to the Tuberculosis Commission, what was the occasion of the appointment of that Commission; was it not some allegation made by Koch?—Yes; that the tuberculosis of cattle and the tuberculosis of man were different diseases—that they were not transferable from one organism to the other.

15501. Had he based that opinion upon his own investigations?—Yes, we think on an incomplete series of investigations.

15502. But investigations on animals?—Yes.

15503. You think that you have now confuted that opinion?—We have obtained a very large amount of evidence which is in opposition to some of his conclusions.

15504. Also by experiments on animals?—Yes, and we have also come to the conclusion that if we took part of our experiments only we might have come to the same conclusion that Koch came to, but taking the whole of our experiments we are satisfied that the diseases are transmissible; at any rate, that cattle tuberculosis can be transferred to the human subject. We cannot experiment directly in the opposite direction.

15505. Incidentally I understand that you have discovered, or have reason to emphasise, the great importance of soil upon the various seeds in the shape of organisms that you plant in it?—Yes.

15506. Now, as regards the antitoxin, you spoke of diphtheria in animals?—Not of diphtheria; of the toxic condition; it is not quite the same thing. You can produce the toxic symptoms in animals, but it is more difficult to produce the local symptoms.

15507. You do not reproduce the disease in animals that we know in man as diphtheria?—No, because diphtheria is primarily a local disease, and only later becomes a constitutional disease. You have, first of all, the local manifestation, the cause of the inflammation, set up in the windpipe or the upper part of the respiratory tract.

15508. To that extent then one of Koch's postulates of causation fails?—Not necessarily; you do not set up the exact kind of local condition seen in diphtheria of the human subject, but you can set up an inflammation by injecting the poison under the skin of an animal, but it does not fly to the throat; the organism does not move from point to point; it settles down at one point, where it produces its poison, and that poison is carried from that local area to the rest of the body, where it acts upon the various tissue of the body.

15509. Do you produce the false membrane in an animal?—No, because you inject the poison under the skin, and the false membrane is nothing more than the evidence of inflammation on a mucous surface; we have simply fibrin thrown out on to the inflamed surface.

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15510. Can you produce that false membrane in an animal?—You can get the fibrin thrown out from the vessels in very acute cases of inflammation, but as it is not on a free surface it forms no false membrane.

15511. But you do not reproduce the symptoms which are familiar as the diphtheritic symptoms in man?—Only the toxic symptoms. The symptoms are due very largely to the interference with respiration in a child, but most of the toxic symptoms may be produced in an animal, just as they may be produced in a man.

15512. Is the use of the antitoxin based upon the presumption or proof that the Klebs-Loeffler bacillus is the cause of diphtheria?—Yes.

15513. We have been told that in 20 per cent. of the cases of diphtheria that organism is not found?—I think that is quite possible, because in many of those cases you have really the Klebs-Loeffler bacillus crowded out by other organisms, and you get a condition which commences as slight diphtheria, and ends as a septic sore throat. The one organism is practically crowded out by others.

15514. We have also been told that it is often found in healthy throats?—Certainly; I have found it in the throats of healthy nurses who have been attending cases of diphtheria.

15515. And even in cases which have not been in contact with diphtheria?—I have not had the opportunity of examining many such cases.

15516. You have not looked for it there?—No, but I have no doubt that organisms similar to it have been found; these, however, do not give the same toxic reactions. There are certain organisms resembling the diphtheria bacillus, but you can always distinguish between the two by determining whether they produce toxin or not.

15517. You mean the Hoffmann bacillus?—Yes.

15518. Has it not been alleged that in something like 15 per cent. of healthy throats the true Klebs-Loeffler bacillus has been found?—I certainly think it is found far more frequently than is realised; but I also think that it does not attack the throat until there is some slight inflammation, some slight breaking down of the tissues. We have, fortunately, an epithelial surface, which presents a barrier to the invasion of all these organisms, and it is only when there are slight breaches in that barrier that the organism gets in and is able to make further breaches.

15519. Where there is some lesion prior to the entrance of the organism?—Yes. If the organism were not there it would heal, and there would be no further disturbance, but if the organism is there it takes the opportunity of getting in and giving rise to very serious conditions.

15520. You cited both clinical and statistical evidence in favour of the antitoxin. What would be about the total number of the cases of diphtheria that you have yourself watched?—When I was house physician I saw a dozen perhaps.

15521. And since then?—And since then I have seen perhaps a couple of dozen. I have only been called in to special cases.

15522. About 36 cases in all?—Yes.

15523. In regard to the statistical evidence you called attention to the fact that in the pre-antitoxin days, as you call them, there was a variation from 26 per cent., I think I understood you to say, at one hospital, to 45 per cent. at another hospital in the same year of deaths from the disease?—Yes.

15524. You did not tell us whether the total number of cases of diphtheria admitted into the hospitals had increased of late years. Should I be right in thinking that that is so?—I should say that it has in London, but I can also say this—I was looking at the statistics in connection with it a short time ago—that there are now actually fewer cases of diphtheria than there used to be, and that there is not only a much lower death-rate, but a lower actual mortality taking all the large cities of the world.

15525. But taking the London figures that you quoted, should I be wrong in thinking that the numbers were very much larger in the later years than in the earlier years?—No; there are a larger number in the later years during the time that I was examining, because we came to the conclusion that they sent cases in when otherwise they would have kept them out, and also that they sent in the very bad cases in hopes that they might be cured in the hospitals.

15526. Was there not another reason? Was not the area of diagnosis extended by using swab cultures?—Not until they came in, of course. At that time we did not get swabs until the patients had come into the hospital.

15527. Do you say that nowadays cases are not diagnosed by swabs?—No doubt there may be nowadays, but I was referring simply to my own statistics.

15528. I was only asking whether in recent years there has not been an augmentation in the number of cases admitted into the Metropolitan Asylums Board Hospitals, because the area of diagnosis had been enlarged by the bacillary test as against the old clinical test?—I can state that generally, but I do not think that it is carried on on such a scale that it will make a great difference in the number of admissions. In the Cambridge outbreak Dr. Cobbett and Dr. Graham Smith examined bacteriologically practically every case that came in contact with a case of diphtheria; as a result of this method of procedure they were able to stamp out diphtheria in both epidemics. Afterwards Dr. Cobbett did the same thing at Colchester.

15529. (*Mr. Ram.*) Did they use the antitoxin?—Yes, in all cases. They examined every case which had been in contact, and injected the antitoxin wherever they found the bacillus, and so were able to stamp out the epidemic in a very short time in both places.

15530. (*Sir William Collins.*) But in regard to the London cases to which you have called our special attention, should I be wrong in thinking—I want to be quite clear—that the numbers in recent years have been considerably augmented by reason of utilising the diagnosis test of the bacillus?—From the figures to which I have called attention that does not hold. I rely only on those figures which I have had to deal with specially; those are my own work; the others I have not had any opportunity of checking.

15531. Then do you stop short at the year 1898?—Yes.

15532. Then with regard to the years up to 1898, had there been much increase in the proportion of cases treated with the antitoxin between 1895 and 1898?—Yes.

15533. Could you give us the percentage of cases treated with the antitoxin in those years?—I can give you the amount of the antitoxin sent in to the hospitals.

15534. I would rather have the number of cases treated with antitoxin?—I have no doubt that I can give you that also.*

15535. Thank you. What I want to know is whether you attribute the lower case mortality that you mentioned, 13 per cent. as against 21 in the earlier years, to the fact of the larger number of cases treated with antitoxin?—Yes, undoubtedly.

15536. I should have been glad to have the figures to complete your story?—I will let the Commission have them. I have not looked over my report except for a few points, but I may say that in 1895 we only supplied 1,200,000 units of antitoxin to the whole of the hospitals under the Board; in 1896 we sent out 25½ million units, and in 1897 we sent out 60¼ million units. That means, of course, a very largely increased number of patients treated. It ran up from, in 1895, 1,200,000, to, in 1896, 25,600,000, and in 1897 to 60,300,000.

15537. Do you happen to have followed up the case in London; do you know what the percentage case mortality now is in the London fever hospitals?—No, I do not know that it is lower.

15538. Is it much lower than 13 per cent.?—I think it is about 10 per cent.

15539. Am I right in thinking that a still larger percentage of cases is now treated with antitoxin than in 1898?—Yes.

15540. And yet the mortality has been reduced only 3 per cent. further?—It comes to a point, you see, at which it is impossible to reduce it very much. If you could get all your patients in at an early period, if you could treat them within 24 to 48 hours, I believe that you could bring the percentage down to 2 per cent.

* 3042 *cases of diphtheria were admitted in* 1894, *of these* 24 *received antitoxic serum. In* 1895 *of* 3824 *cases admitted* 2403 *were injected, and in* 1896, 3758 *cases were injected out of* 5068 *admitted.*—G.S.W.

15541. Within 48 hours of what?—Of the outbreak of the disease by the appearance of the false membrane.

15542. But you may find the bacillus and there may be no false membrane?—Yes.

15543. Is it not the case now that if the bacillus is found the antitoxin treatment follows?—Yes.

15544. Even without waiting for the false membrane?—Yes, and very wisely so. If you do that, if there should by any slight inflammation, even though the bacillus forms the toxin, and the toxin gets into the tissues and is carried away by the blood and lymph, it is neutralised and the patient is put under the most favourable conditions for recovery at once; But you do not get the patients in as a rule until there is some false membrane. If you could treat them as soon as acute inflammation appears, then I believe you would save almost every case, but if you allow the toxin to act for any length of time on the various organs of the body, you cannot neutralise the harm that has been done, though you may neutralise the poison that has done it, now of course too late.

15545. You think that by using the antitoxin the mortality of diphtheria may be reduced to nil?—Almost, if you could get the cases early enough. Our Northern and Gore Farm Hospital statistics bear this out most fully.

15546. You told us just now about stamping out certain outbreaks. That was using it as prophylactic?—Yes, using it as a prophylactic in cases where there had been contact with diphtheria patients.

15547. Is it your experience that it has been valuable as a prophylactic?—I should say, from our experience of these epidemics, that it is distinctly valuable.

15548. Have you seen a recent paper by Dr. Peters, of Nottingham, rather questioning its value as a prophylactic?—No. But I am afraid that he would have to bring forward very convincing arguments to induce me to alter my opinion under present conditions.

15549. You think that by the prophylactic use of the antitoxin, diphtheria may be abolished in this country?—If you carefully examine all the throats of all "contact" cases I think it would.

15550. The actual preparation of the antitoxin from the horse does not require a vivisection licence?—Not now. We prepared it under a vivisection licence because we did not know the legal position. In fact, it was on our application, I believe, that the matter was placed on its present footing.

15551. The production of vaccine and other commercial bodies does not require a licence under the Act?—I believe not.

15552. (*Mr. Ram.*) The specific organisms of certain diseases have not yet been found?—They have not been demonstrated.

15553. Is it your opinion that in other diseases the specific organism has been discovered?—Yes.

15554. And can be dealt with?—Yes.

15555. And with regard to what you have been telling us as to yellow fever, is it a fact that not only the specific organism can be dealt with as you have indicated, but that it has been made commercially useful as a means of stamping out yellow fever?—Yes, the statistics in Havanna are marvellous. In Havanna, from 1853 to 1900, there were 35,952 deaths. In 1900-01 there were cases varying from 308 in a single month in the seasonal period down to 5; and during the same period there were from 74 deaths in one month down to no deaths. In 1901-2, when these protective measures were put into force, only one year later there were never more than eight cases in any single month, and never more than two deaths. So that from 308 cases, with 74 deaths, there was a fall to 8 cases and 2 deaths. That was entirely the result of putting into force agencies for killing the mosquito. That was the direct outcome of experiments.

15556. That preventive result followed directly from experiments which had been made, without which the origin of the disease in the mosquito would not have been suspected?—That is so, we should not have known anything about the life history and mode of transmission of bird or malarial parasites, and our knowledge of their relation to mosquitoes not being acquired could not have been applied to the infective agents of yellow fever.

15557. One other question with regard to diphtheritic cases. Are you, in your own mind, clear that even discounting other causes which have been suggested as assisting, at any rate, in the diminution of mortality——?—Which I do believe play a part, undoubtedly.

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15558. After discounting those other causes, are you still satisfied that the antitoxin treatment is of the greatest benefit in diphtheritic cases?—I am.

15559. Do you find that that is the opinion of the vast bulk of those practitioners who are brought in daily contact with diphtheria?—Yes.

15560. And not only is it their opinion, but is it put by them into practice?—Yes, as I know from the amount of antitoxin that is being used in the country at the present time.

15561. (*Dr. Gaskell.*) Do you use demonstrations at all with your students in your laboratory?—Not before the junior students.

15562. But for the senior students?—Yes.

15563. Do you consider that that is necessary?—I think so. Of course, I am in a specially good position, because I have to do a number of these experiments on animals, and therefore I do not do any special ones. I get the students down to see the experiments that we are carrying out—the senior students, that is to say; so that although I am not performing any special experiments for demonstration purposes, I take the opportunity of letting them see the experiments that we are doing in connection with antitoxin and in similar work.

15564. And those experiments are experiments under licence?—They are all under licence.

15565. Are you able yourself to sign applications for a licence?—No.

15566. Do you not think that you ought to be?—I think that I ought to be.

15567. Would you suggest, then, that in addition to the names of the professors of physiology, medicine, anatomy, medical jurisprudence, materia medica, and surgery, there should also be included in the Act among those people who must sign licences, the professors of pathology, bacteriology, and hygiene?—Yes, I think so, certainly.

15569. All those three ought to be included?—Certainly.

15569. That part of the Act which should be reformed?—Yes, because we really see more of this work than almost any one, except the physiologists.

15570. Is it necessary, do you think, to use the word "professor," or would "teacher" be better?—"Teacher" would be better. A year ago Dr. Nuttall, for example, would not have been able to sign as he was a reader, but he had all the responsibility of a professor as the head of a departmental laboratory.

15571. Perhaps "teacher" and the head of a laboratory would be better?—Yes.

15572. (*Chairman.*) Is there anything else you wish to bring before us?—Professor Langley has suggested that after an operation has been completed we might be allowed to take an animal from the laboratory to some place outside. At present we have to keep all the animals on the premises. Those premises are not always very commodious, and sometimes are not very well suited for animals which must necessarily be carefully looked after, and should be kept under the most hygienic conditions possible, and it would be a very great advantage if we were allowed to take the animals to some place where they may be under better conditions than in the laboratory.

15573. (*Mr. Ram.*) Leaving the address for the Inspector if he wanted to see them?—Yes, I do not wish in any way to limit inspection. I think it is a very good thing that everyone should know that everything must be carried on carefully and without any abuse.

15574. And you have found no trouble from inspection?—None at all.

15575. It has been no nuisance to you?—No.

15576. Might I ask you on that whether your view is the same as that of Professor Langley. I suggested to him, partly to meet public feeling, that in the few cases where curare was used it might be recommended perhaps that the Inspector ought to be present in order to make sure that the anæsthesia was properly complete, and so forth. Would you see any objection to that?—I think it would be a very difficult matter to have the Inspector present. I think if you cannot

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trust a man to do experiments you must not give him a licence.

15577. Including the use of curare?—Including the use of curare.

15578. You will observe that I limited my question to that?—I should not object to a special certificate so that the Inspector might be more vigilant if it were thought necessary in order to allay public fears.

15579. And he would know of the granting of that certificate?—Yes.

15580. Would he know when it was going to be utilised?—Probably not; you could not let him know beforehand the exact hour for the performance of every experiment, but knowing that such experiments were being carried on, he would probably make it convenient to be present at some of them.

15581. (*Chairman.*) Is that all that you wish to bring before us?—There is one other matter, that is, as to the use of dogs. In this matter I should like to support what Professor Langley has put forward. It has been said that they are not useful for pathological work, but I have found on several occasions that we could not have carried out our experiments to a successful issue without recourse to the use of dogs.

15582. I think that the suggestion was that they were not necessary, not that they were not useful, but whether they were necessary?—I should say that they are necessary, even for the very experiment which Professor Langley spoke of, which we have had to perform in connection with the transmission of the tubercle bacillus from the intestine into the body that is along the lacteals. We have had to operate on the thoracic duct; that experiment has been done by a number of people in order to determine the path that the tubercle bacilli traverse in getting to various parts of the body. It is a much more complicated process than we had any idea of. It has been found necessary to trace the tubercle bacillus in the body, not only in the alimentary canal, but after it gets into the respiratory tract and the lymphatics, and so on to its invasion of the blood vessels. It is a much more complicated process than we imagined when we began our research, and one of the experiments which had to be done was the inspection of the thoracic duct for the passage of the tubercle bacillus, and we could not have done that in the same way on any other animal that I know of.

THIRTY-FIFTH DAY.

Wednesday, 13th November, 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, M.D., LL.D.
Capt. C. BIGHAM, C.M.G. (*Secretary*).

Sir VICTOR HORSLEY, F.R.S., F.R.C.S., called in; and Examined.

Sir V. Horsley, F.R.S., F.R.C.S.

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15583. (*Chairman.*) You are a Fellow of the Royal Society and a Fellow of the Royal College of Surgeons?—Yes.

15584. And I believe you come here to give evidence before us as representing the British Medical Association?—Yes.

15585. Would you just tell us what that body is?—It is a body composed of medical practitioners. It includes about half the British medical profession, and each member subscribes 25s. a year to further the objects of the Association. The objects of the Association very briefly are to further the interests of the medical profession and the scientific knowledge of the medical profession.

15586. And they have acted in various respects in furtherance of those views in various ways?—Yes, they have. They have a Scientific Grants Committee for the express purpose of allotting a certain proportion of the subscriptions of members in the furtherance of scientific work by giving grants of money to young members to pay their expenses for the researches they conduct, and in this manner they have expended upwards of £30,000 since about 1874—since about the beginning of the administration of the Vivisection Act. I need hardly say that medical practitioners have not much money to spend, but they have, I suggest, been led to give this money out of their subscriptions because they see in their daily work the necessity of more scientific knowledge.

15587. You propose to give us your views on what you call the ethical side of vivisection and anti-vivisection?—Very briefly.

15588. Are those the views of the British Medical Association?—Yes. I can put it in almost one sentence, not to waste the time of the Commission. The medical profession consider that every effort ought to be made to increase knowledge for the prevention of suffering and disease, affecting not only man, of course, but also the lower animals; and they consider that anyone who opposes such pursuit of knowledge is acting immorally. To them the moral duty is the pursuit of knowledge, and an immoral act is the obstruction of that pursuit of knowledge. I would compare in this particular the fact that of their own moneys they have subscribed this large sum of £30,000 for the pursuit of knowledge, while of the anti-vivisection party, Mr. Coleridge's Society, to take that alone, have collected from the public £86,000 and have not added one item of knowledge to us for the relief of suffering or the prevention of disease.

15589. I understand that you desire, in reference to this point, to call attention to Mr. Coleridge's evidence?—I do. But before I leave that point may I also draw the attention of the Commissioners to the fact that several witnesses have spoken as though the anti-vivisection party were morally justified in accepting the knowledge gained by vivisection. May I point out to you that it has always been the feeling of the profession that the anti-vivisectionists were not morally justified in accepting knowledge thus gained. The profession would, in fact, regard it thus: that if the anti-vivisection party consider that facts have, as it were, been immorally obtained from Nature, stolen from Nature, the persons who benefit thereby are practically receivers of the stolen facts, and they suggest that the legal position of the moral responsibility of a receiver being almost as bad as that of the thief holds in this particular.

15590. But how would you deal with the question? It is said, and I think generally believed, though it is disputed, that Harvey discovered the circulation of the blood by experiments on animals?—Certainly.

15591. Does that, do you mean, bind everybody in the present day not to be an anti-vivisectionist?—Certainly.

15592. Because it is impossible for him to avoid the conclusions arrived at?—Exactly, and in that connection I have placed upon my *précis* a very brief reference to the Anti-Vivisection Hospital, because the money for that hospital was originally collected many years ago, by Mr. Coleridge's society in part, on the idea that medical practice could be conducted without any reference to vivisection.

15593. Vivisection past or present you mean?—Past or present; whereas we know of course that no one feels the pulse of a patient and derives any information therefrom without being indebted to vivisection. Morally speaking, it seems to me (I am only giving my own personal opinion, of course) that the anti-vivisectionists have no right to derive benefit from experiments upon animals.

15594. Holding the views that they do?—Holding the views that they do. I would add that the Editor

of the "Zoophilist," one of Mr. Coleridge's employees, Dr. Berdoe, stated in the "Times" that he would rather be treated with a knowledge of Hippocrates than the knowledge of the present day. I can only say as a commentary on that that from my own personal experience he does not rely on the knowledge of Hippocrates, but he sends his own family to receive the opinions of men like myself. However, I pass from that, my lord, and pass now to the single example that I have selected to illustrate the action of the British Medical Association in promoting the pursuit of knowledge, and I have taken the instance of the Pasteur Institute as being also a subject of considerable controversy and object of attack by the anti-vivisectionist party. In the evidence of Mr. Stephen Coleridge, on page 144, line 14, Mr. Coleridge used these words, "The first striking achievement of my society, with which I had the honour to be associated, was the prevention of a public meeting held at the Mansion House" (it should obviously be "to be held') "in 1889, the object of which was to start a fund to erect a Pasteur Institute in England. We prevented that public meeting taking place, and the result was that no Pasteur Institute was set up. England to-day is, I believe, one of the few European countries of importance that is spared a Pasteur Institute, and in England there is no rabies." Now the freedom of England from rabies I take to be one of the great achievements of modern science, and we owe it entirely to M. Pasteur. The position is as follows: In the first place Mr. Coleridge informs the Commission that that meeting was prevented by his Society from taking place. It is not so. The meeting took place at the Mansion House, the Lord Mayor in the chair, on the 1st July, 1889. In the second place, the object was not as he states it; it was not called for the purpose of establishing a Pasteur Institute in England, it was called "for the purpose of hearing statements from Sir James Paget and other representatives of scientific and medical opinion with regard to the recent increase of rabies in this country, and as to the efficiency of the treatment discovered by M. Pasteur for the prevention of hydrophobia." At that meeting speeches were made and resolutions were passed in favour of thanking M. Pasteur for the treatment that he had given to English patients, and then another resolution that I had the honour of moving was as follows: "Professor Horsley proposed, and Dr. Farquharson, M.P., seconded, a resolution to the effect that rabies might be stamped out of these islands, and inviting the Government 'to introduce without delay a Bill for the simultaneous muzzling of all dogs throughout the British Islands as provided in the measure drafted by the Society for the Prevention of Hydrophobia, and for the establishment of quarantine for a reasonable period of all dogs imported.'" I had the honour of acting as secretary of a Committee that was appointed by the Government to inquire into M. Pasteur's treatment, and when the Committee was in Paris M. Pasteur said to us, "Why do you come here to study my method of a curative for inoculation? You do not require it in England at all. I have proved that this in an infectious disease; all you have to do is to establish a brief quarantine covering the incubation period, muzzle all your dogs at the present moment, and in a few years you will be free." When the Committee returned and reported to the Houses of Parliament, this point of course was always before us, and I moved that resolution with the sympathy of the Committee appointed by our Government.

15595. You mean that you had no intention in calling the meeting to set up a Pasteur Institute for the treatment of rabies?—No.

15596. And no such proposal was made?—No such proposal was made at all.

15597. Your resolution I suppose was carried?—Yes, they were all carried. Immediately this point of universal muzzling was raised the anti-vivisection party, Mr. Coleridge's Society in particular, entered upon a virulent campaign against us, and held meetings and endeavoured to show that M. Pasteur's discovery of the real infective nature of the disease was wrong; they revived the old ideas that it was lunacy in dogs, that it was not a disease, and they revived also the particularly dangerous idea (from the public point of view) that it could spontaneously generate in the dog; whereas M. Pasteur had proved that the exact opposite was the case. I need not weary the Commission, but I would point out that we obtained partial muzzling in spite of this opposition, and that that did so much good that then Mr. Walter Long applied the muzzling universally and established quarantine.

15598. First they had the power to muzzle by counties?—Yes; and in the County of London, I showed from the records of the Brown Institution, the laboratory of which I then directed, that the effect was wonderful in immediately cutting down our admissions of rabies, and that it also cut down another contagious disease—distemper, which, you will understand, is due to a respiratory infection. The effect of that was that Mr. Walter Long, when he was at the Board of Agriculture, imposed a universal muzzling and brief quarantine, with the result that, fortunately, now the United Kingdom is free from rabies, and that is the only correct statement in that passage I have read. But it is not due to anything really but the scientific knowledge that we derived on the subject of rabies.

15599. You say that there were two resolutions passed at that meeting—one thanking M. Pasteur for his discovery and labours, and the other in favour of muzzling dogs?—The universal muzzling.

15600. I understand that Mr. Coleridge and his Society attacked the first resolution. Did they also oppose the second?—Yes; they attacked both.

15601. They attacked muzzling as being what—unnecessary?—They attacked muzzling vigorously as being cruelty to the dog and being unkind, because the disease, as they said, could arise spontaneously; therefore, what was the good of muzzling dogs? May I add that subscriptions were collected at that Mansion House meeting, and later on this country sent 40,000 francs to M. Pasteur as a token of the work he had done for British subjects.

15602. Do you say that the desirability and the efficacy of muzzling dogs were proved by Pasteur by experiments on animals?—Yes, certainly.

15603. What sort of experiments?—By his inoculation experiments on animals, which for the first time proved the existence of a distinct virus and of its transmissibility to different animals. Up to that time these really absurd ideas prevailed that it could be spontaneously generated, that it was not a contagious disease at all, that it was really lunacy in the dog and insanity. It required the scientific investigation of M. Pasteur to demolish that. Of course anybody who had had personal experience of rabies in dogs recognised that it was a contagious disease, and that M. Pasteur was quite right. But all the action of the authorities previously had been in ignorance of the proved fact. That is the first point that I wanted to draw the attention of the Commission to, and I should add that the British Medical Association sent a special deputation of their Scientific Grants Committee to that meeting to express the feeling of the profession as to the value of M. Pasteur's work.

15604. As it seems rather important on the question that we have to report upon, could you refer us to any action taken by the National Anti-Vivisection Society against muzzling?—Yes, I could find it, of course.*

* Sir Victor Horsley subsequently forwarded the following note:—

"*The responsibility of originating and fomenting an agitation against the use of the muzzle to stamp out hydrophobia rests wholly on the anti-vivisection party as shown by the following facts:—*

"*After the issue in 1887 of the Report of the Committee appointed by the Local Government Board to enquire into M. Pasteur's treatment of hydrophobia, and in which the general application of the muzzle was strongly advocated, Miss Francis Power Cobbe, the leader and organiser of Mr. Coleridge's Anti-Vivisection Society, began an anti-muzzling agitation.*

"*One of her pamphlets entitled "The Amendment of the Dog Laws" was published by a small body of antivi-visectionists organised by one of the Executive Committee of Mr. Coleridge's Society, namely, Mr. Pirkis, and though this body was never recognised by the dog breeders and owners of the country and was but an offshoot of Mr. Coleridge's Society, it styled itself "The Dog Owners' Protection Association." Lord Mount Temple introduced into the House of Lords a Bill on their behalf in opposition to a Muzzling Bill introduced by the Society for the Prevention of Hydrophobia. The agitation reached a culminating point on November 25th, 1889, when a public meeting was called by leading members of Mr. Coleridge's Society, Surgeon-General Gordon and others, to protest against the muzzling regulations, at the time when the London County Council was pressing the Privy Council to apply them generally.*

"*At the meeting, however, a resolution was carried by a large majority in favour of muzzling. After this the agitation gradually died out, although an attempt was made to revive it when Mr. Long subsequently issued his Order.*"—

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15605. Thank you, if you would?—The action of the anti-vivisection party in that respect we consider to have been immoral. We consider that it was opposing the results gained by the scientific research, that those results were of value to the human community, and also to the lower animals; and that that opposition was immoral. I have also brought the incident before you because there are a great many passages in this evidence by Mr. Coleridge to which I should like to take similar exception; but this was such a striking contradiction of the actual fact that that is why I have taken it as an example of the rest. I propose now to pass to the next part of my *précis*.

15606. If you please?—The second part of my *précis* deals with the question of teaching students by means of experiments on animals. I desire to lay before the Commission the view that such teaching is desirable in the following subjects:—Physiology, pathology, pharmacology, and surgery; because, with the exception of the third I have had experience in teaching each of these subjects. In the first place I would suggest that Sub-section (5) of Section 3 of the Act should be repealed. It is in effect abrogated in the Act by the second part of Section 3, because experiments may be shown to students in illustration of lectures under a special certificate, but as I hope to conclude by persuading the Commission that the certificate system has so many faults that we should get rid of it, may I suggest also that the recommendation should be that this Sub-section (5) of Section 3 should be repealed. That is the first point. The argument in favour of such repealing of Sub-section (5) would be, of course, an argument against the so-called disadvantages of showing experiments to students, and therefore very briefly I would allude to the evidence which has been given by Sir Thornley Stoker, at Questions 761 and 1051, because it is quite evident that that evidence was offered by Sir Thornley Stoker in complete ignorance of the subject. He says at Question 761, page 32, two-thirds of the way down the columns, after reference to the Act: "I am strongly of opinion that . . . experiments on living animals should be forbidden in illustration of lectures, on the ground of their uselessness, and, perhaps, cruelty also." I think that Sir Thornley Stoker's further evidence, in which he informed the Commission that he was cruel in early life shows that his education as a student had been neglected by his not witnessing experiments on animals, because I think it would have assisted him in becoming more humane, and, at any rate, would have certainly prevented him from giving expression to such an opinion as this. He then goes on to say: "Such demonstrations cannot but be demoralising to the young men and women who witness their performance. They seem to be an offence against humanity." Then also Mr. Coleridge referred at length to this in Questions 11372 and 11396. All I can say is that I have shown a very large number of experiments to students, that they have always evinced the utmost interest in them, and they have told me that the facts which the experiment illustrated have been imprinted upon their memory in a way that they could not have obtained by any other means; in fact, they have simply emphasised the statement of Professor Huxley before the 1876 Royal Commission, where he said: "There is no teaching of physical science worth anything as thorough teaching unless it is accompanied by practical instruction." And I unhesitatingly affirm that there is no demoralisation whatever such as has been suggested to this Commission by Sir Thornley Stoker and by Mr. Coleridge. May I also, in speaking of the previous Royal Commission, draw your attention to the fact that in the report of that Commission, on page 17, the Commissioners say: "The evidence we have quoted above seems to show conclusively that at the medical schools where such demonstrations are exhibited under anæsthetics the sense of humanity in the students is not in fact impaired." I have nothing to add to that statement; it is as true now as it was 30 years ago. I suppose, however, that the Commission would expect me to justify my suggestion in a little detail. I therefore would point out that for instance in physiology, I think it is most essential that students should see, and in fact the senior students perform, an experiment on a warm-blooded animal—a mammalian animal—on the heart and the circulation. I choose that as an example. I am sure that no one can acquire a proper knowledge of the action of the heart or of the real phenomena of the pressure of the blood within the vessels unless he has witnessed, and indeed taken part in such an experiment.

15607. Have demonstrations of that description to show the circulation of the blood in living animals been common in past times or are they only of the present time?—I entered as medical student at University College in 1876 when this Act began its operations, and Professor Burdon Sanderson in that year showed us these experiments.

15608. Under the Act?—Yes, he gave us about a dozen, I think, demonstrations on animals during the session.

15609. Supposing that you took 20 or 30 years earlier than that; were demonstrations used then?—I do not know. I should hardly think they were.

15610. Dr. Burdon Sanderson is dead now?—Yes, he is dead.

15611. He was a very strong advocate of demonstrations, was he not?—Yes, he instituted the practical class in microscopic anatomy with Professor Michael Foster, who is now dead; but I think Professor Sharpey also gave demonstrations at University College before Professor Burdon Sanderson.

15612. I am only asking the question with reference to what you would say as to the comparative knowledge of medical men of the present day and of medical men, say, 50 years ago on such a matter as the circulation?—Of course, at the present time we have greatly increased knowledge. Take this question of the blood pressure, for instance the actual pressure of the blood; we now measure it accurately in the human being, and it is one of the essential phenomena which indicate to us the condition of shock after a surgical operation. The question of the pressure of the blood is going to be unquestionably one of the cardinal points in this great question of surgical shock.

15613. You mean that a much more detailed and accurate knowledge of the circulation has been obtained?—Yes.

15614. And therefore is a necessary equipment of the modern doctor?—Absolutely. When I was a student the blood pressure was looked upon as a scientific amusement, as it were—I mean by many practising physicians and surgeons. They took no heed of it; they never attempted to measure it, and I may say they did not attend the demonstrations of Sir John Burdon Sanderson. The next subject is pathology. I wish to point out that having lectured on pathology, and having illustrated my lectures by experiments on animals, the necessity of this method of teaching students has been borne in upon me very closely. As an example I have chosen epilepsy with convulsions of all kinds as being a very common form of disease, and yet one which the student has no means of analysing in ordinary hospital work, and very often hardly sees a patient in a fit at all. And yet epilepsy, for instance, can be reproduced experimentally with absolute fidelity by the simple injection into the veins of a drop of essence of absinthe. In 25 seconds you have a typical epileptic fit produced, and a student who has once seen it, and watched it develop through the body of the animal never could forget, and never has forgotten it.

15615. (*Sir Mackenzie Chalmers.*) Is that the ordinary absinthe of commerce?—It is the pure essence which is diluted for the drink of vermouth. It is extremely toxic.

15616. (*Mr. Ram.*) Would the animal be under anæsthetics?—Entirely.

15617. The anæsthetics would not interfere with the development of the epileptic symptoms?—No.

15618. (*Chairman.*) Nor would the animal be allowed to recover?—No.

15619. (*Dr. Wilson.*) Do you distinguish in that case between epilepsy and convulsions?—No, I am using both the words because I believe myself that practically in all convulsions the cortex of the brain is involved.

15620. But it is the disease in one case and it is the absinthe in the other that is acting. Supposing that the animal were to recover you would not make that animal an epileptic animal?—No, not permanently epileptic, the fit is produced at the time toxically. In the same connection the electrical excitation of the cortex of the brain for the localisation of the different centres is also an absolutely essential part, or should be, of a student's physiological education. Not only do you teach him the geography of the brain, but you again show him another kind of epilepsy, that which is called Jacksonian epilepsy in the most perfect degree; in fact there is no other way of showing it.

15621. (*Chairman.*) Are there any of these demonstrations which it is desirable in your view to use

before students, in which it is also necessary in order to complete the demonstrations, that the animal should be allowed to survive?—Only under Certificate B.

15622. Are there any which you use by way of demonstration to students?—No.

15623. There are no experiments used by you for demonstrations to students, in which the animal is not put to death before it comes to?—No, unless indeed, for instance, you want to show the students some points in regard to paralysis. Supposing, for instance, you had an experiment in which you divided one half of the spinal cord antiseptically and then dressed the wound, and allowed the animal to recover from the anæsthetic and it healed up, and you showed that animal to the class, say in three week's time, I could conceive that that is a demonstration which would be considered useful to students certainly as demonstrating the symptoms produced by paralysis of one-half the spinal cord.

15624. When it had been shown a second time to the students the animal would be put to death?—Yes, afterwards.

15625. Do you know of that demonstration having been made?—No.

15626. So far as you know, they have all been cases in which the animal has died under anæsthesia?—Yes, every one.

15627. (*Sir Mackenzie Chalmers.*) They must be, under the statute at present?—Yes.

15628. (*Chairman.*) You have already told me that you have never heard of any demonstrations except where the animal had died under the anæsthetic?—That is so, but I am proposing that the whole of that should be repeated.

15629. I am talking rather of the present practice?—Quite so.

15630. Then pharmacology is the next subject?—Yes. Pharmacology is the science of the action of drugs—at the present moment the student is taught *materia medica*, to recognise drugs, but he is only told their action, and if students in Pharmacology were made to perform practical experiments, for instance on an isolated frog's heart, they would receive valuable information. I give one example. I take the two alkaline metals, potassium and sodium; they are almost exactly alike, they are spoken of as simply alkalis, and yet their physiological action on the heart is absolutely different. If a student perform the simple experiment of irrigating a frog's heart with a solution of one and then a solution of the other, he would see that their drug action was totally different. And the same could be said of a great many drugs, and especially of drugs that act on the heart. I think that a student ought to see all those in action on an isolated frog's heart or even on a mammalian heart before he goes into the hospital to give them to human beings.

15631. Would the action be different to such an extent as to make it dangerous to use the one as a remedy precisely as they use the other?—The actual dose depends of course upon many factors.

15632. It would depend on the dose?—Yes, entirely. Then the next subject in which I suggest that students should be taught from experiments upon animals is the one in which I am personally most interested, namely, surgery. The Commission are well aware that at the present moment the only practical teaching in surgery that a student receives, except by what amounts to his experiments on human beings is from operations on a dead body, and those from the anatomical point of view are lamentably few in number. I am referring to the well-known fact also of course that the supply of dead bodies is too scanty in London. But that operating upon a dead body is practically only the revision for the student of his anatomical knowledge. It does not teach him the science of surgery, that is the operating on living tissues, which is a totally different thing. I say the texture and the method of dealing with the live tissue is quite different from that in dealing with the dead tissue; and from the ethical point of view it seems to me that it is not moral for students to gain their knowledge on man when they can perfectly well gain it on a lower animal on an anæsthetised lower animal. In that sense I would bring the use of animals for education in surgery on to exactly the same level as the use of animals for food. What is justifiable for the one is justifiable for the other. Then as regards Section 3 again of the Act, Sub-section (6) relating to manual dexterity I suggest ought to be repealed. It says "experiments shall not be performed for the purpose of obtaining manual skill," that is precisely the reason I think why they should be performed by a student; he ought to obtain his manual skill by operating on the lower animal and not on man.

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15633. To what class of student would you restrict it if you restrict it at all?—This all comes to students in their last two years of the curriculum.

15634. The whole curriculum lasting how long?—The whole curriculum lasting nominally five years; it usually lasts six years. Before a student for instance received a hospital appointment he certainly should do such a course.

15635. And what age would a student be in his last two years; is there any minimum age?—Yes, there is a minimum age, but on the average he would be about 22, from 21 to 23 years of age.

15636. (*Sir Mackenzie Chalmers.*) Before he gets a hospital appointment?—Yes. That means before he is qualified you see, because practically now we do not admit any students to hospital appointments until they are qualified. It is part of their surgical education. It is always very difficult to understand why some of these sections and sub-sections were introduced into this Act, even when you read the Report of the Commission of 1876; and that one in particular I feel sure must have got in somehow under the impression that students would practice on animals without anæsthetics or something of that sort.

15637. (*Chairman.*) Did not the Royal Commission report in favour of that?—I forget. I do not remember the point. But it is in the Act, and I suggest that it should be repealed. Moreover, I am not speaking now without the fact of this method of teaching having been already in operation; it is in operation at the John Hopkins Hospital, Baltimore, perhaps the most advanced school of medicine.

15638. You mean the method that you are advocating?—Yes; it is the most advanced school of medicine and teaching students operations upon animals which they compel them to execute in precisely the same way as they will subsequently operate on a human being. Professor Harvey Cushing, at the John Hopkins Hospital, has now a building for this practical work; perhaps I may show you a photograph of the students equipped as they are (as we all are now for modern operations) and performing the operations on animals (*handing in the same*). The usefulness of this is greatly enhanced by the fact that Dr. Cushing has copied the constitution of our Brown Institution here in London, that he has also instituted hospital wards as part of the building for animals, rooms for the care of the animals, and in that way the poorer classes in Baltimore bring diseased animals to the laboratory to be thus operated upon by the students and cared for in the animals hospital.

15639. (*Mr. Ram.*) In the interests of the animals?—In the interests of the animals.

15640. For curative purposes?—Yes; of course, very often the work is also combined with the requirements of the physiological laboratory. If, for instance, a physiologist wishes to investigate a stomach which has a fistulous opening in it, then it is made under these circumstances in this laboratory, and the animal is transferred to the Physiological Department when the fistula is healed.

15641. (*Chairman.*) In this laboratory of Dr. Cushing's is there a professor or doctor or anyone in charge of the students present in the room?—Yes, a demonstrator.

15642. Some responsible person?—Certainly.

15643. (*Sir Mackenzie Chalmers.*) Who is responsible for seeing that the anæsthetics are properly administered?—Yes, I am coming to the question of anæsthesia directly.

15644. (*Chairman.*) I merely meant in a case like this, where the students work and are allowed to learn their profession upon animals by experiment, do you say that there it is conducted under some responsible head?—Yes, it is conducted most strictly, like any other practical class, and the demonstrator sees to it. The different students are told off to act in different ways; one to act as anæsthetist, another to take the notes, another to do the operation, two or three more to act as dressers, and so forth. They are all told off, and they keep records of all their work. Then, in association with that point, is the

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very important question which has just been raised of anæsthesia. I wish to draw the attention of the Commission to the fact that the risk of death from anæsthesia has always been justly looked upon as a great reproach, and I wish to express my personal opinion that it is purely a matter of knowledge of the dose required, and that as regards the education of students in anæsthetising patients no one ought to be allowed to render a human being unconscious before he has had practice on animals.

15645. What is it that he has to study in it particularly?—The signs of danger, the signs of an overdose with an anæsthetic drug. He can observe all these points on animals exactly as in man. I may say that as regards the investigation of this subject (the actual dose) that is now under the consideration of the British Medical Association, which I represent, and that they have already spent on this subject over £500 to try and determine the exact dose which shall render a person unconscious to pain and at the same time shall not put his life in any danger.

15646. Is there any ascertained scale of the difference between the doses. If you have ascertained it for a man is there any scale that is ascertained to be the proper dose for a rabbit?—It is apparently the same. The percentage of chloroform in the atmosphere breathed that leads the heart to fail or the respirator centre to fail is apparently very much the same in whatever animal. I am speaking now, of course, of warm-blooded animals like man.

15647. Is that a recent discovery?—Yes.

15648. (*Sir Mackenzie Chalmers.*) It is not the same amount but the same proportion you mean?—Yes, the same proportion, not the same amount. The difference of weight, of course, comes in there. You have to poison so much per lb. weight.

15649. (*Chairman.*) That is what I understood from other witnesses. I did not quite follow your answer?—I wish to say that in reading this evidence which has been offered to the Commission I am struck with the extraordinary ignorance that prevails even now on the question of anæsthetising animals. Over and over again witnesses have spoken as though anæsthetising an animal was a totally different thing from anæsthetising a human being. It is nothing of the kind. We have even had revived before this Commission the absurd statement made thirty years ago that you could not keep a dog under chloroform for I forget what the limit was—two hours, or something of that sort. You can keep it under chloroform for a week if you only take the trouble. I have kept an ourang under ether for eight hours, and I would keep a dog or any other animal under an anæsthetic as long as you wish.

15650. By under an anæsthetic, you mean in a condition in which it does not suffer pain?—Absolute unconsciousness to pain. And that, from one's personal experience in the human subject, of course, applies to chloroform as well as to ether, and it applies to morphia—those three narcotics.

15651. You think that that ought to be practised upon animals before anyone practises it on human beings, and that the student's experience gained upon animals is an education, and a complete education, for applying it to man?—I do.

15652. (*Dr. Wilson.*) But the knowledge gained about anæsthesia is never certain—is never complete—because you admit that there are experiments going on now?—But the experiments which are going on now are refinements in order to water down, as it were, the limit. We want to extend the margin of safety without encroaching upon the consciousness to pain. I look, therefore, upon the importance of teaching anæsthesia by experiments upon animals as quite equal to the importance of teaching manipulative dexterity and the recognition and handling of tissues in ordinary surgical operations. Before leaving that point, may I also draw attention to the fact that the anæsthetic substances used in surgery fall into two classes—the respirable, and those which are injected as liquids, such as morphia, for instance, in solution. The action of the respirable gases on the body, and the use of vapours in anæsthesia really date back as long ago as Priestly's experiments on mice with oxygen.

15653. (*Chairman.*) You mean the Dr. Priestly of 100 years ago?—Yes. The idea that invisible gases affected the body was established in that cost by Priestly on mice, and then followed the experiments by Davy on nitrous oxide; those were experiments on men. But again that was a respirable gas. Then we come down to ether and chloroform; but, as regards ether, the real discoverer of the anæsthetic action of ether, Morton, the American, made experiments on dogs. He very quickly, and, I should have said, if I might venture to say so, on slender evidence applied it to man, but he made experiments on dogs, and so did Flourens also. I would accordingly propose that all such experiments with new drugs should always be made on dogs rather than upon man. The next point in my *précis* is the question of new operations in surgery. I shall show directly that many of the operations which are performed now are based entirely on experiments on animals; but I would like to point out that from the ethical and moral point of view, it seems to me that this is an absolutely essential procedure, which ought to be adopted before any new operation is carried out for the reason that if a new operation upon an organ or tissue of the body is performed, no one can foretell what will be the immediate consequences to the animal as a whole. This not only applies to the operation, but it also applies to the actual details of execution. For instance, in operations upon the alimentary canal, the mere handling of the organ to be operated upon, the mere handling of the bowel, for instance, the method of inserting stitches, and so forth—all that comes in as essentials in execution of the operation, and that having to be learnt *de novo*, I suggest ought to be done upon an animal and not upon man. There can be very little doubt that some of the anti-vivisection agitation relating to so-called experiments in hospitals actually bears upon this very point. The anti-vivisection party say that it is immoral to try a new operation, or a new drug, upon a human being in a hospital. They call it an experiment. But, then, they also say it is immoral to do it upon an animal. The only conclusion, of course, is that there should be no more new operations and no more new drugs used in the endeavour to relieve suffering or avert disease. It seems to me an absolute *impasse*, and that if attempts have been made upon patients in hospitals, it has been because a physician or surgeon thought he had a new idea, and wished to put it in practice for the relief of humanity.

15654. I do not think that we have had any witness, so far as I remember, who has come before us from either side who has said that no new drug ought to be employed in the case of a patient.

15655. (*Sir William Collins.*) The distinction was between voluntary and involuntary. I do not know whether you deal with that?—Yes, of course, I do recognise that.

15656. You did not mention it in your *impasse*?—No.

15657. That would be the retort that would be made, I suppose? Probably you will deal with it?—Yes, of course, a very great many investigators have swallowed a great many drugs—Simpson, in inhaling chloroform, for instance.

15658. (*Chairman.*) You were meaning whether the patient's consent was obtained?—Yes, I quite see the point.

15659. That would be the case in an operation; in the case of a drug he could not form any opinion. He might form an opinion whether he would rather die or suffer the risk of pain?—Yes, of course. I have seen a physician propose to a patient, and say, "Here is a remedy that has been suggested; I do not know what its effect is. Will you try it?"

15660. But in the case of an absolutely ignorant person who knew nothing whatever about it?—He probably would not consult him.

15661. He would leave himself in your hands, or you would assume that he had left himself in your hands if you were really doing it not for experiment but with a view of curing him?—Yes, quite so. I gathered that one witness at Question 6077 was rather brought to that conclusion. He said that all experiments were immoral. It was Mr. Graham. He was brought down finally to the point that if there was any serious pain afterwards the remedy might be tried on an animal. It seems to me that is begging the question. The thing is either moral or immoral, and I venture to suggest that any new operation, any new operative method or procedure ought to be tried on an animal before it is tried on man.

15662. That question was with reference to an operation on an animal?—Yes. Then we come to Part III. of my *précis*.

15663. With reference to this I see the first head you wish to speak of is about antiseptic surgery?—Yes.

15664. I do not know whether you have read the evidence that Dr. Bantock gave the other day?—No, I have not seen it. I should like to read it very much.

15665. I am a little afraid of speaking of these scientific subjects which I do not understand, and of misrepresenting anybody, but he generally seemed to think that surgery as practised now aseptically was nothing but a return to the practice of any really careful surgeon before Lord Lister's views about antiseptic surgery were promulgated. What would you say with regard to that?—The answer to that, of course, is extremely simple; that in the practice of the so-called careful surgeon of 40 or 50 years ago, there occurred numerous deaths from pyæmia, septicæmia, erysipelas, hospital gangrene, and so forth, and those do not occur now. Consequently the practice of that careful surgeon was not modern aseptic surgery at all, and for a very good reason—he did not know his danger, he did not know that it was not merely sufficient for him to wash his hands; he did not know that he had to exclude every avenue of entry of microbes into the wound. It is wholly incorrect to make a statement of that sort.

15666. I think Dr. Bantock's view was that microbes did not cause the disease?—Oh, very well.

15667. That microbes were possibly existing in the body, and became active by reason of the disease, but were not the cause of the disease?—That is auto-infection. Of course, we know that is not true. The vast majority of infection in surgical cases comes from outside, that is introduced into the wound.

15668. We have had a large body of skilled evidence against Dr. Bantock's views, but I was not going into the whole of his views—merely that he seemed to think that Lord Lister had discovered and recommended the antiseptic treatment, and that it had proved a complete failure, and that then they had had resort to aseptic surgery, and he said that aseptic surgery in his view was nothing more than what careful surgeons did before Lord Lister touched the matter?—I am sorry to say that, in my opinion, that is absolutely a perversion of the actual position. As I say, I began my surgical work in 1876, just after the introduction of the antiseptic system; consequently I have seen the whole thing develop, and I affirm that our present practice is nothing but the logical outcome of Lister's teaching 35 years ago. That is to say, Lord Lister taught that you had to exclude the microbes from the wound; apparently they were in the air, and were ubiquitous; you had therefore to employ antiseptic substances to kill them, as they fall into the wound, and that was the reason why he employed the spray for one thing and antiseptic lotions for another—that you could not trust (because the whole thing is a question of trust now that the whole of modern surgery is a constant anxiety to a surgeon), the condition of your instruments, and your dressings also, unless they were saturated with antiseptics. But Lord Lister himself said over and over again that inasmuch as these precautions were exceedingly irksome to the surgeon, he would be only too delighted to get rid of them, and no one was more delighted than he when it was shown that the risk of infection from the air was so slight that certainly the use of lotions was enough to avert it, and, therefore, away went the spray. But that was always Lister's own position, and always has been.

15669. I gathered from other medical witnesses that that was what you would probably say. I do not think that Dr. Bantock's views were largely supported by any evidence we have had before us, but still, as he is an eminent surgeon, I thought I should like to ask you?—May I add on this very matter of technique in experimenting the investigations which have been made on animals with regard to infection from the peritoneal cavity, because most of the controversy has arisen over the surgery of the peritoneum. Our ideas now about the physiological action of the peritoneum and its resistance to microbes and infection have entirely altered in the last ten years, and consequently our technique has been altered with regard to the use of antiseptics with regard to the peritoneum. I do not think I need labour the point. Of course I have seen, as I said before, the absolute disappearance from my hospital, University College, of certain diseases—they have gone. Anybody who would be asked now to write an article on pyemia or blood-poisoning in a dictionary of surgery, could not do it; the diseases are gone; and that is wholly due to the introduction and the proper execution of antiseptic surgery. Then, in regard to the saving of pain, the invention of antiseptic surgery has saved a degree of pain which is absolutely incalculable. I believe that it has saved more pain than anæsthetics. I suppose that most of us have been operated upon at one time or another, and one knows that the pain of a wound only lasts for about forty hours, and with a wound that has been well made it is a perfectly tolerable pain. After that, in the healing, there is no pain. But when I was a student a simple case like the removal of a breast for cancer used to have painful dressings for six or eight weeks, and amputations still more. If an amputation now is not healed in a week you blame yourself or the house surgeon. Fortunately we very rarely do amputations now owing to Lord Lister again—very rarely indeed; but in the old days amputations used to go on for a couple of months of very painful dressing.

15670. What is it that renders the dressing unnecessary?—The immediate healing; the complete exclusion of microbes from the wound, and therefore immediate healing of the tissues.

15671. The immediate healthy healing?—The immediate healthy healing, so that the saving of pain by the introduction of the antiseptic system, for which, of course, we also have to thank M. Pasteur, as well as Lord Lister, is enormous. Then, as a third point, the antiseptic system has led to the discovery of new operative treatment; it has led to the discovery of absolutely new operations, by which condition are now perfectly easily and safely treated by surgery which before, when I was a student, were looked upon as hopeless.

15672. You connect antiseptic surgery with experiments on animals?—Entirely, from beginning to end. Because, you see, until you could show that a microbe had a toxic or destructive effect on animal tissues there was no basis for antiseptic surgery. It was Pasteur's work by experiments on animals which proved that point. It was the crucial proof that was requisite. And the Anti-Vivisection Society, in their opposition to antiseptic surgery and their repeated quotation of Lawson Tait, for instance, show, I think, a distinct lack of moral feeling. Anybody who could oppose the means of relieving humanity and relieving the lower animals by a means so simple in execution as antiseptic surgery, I think, cannot be actuated by proper moral feeling. I have seen over and over again Mr. Lawson Tait's name mentioned in this connection. I am not here in any way to answer for Mr. Lawson Tait, who is dead now, of course, but I would like to point out his action in regard to the question of the institution of the Pasteur Institute in his country, Mr. Coleridge informed the Commission that no Pasteur Institute was established. If he means that no institute like the Pasteur Institute in Paris was established, then that statement is contrary to the fact, because the British Institute of Preventive Medicine, now called the Lister Institute, is a Pasteur Institute of the same class, established on the Embankment.

15673. When was it founded?—The committee for its founding, I think, was instituted in 1891. At any rate, a meeting was held in 1893 in favour of it at Birmingham, and as I was very keenly interested in the subject from a public point of view, I went to that meeting. Mr. Lawson Tait attended that meeting, and the first resolution was: "That the members of the medical profession in Birmingham and district cordially approve of the objects of the British Institute of Preventive Medicine." "Mr. Lawson Tait said that he fully assented to the resolution, feeling that, while he objected to a certain class of surgical investigations, bacteriological experiments on animals had proved of great value."

15674. That shows that he did not agree with Dr. Bantock's views about microbes?—Evidently not, and yet he is quoted very widely.

15675. (*Sir Mackenzie Chalmers.*) By Dr. Bantock?—Yes, and by Surgeon-General Thornton.

15676. (*Dr. Wilson.*) That is about the surgical point of view of the question, I think—Lawson Tait's surgical point of view, not the bacteriological point of view?—But surgery is applied bacteriology, is it not?

15677. Not altogether?—I should say it was.

15678. (*Mr. Ram.*) You will find at the bottom of page 625, Dr. Bantock was asked, "Was Mr. Lawson Tait practically on the same lines with yourself?—(A.) Almost absolutely"?—That lies between Dr. Bantock and Mr. Lawson Tait.

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15679. (*Chairman.*) And the views of Lord Lister on both the subjects of bacteriology and surgery have been generally adopted, not only in England, but over the whole Continent?—Yes, over the whole civilised world—everywhere.

15680. You now come to the question of the surgery of the nervous system?—That, of course, again is a very typical instance of the influence of experiments on animals. There is no special surgery of the nervous system, it is simply the application of Lord Lister's principles to the treatment of diseases of the nervous system, and consequently the basis of it is a question of diagnosis. I wish to put to the Commission that there is not a single function of the nervous system, the principle of which we know now, which is not derived from experiments on animals; all our fundamental knowledge of the different parts of the nervous system is derived from experiments on animals. I would like also to point out, to begin with (because it is a point that will have to come up several times), that frequently in the history of medicine the greatest mistakes have been made because people have endeavoured to reason from structure to function, and have endeavoured, by mere anatomical investigation, to guess at function. That has led to the worst mistakes. With regard to the nervous system, Aristotle, you remember, said that the function of the brain was to temper and soothe the heart, it was to cool the heart; and it was nothing but the experiments of Galen, simply opening the rigid box of the skull and showing that the brain was as hot as the heart, that got rid of that idea. That mistake, of course, has been repeated again and again. And then again, if you take the experiments of Bell on the nerve roots, the nerve roots, of course, were known to Galen, but their function was not dissociated until Bell did it, and then only in the case of one root. But his language is very striking. What he did was to expose the roots in a rabbit, and as he had no electrical means of stimulation, he touched the two roots, first one with a point of a knife, and then the other, and he found that if he touched one there was no movement as the result—but if he touched the other, immediately there was a movement of the muscles; in other words, that the two things had different functions.

15681. This was, of course, without anæsthetics?—Yes. He says: "I now saw the meaning of the double connection of the nerves with the spinal marrow." Of course he did. The thing had been known to anatomy certainly (Galen lived in the second century, A.D.), for 1,600 years, but it was not until an actual physiological experiment was done that the investigator now saw the difference between the two things. I always think that is a most striking demonstration of the absolute value of experiment. And as regards Bell's position, which has been referred to again and again in the evidence, the facts are extremely simple. Bell did a great many experiments. If you look at his original pamphlet of 1811 (which is in the library of the Royal Society) it is very interesting; it is covered with manuscript notes of his experiments on the brain, the respiratory centres, and a great many other experiments.

15682. On animals?—Yes, he notes over 100 that he did, and as long as he was investigating the physiological side of nature he made discoveries. He gave it up, and he then later become involved in a certain amount of controversy, because Magendie and the French physiologists by their experiments discovered the function of the other root, the sensory root. They proved that it was sensory by stimulating its central end, which is again the only way of discovering its function. And then Bell, unfortunately, entered on a priority discussion, and in a good deal of feeling said he did not owe anything to his experiments. I have forgotten the exact words—at any rate, he repudiated his experiments. As a matter of fact, at that time he was doing nothing, and was adding nothing to science. From the moment that he gave up experiments on animals, he equally gave up his contributions to science. As I am speaking about Bell, may I just allude to a point that I have raised a little lower down about neuralgia, namely, that it was Bell's experiments on the facial nerve, which is a motor nerve, as contrasted with his experiments on the fifth nerve, which also goes to the face, but is a sensory nerve, which saved people from having unnecessary operations performed upon them for neuralgia. But it was not until he made this experiment on a donkey that the difference of function between these two nerves was understood at all. Anatomically they had been known, of course, again for something like 1,600 years. Then I wish to draw the attention of the Commission to the fact that as regards investigation of the anatomy of the nervous system, by far the most illuminating way of doing this now is the method of Marchi. The method is very simple. The animal is anæsthetised; the portion desired to be investigated is excised by a little operation of a minute puncture (or more extensive, if necessary), made in it, the wound is closed, it heals in a few days, and the animal is kept for three weeks alive, apparently in perfect health; it is then killed, the brain is taken out, hardened and stained in a particular way with osmic acid. The result of that is that the nerve fibres degenerate from the injured spot, and when the brain is hardened and stained in this particular method, only those degenerated fibres stain jet black, all the rest form a pale yellowish brown background. The result is now that if we make a small lesion in the front of the brain, and there is a fibre going all the way down the nervous system, we can trace it all the way down to its very end. A single fibre is a thing which, of course, is absolutely microscopic, and until this method was devised our knowledge of the bundles of fibres which connect different parts of the nervous system was to a large extent surmise, because the fibres interlace so that you could follow one fibre—it was absolutely impossible. But now by this method, the degeneration method, as it is called, we have a means of investigating the anatomical structure of the nervous system which is unrivalled and is already adding every day, as researches come in, enormously to our knowledge. So that anatomy, which rather misled us in regard to the functions of the nervous system, is now actually being helped from the physiological side. That is rather interesting in this matter of experiments on animals.

15683. You apply both the experiments on the living animal and anatomy of the dead body afterwards?—Yes.

15684. And its condition?—Yes. Then, of course, the surgical treatment of nervous disease includes operations on the brain, operations on the spinal cord and operations on the nerves. In the case of operations on the brain, for instance, you realise that this is a question of being able to treat disease within a closed cavity; you cannot examine the part; you have to form your opinion as to what is going on entirely from your knowledge of the physiology of the brain, and that we owe, of course, in the greatest measure to the discoveries of the late Professors Hitzig and Fritsch, and in our country Dr. Ferrier. That has all happened since 1870, and we are now able to cure epilepsy, we are able to cure abscess of the brain, and we are able to cure tumours of the brain. These conditions, abscess and tumour of course being invariably fatal and causing blindness, severe pain, vomiting, and so forth. Then in operations on the spinal cord the same thing prevails. In fact, the first operation on the spinal cord I am responsible for, so that I know the history of that subject. The technique of that operation I owe entirely to experiments on animals, and I am very glad of the present opportunity to repeat what I have said on that subject. Then as regards operations on the peripheral nerves, I have already referred to Bell's operative treatment of neuralgia as being guided entirely by his experiments on animals. Then we come to the great subject of nerve suture. Injuries to nerves now of course we deal with successfully by dividing the nerves again and sewing them together in various manners; and the initial work bearing upon that subject was carried out by Flourens, who was the first to my knowledge to make experiment on animals, to suture nerves together, to investigate their function.

15685. About when was that?—I have not got the exact date; I think about 1845, but I can find it out.

15686. Professor Langley was giving us some evidence yesterday about the joining of nerves, and the history of it, and he spoke of that knowledge as having been extended a good deal at the present time?—Yes, enormously. I pass from the nervous system to the alimentary canal, because operations on the stomach and intestines are now done on a very large scale, with great benefit to man, and this is being extended to the lower animals, for obstructions of various kinds, and for cancer and tumours generally. The first direct experiments—this is an instance of direct investigation for a surgical purpose—were made by Barischewsky. That was in 1875, but I would like to point out that Barischewsky himself was already indebted to the experiments of Lembert in 1836, who, by his experiments on dogs, showed what had always

been the weakness of this department of surgery, the correct manner of applying a suture. I only mention that again to emphasise what I said about the teaching of students, that the actual technique even in such a minutia as the passing of a stitch ought to be done by experiment first. Then our own observer and great surgeon, Travers, in 1821 had made similar experiments, but in 1875 Barischewsky showed a remarkable series of experiments on dogs, in which he had extirpated portions of the bowel, and proved the detailed technique, whereby dogs could be got to survive. Following upon his work, in the next year, Gussenbauer and Winwarter made direct experiments on joining the stomach and the bowel of the same kind; and then Billroth, on the strength of these experiments, performed the first successful excision of cancer of the stomach, and that really inaugurated not only the operations for the extirpation of the disease but also operations for the anastomosis of different parts in the canal, getting round obstructions and removing dilatations, and so forth. While speaking on this question of the surgery of the intestinal canal I should like to make a remark with regard to fistulæ, because here again constantly the anti-vivisection party represent that it is an exceedingly painful proceeding to make a fistula on a dog to connect one of its ducts or its bowel with the external surface, and to let it heal up. The historical case, of course, of Beaumont, of Alexis St. Martin, is always against that. Every case of colotomy, cancer of the bowel, is of course a direct negation of that assertion, and yet it is repeated, and has been repeated before this Commission.

15687. What is repeated?—That a fistulous opening between the bowel and the surface of the skin causes pain, and that the making of a fistula causes pain. I would like to point out that by one method of making operations to relieve the bowel the wall of the bowel is sewn up to the surface—I am speaking now of an operation on man, of course—the bowel is not immediately opened. Three or four days later you slit open the bowel, and the patient is not conscious of it in the least. You can cut open the bowel, you can cut open the stomach, and the patient not be aware that you have touched him.

15688. Without anæsthetics?—Yes. It is an entire misrepresentation to suggest that animals with fistulæ are suffering pain, although they are kept alive for months, of course, very often. But that was rather in passing. I now come to No. 4—surgery of the thyroid gland. I choose that subject also because it is one that I am personally interested in, and therefore, perhaps, know more about; and here again it is most interesting to see how anatomy misled, and, in fact, hindered, investigation, because even the great anatomist, Luschka, as late, I think, as 1880, suggested that the thyroid gland really was of no service, and that it might have been put by the Creator in the neck from cosmetical reasons to pad it out, exactly like a bolster, for support. Of course he was unaware that Schiff, as long ago as 1859, had made experiments on the extirpation of the thyroid gland, and had shown that it was not an accidental structure, but that it was an organ which was necessary to life. Then the rest of the history of the subject also is interesting, because it shows the necessity of experiments on animals for the direct scientific proof of a biological fact. Sir William Gull, and more especially Dr. Ord, had shown that certain patients exhibited symptoms resembling the cretins in Switzerland, and that this could come on them, as it were, as a disease; and Dr. Ord described that the thyroid gland was diseased in these cases. He had one autopsy. Then Professor Kocher had noticed the same thing in his operations on the thyroid gland, but it was suggested universally, and especially in this country, that this was a disease of the sympathetic nervous system, and that if it had occurred in surgery of the neck it was because the sympathetic nerve in the neck had been damaged during the operation. A Committee was appointed, and I was appointed to make the crucial experiment of removing the thyroid gland in a monkey, in order to see whether that was the cause, or whether it was due to damage of the nerves in the neck; and my experiments proved that it was due to the loss of the thyroid gland. The cost of the work was partly defrayed by the Royal Society and by the British Medical Association. This was 22 years ago. I also suggested, in consequence of these results, that we ought to graft a healthy gland into people as a means of cure. I had no opportunity of doing that myself, but it was done abroad with apparent success. The result of these experiments and of his own researches led Dr. George Murray, of Newcastle, to suggest that we could compensate for the loss of the gland by injecting the juice of the gland, the secretion of the gland, and that was the beginning of the successful treatment of myxoedema and sporadic cretinism. In ordinary typical cases of myxoedema the people lose their hair, they become stupid, the subcutaneous tissues are swollen, and so forth. After treatment their hair begins to grow again, and they recover their mental activity and resume their work, as you will see (*handing in Murray's Book with Examples*). The thyroid gland, I should say in parenthesis, has no duct, and that was the reason no doubt why it was looked upon as unimportant; but in those days people did not know what we know now, that there is a whole chapter in physiology on internal secretion which was not dreamt of when I was a student, and that these researches and others of even a more striking character on the suprarenal body by Professor Schafer and Dr. Oliver, as I say, have opened up a whole new field of work, which certainly would occupy several laboratories alone; so that both from the practical point of view in medicine and surgery, and from the physiological point of view, these experiments on animals have been of very considerable service. That is all I propose to lay before you as mere examples of direct advantage accruing from experiments on animals. I hope the Commission recognise that I have only touched on three or four, and that one could spend a great many days on that subject. I now come to a rather more personal point, because I see that a great deal has been made of it in the evidence laid before the Commission, which is also connected with what I have been stating—namely, Part IV., Experiments on Animals in Relation to Surgical Shock and Incomplete Anæsthesia. In the first place, with regard to surgical shock, it is probably not recognised by the public that in getting rid of blood-poisoning Lord Lister had left practically only one cause of death from operations. There is practically only one cause of death from operations, and that is shock, or collapse; it is the one thing we dread. We never expect a patient to die nowadays from the operation unless an accident has occurred, or if they die from this shock. The importance of this was obvious as long ago as 1895, and Dr. Crile, who is Professor of Surgery now in Cleveland, Ohio, came to University College to study British surgery, and, being naturally interested in the question of shock, I suggested to him that we ought to know more precisely what was the condition of the circulation, as the result of an operation performed in the ordinary way. He therefore commenced under my guidance at University College an investigation, which, I understand, also has been put in evidence here, which I notice has been described as "a series of tremendous experiments," as "very horrible experiments"—in fact, all sorts of adjectives—"repulsive," "loathsome," "filling decent people with disgust and horror," and so on.

15689. Are you referring to some evidence given before us?—Yes, all those adjectives have been applied to these experiments of Dr. Crile's.

15690. (*Sir Mackenzie Chalmers.*) By what witness?—Chiefly by Mr. Coleridge, but in some of the questions that I saw they were described as seeming to be very terrible, and so forth. Anyone who is not a surgeon, of course, might naturally apply such expressions to any surgical procedure. In surgery many things are repellent—naturally repellent—but, of course, we do not hesitate from completing an operation because it is unpleasant. If we did surgery would be in a bad way. What I wish to point out is that there is nothing unusual about these experiments of Dr. Crile's. What he describes as manipulations, in ordinary language are exactly what we do upon the human being. I notice that Mr. Coleridge, for example, selected two things. I will select those two also.

15691. (*Sir Mackenzie Chalmers.*) Are you referring to pages 182 and 183?—No, I think it was in his introductory statement—pouring boiling water into the abdomen, on page 146, Question 10265, in answer to Colonel Lockwood. I should like to begin here: "From the dog we can learn courage, constancy," and so on; "it is the weight of its spleen, or the pressure of its blood, which elicits his curiosity, and he digs into its living body in his horrid quest. I desire to bring before the Commissioners the sixteen experiments performed by Dr. Crile in Sir Victor Horsley's laboratory, in one of which the foot of a dog was

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deliberately crushed 'under incomplete anæsthesia.'" Then follow the adjectives to which I have referred; and a little lower down: "The knowledge of what will happen to a dog when its feet are crushed in pincers and boiling water is poured into its inside seems to me as a layman, as remote from any practical service to humanity," and so on. To anyone who is completely ignorant of surgery and the difficulties of surgery these things may, no doubt, seem extraordinary, but I will take those two points—the crushing of a dog's foot with pincers, and the pouring of boiling water into the abdomen. Taking, first, the crushing of the dog's foot with pincers, that is an ordinary description of what we call tarsectomy.

15692. (*Chairman.*) Is tarsectomy an operation performed on man?—Yes, exactly. I was just about to say that a fortnight ago I operated on a young fellow whose leg had been severely smashed in Canada, and the bones of his foot were all welded together, and fixed on to the rest of his leg—absolutely welded on by callus in such a way that he could not walk. It was necessary, therefore, to perform a partial tarsectomy. If I were to describe that operation in this sort of language I should say a large gash was made in the side of the foot, that a stragulus was seized with pincers and was crushed with other cutting pincers, and was dragged out; then other portions of the bone were similarly crushed with pincers, and the wound was closed. In that case the patient survived. In these cases of Dr. Crile's, the foot was similarly gashed open, if you like, the bones were crushed with pincers in a precisely similar way, but the animal did not survive. These experiments were all done under a licence, and the animal was anæsthetised from beginning to end of the experiments, and its death is described by Dr. Crile in his paper in each instance, and he often made a *post-mortem* examination.

15693. What was the object of the experiment on the dog?—In an operation on the periphery of the body (the foot, ends, limbs, and so on), from accidents in manufactories, the crushing of the bone has been known from the time Pirogoff, the great Russian surgeon, to be a fertile source of shock—surgical shock—so that the first thing to do in investigating shock was to devise some operative procedure such as we do upon man, which should involve the cutting of bone. All cutting of bone is crushing or sawing. Therefore you have these descriptions here of Dr. Crile's of repeatedly crushing bones and sawing bones. He sawed one bone six times, I think. That is in order to imitate on the lower animal what we do on man, and yet they are spoken of as something phenomenal—something extraordinary.

15694. (*Mr. Tomkinson.*) With a view of seeing whether that operation would be likely to be fatal?—No; with a view of seeing what effect it produced on the circulation. These tracings give you the state of the circulation as registered by the blood pressure in a mercurial manometer. If you got an indication of fall of blood pressure it would mean that the patient was becoming subject to what we call surgical shock, and, therefore, you must alter your procedure; you must do something to avert it. That was the object of the experiment—to investigate the effect of blood pressure, so that in all these cases he records respiration, effects on respiration, and effects on blood pressure.

15695. (*Chairman.*) But supposing there was a fall of blood pressure, there would be more danger of collapse?—Yes, certainly.

15696. Supposing you do find at a certain point of your operation on a dog that collapse ensues, or threatens to ensue, how do you apply the knowledge that you gain by that experiment to a human being—what is the benefit to mankind?—By trying on the dog further remedies. For instance, Dr. Crile tried an injection of strychnia and an injection of adrenalin to artificially raise the pressure, and all those things we use nowadays invariably in the operating theatre; no operating theatre is complete without them.

15697. I understand that you selected this particular operation with a view to studying shock and how to avoid it?—Yes.

15698. And you selected this particular operation on the dog because the same operation on mankind was found to be one which frequently involved collapse?—Yes, crushing bones and sawing bones.

15699. Finding collapse or symptoms of collapse in a dog, you then tested remedies?—Yes; that is the whole point exactly. Now let me take the other point, because apparently more has been made of that. What are the words? "The knowledge of what will happen to a dog when its feet are crushed in pincers and boiling water is poured into its inside," and so on. As regards the application of boiling water to intestines probably no one is aware of what is done to a human being in many respects. The use of the actual cautery, of the hot iron, or flame, on the effect of which, again Dr. Crile experimented, is very common, that is to the public. But I do not think it is known to the public that a jet of scalding steam is injected into the womb of a woman for certain forms of inflammation.

15700. Under anæsthetics?—Yes, of course, and it is also injected to stop violent hæmorrhage when you are operating on the liver. These procedures are carried out, and what we must know is how far is it safe and justifiable to use such things.

15701. Are all these things under anæsthetics in the case of an animal?—Yes, everything; and to my mind it is perfectly ridiculous to select a set of experiments on an animal which are parallel to what is done on man and hold them up to reprobation. I do not care whether Mr. Coleridge or anybody else reprobates a thing if one is conscious that one is only fulfilling a moral duty—namely, investigating the causes of suffering, and the means of alleviating it and preventing disease.

15702. And without inflicting pain on the animal?—Precisely; without inflicting any pain.

15703. Assuming that reasonable and proper care is used with the anæsthetics?—Certainly, the same moral principle that one applies in this case as, of course, in the case of the education of students. If an animal is anæsthetised, and is kept anæsthetised until it dies, it does not matter morally what you do to it. I repudiate entirely the comments passed upon these researches, and I repudiate entirely the personal allegations of Mr. Coleridge against Dr. Crile's character. At Question 10285 he says: "I want to show that the personal taste of Dr. Crile does not prevent him from torturing animals." Now, in 1899 Dr. Crile proved in the New York press that these statements of Mr. Coleridge regarding him were untrue.

15704. Which statements?—That he had made unnecessary and cruel painful experiments upon animals without anæsthetics, and that the words "incomplete anæsthesia" meant that the animal was conscious of pain.

15705. (*Sir Mackenzie Chalmers.*) If you are dealing with that, I think we ought to call your attention to Mr. Coleridge's evidence at page 182, beginning at Question 10938?—Yes, and 10939: "Would you accept his own explanation?—Certainly not; of course not." I am quite aware that he does not accept it.

15706. May I also call your attention to Question 10947 and Question 10949?—Yes, quite so; I have read it. Of course, Mr. Coleridge accepts nothing.

15707. (*Mr. Ram.*) And Question 10951?—Yes, quite so; he accepts nothing. But my point is that he repeats these charges again and again after they have been publicly proved to be incorrect. I want to point out that his personal allegation against Dr. Crile is wholly unjustifiable in view of Dr. Crile's refutation of the accusation in 1899; that is eight years ago.

15708. (*Chairman.*) To come back to what you did not quite finish, Mr. Coleridge says at Question 10285: "In Experiment CXXXIII. we find these words . . . . 'experiment lasted 2½ hours. Flame was applied to the dog's paw, and we are told in the control experiments, as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame'"?—Quite so.

15709. You were going to say something as to whether there was incomplete anæsthesia?—I come to that a few minutes later.

15710. Perhaps, as I have read that statement, you would not mind answering it now?—The statement there that the dog was not under full anæsthesia means that some reflexes were present, and that is certainly probable—the withdrawal of the leg on application of the flame; but that dog was perfectly unconscious to pain.

15711. Were you present at the operation?—No, but Dr. Crile says so in his introduction to his paper. He says that all these experiments were done on animals anæsthetised.

15712. He says: "We are told in the control experiments, as well as in this, the dog was not under full anæsthesia." Do you know by whom he is told?—No, that is Mr. Coleridge.

15713. I gather that he is told in this very account. Is that the account given in Experiment CXXXIII.?—I will look it up.

15714. (*Mr. Ram.*) In the following answer to Question 10288, you will see: "The animal did not take the anæsthetic well, and the entire experiment was made under complete anæsthesia"?—Here is Experiment CXXXIII. What happened was that the flame was applied before and after. It begins: "Fox-terrier, weight 15 kilos," and so forth, "Chloroform and ether anæsthesia." It was under chloroform and ether. Then "applied Bunsen's flame to the paw" before and after he had injected the sheaves of certain nerves in the leg with cocaine.

15715. (*Sir Mackenzie Chalmers.*) Keeping up the general anæsthesia?—Keeping up the general anæsthesia. And then he says, "in the control experiments as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame; after the injection of cocaine he did not," showing that the cocaine paralysed the nerves. That was the point of that experiment.

15716. (*Chairman.*) The nerves which produced what you say were reflex movements?—Yes.

15717. (*Dr. Wilson.*) But "struggled" would imply more than reflex movements?—Oh, no.

15718. Persistent struggling?—It depends upon what you mean. Do you mean simple struggling, or a purposive movement?

15719. Purposive movement—struggle?—Purposive movement and struggle are not the same at all. People struggle violently, and they require several people to hold them down sometimes in an operation, but that is quite a different thing from purposive movement.

15720. That is from the effect of the anæsthesia?—No—during an operation. But to return to my point, my point is that although these experiments have been treated in this way as something particularly outside the ordinary, they are not so—that they are what they were deliberately intended to be in the original purpose of the investigation, a repetition of procedures performed upon the human being which are capable of producing shock; and, if so we ought to know how that shock was produced.

15721. (*Chairman.*) There is another passage which I think it would be well if you would explain in answer to Question 10289 about ten lines down; there is a description given by Dr. Crile himself, I presume, of an operation in your laboratory?—Yes.

15722. Then he says: "Under incomplete anæsthesia crushing the foot caused a very sharp rise, followed by an equally sharp decline of pressure. This was repeated several times." What does "incomplete anæsthesia" mean there?—What it means here is that the anæsthesia was not so deep as to exclude a reflex effect upon the blood pressure. The sharp rise and sharp fall are the rise and fall of blood pressures which are produced when you stimulate the central end of any sensory nerve.

15723. I call your attention to those statements because certainly, to an uneducated mind, as mine is in these matters, reading that a particular operation was performed under incomplete anæsthesia, and that an action likely to cause pain was followed by a sharp rise, would imply that there was not sufficiently complete anæsthesia to prevent pain—that pain was felt?—That is the whole point in regard to the meaning of the word "incomplete."

15724. It is important to explain that as the evidence will be read by many persons, I dare say, who will not understand any more than I do the technical language of surgery?—Quite so. I want to devote now a certain portion of my evidence to that subject alone, namely, incomplete anæsthesia, because here again the amount of ignorance that prevails on this subject is very great. By the term "incomplete anæsthesia" we do not mean that the object of the operation or experiment, as the case may be, is conscious of pain at all. All that we mean is that its nerve centres are not so poisoned that it is not capable of making various demonstrations, even reflex acts. Another expression for the same thing that is very frequently used is "light anæsthesia," or very light anæsthesia.

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15725. Are those common and well-understood terms among surgeons?—Very.

15726. Would any surgeon reading this account understand it in the sense that you are giving to us?—Certainly. A surgeon, Mr. Berry, was reading a paper last night on the removal of goitres, and he insisted that anæsthesia should be very light, and that it should be such, that the patient could be roused even to vomit. The real thing is that hitherto owing again I suggest to want of knowledge, the anæsthesia in most operation cases has been far too deep—of that we are quite certain in ordinary surgery; and the introduction of the method of giving chloroform by known doses, to which I referred before, has brought us now to find that during an operation you can very often give no anæsthetic at all for eight or ten minutes, and, in the case of a child, for twenty minutes. The person will remain sound asleep and absolutely indifferent to pain. Then comes the question, when are you to give him any more? In answer just now to a Commissioner, I said that my guide is what I call a purposive movement. If I see a movement which is not a simple reflex withdrawal, or that sort of thing, if I see a movement which seems to have a more purposive character, I always at once order the anæsthetic to be increased. It is perfectly easy to recognise.

15727. Do you wait for a purposive movement, or do you anticipate it in any way?—Frequently we wait for it. If there is any evidence of shock, if there is any reasonable evidence of shock, we wait for it. People admitted into hospitals with a leg completely smashed do not usually suffer pain. A man may have his leg completely crushed by machinery up to the middle of his thigh, and as a rule he is not in pain; he is simply lying there absolutely shocked, perfectly conscious—he will talk to you all about it, but not exhibiting any sign of pain. And, further, there are many factors during anæsthetisation which have yet to be worked out, and this "incomplete anæsthesia" is a degree of anæsthesia under which it is only safe to perform some operations, on the tongue, the throat, and so forth. As a great deal has been made on this point of "incomplete anæsthesia," and an immense amount of evidence has been laid before the Commission, I should like to take the subject up again.

15728. (*Chairman.*) You were going to refer to some instances of error or misstatement about incomplete anæsthesia?—Yes. This is really a much wider question than a mere mistake as to the degree of unconsciousness to pain, because, as a matter of fact, the anti-vivisection party have preserved their existence wholly, I think, on the assumption that cutting operations are performed in England without the employment of anæsthetics, or with a very inadequate employment of anæsthetics. I would like to point out that this subject dates from 1899, when the first attack was made upon Dr. Crile—that in 1899 Mr. Paulton, in the House of Commons, asked a question of Sir Matthew White Ridley, which conveyed the assumption that experiments of this kind were done. In 1899 Mr. Coleridge attacked Dr. Crile at the Home Office, and after he had been fully informed by the Home Office as to the meaning of this term "incomplete anæsthesia," he then wrote to several surgeons, and asked them a question about incomplete anæsthesia, and he informed this Commission that he wrote exactly in the terms in which the Home Office had written to him. As a matter of fact, he did not write exactly in the terms of the letter of the Home Office, because he omitted the all important sentence which defined this term "incomplete anæsthesia." And the result was that he deceived these surgeons into thinking that it was suggested that operations were done upon human beings in hospitals without adequate narcosis. Subsequently they found out (but not until 1902) the manner in which they had been deceived, and the way in which it all came out was so striking that I venture to lay it before you. In 1902 Mr. Coleridge issued to the public a letter, addressed as follows. It was addressed to ladies: "Dear Madam,—This catalogue, which I respectfully ask you to look at, has been translated

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from the German. In reproducing it thus, I have followed the immortal injunction:—

. . . 'Nothing extenuate,
Nor set down aught in malice.'

I place it, therefore, in your hands, and leave it to exercise its influence upon your heart. If it lead you to feel that anything is better than that such things should be, if it lead you to know, beyond the reach of gainsay, that pitifulness is a higher thing in the sight of God than knowledge thus obtained, you will send me your help, great or little, according to your means, that I may do what I can as effectively as you make me able, to put an end to these unspeakable deeds, and I shall continue to be your and the poor animals Ever faithful servant,—Stephen Coleridge." That letter is fixed into a catalogue, which is a translation of the German catalogue of an ordinary surgical instrument maker, who makes physiological apparatus for holding animals. In that letter nothing is said about these animals being fixed in this way on tables and boards under anæsthetics. There is not a word about the use of anæsthetics. I have had that catalogue sent to me over and over again by ladies, asking what this means. And it is all part of this constructive misstatement, that experiments are done in England without anæsthetics. This particular instance, I think, is the most immoral really, of any act of the anti-vivisection party, because the "Times," in 1902, published a serious article upon it, drawing attention to it, and subsequently a correspondence arose in which I took part, and in which we appealed to Lord Llangattock, the Duke of Portland, and several bishops who had supported the issue of this misleading document to withdraw it. They have not done so. And it does seem really a terrible condition of things, that persons holding such high authority should lend themselves to the issuing of a statement of that sort.

15729. A statement suggesting that all this is done in England and without anæsthetics?—Yes. Then, further, this misrepresentation is still going on; some 50,000 copies of this apparently have been circulated. It was corrected, as I said before, in 1902, and Mr. Coleridge stated to this Commission, at Question 10559, that where he was shown anything which was wrong he liked to correct it. I may say in parenthesis that he has never corrected anything which he has been shown repeatedly was incorrect, nor has he ever apologised; but that is only in passing. I simply mention this to show that since 1899 there has been this systematic campaign. From Mr. Coleridge's evidence we now know that he bases it all on his reading of Certificate B. His reading of Certificate B is that Certificate B enables workers in this country to do experiments without anæsthetics or to do them in completely—that is to say, that he was pleased to speak of an initial operation and then of the rest of the experiment. Neither the Home Office nor the scientific workers in this country have recognised such a distinction as regards any cutting operation. And as regards the actual term narcosis, anæsthesia, and so forth, it has always been interpreted by the workers to mean unconsciousness of pain, therefore, whatever phenomena are observed (struggling, the corneal reflex, change in pupils, change in vaso-motor pressure), they are all phenomena which obtain with the animal unconscious of pain. As I said before, this is an important subject, because it has led to the production of this wholesale misrepresentation—to that misrepresentation being still supported by certain persons who would not wittingly, of course, support an immoral Act, who as leaders of the anti-vivisection party (take Bishop Mitchinson, for instance, and the Duke of Portland), were appealed to in 1902, in the public press, to withdraw their support from Mr. Coleridge's letter and catalogue; but they have, nevertheless, continued to give their support. It is not surprising, therefore, that this matter has come before this Royal Commission, and I shall be very glad, in cross-examination, to answer any points in relation to it. I have done my duty, I think, in laying before you a succinct statement of the way in which this particular point has been pressed. And so we come to the last part of my *précis*, the existing Statute of 1876, its administration by the Home Office, and suggestions for its amendment. The Statute itself, of course, naturally, as it is 30 years old, is no longer quite up to date; and it was originally constructed by some one, of course, who was evidently not a scientific physiologist or pathologist, because it might have been very much simplified. In the first place, I take it that the principles of licensing and registration will both be preserved; there is no objection to them, and they certainly must be a satisfaction to the public, therefore they will be preserved. The only question comes to be a consideration of how they can best be regulated. In the first place, as regards special conditions in the licence, assuming that a man has been given a licence, I notice that Mr. Coleridge made several so-called charges against the Home Office. It is not my purpose, of course, to go through those; it seemed to me that they were all baseless; but there was one particular charge, in which he mentioned my name frequently, and therefore we may as well turn to that.

15730. (*Sir Mackenzie Chalmers.*) I was going to ask you upon the tenth charge, which is the specific one?—Yes, I will now enter upon it, Question 10645.

15731. It begins earlier?—Yes, he begins it in his opening statement.

15732. He begins in the last words of Question 10262, on page 145, in the first column; he there formulates the charge: "I am here to charge the Home Office officials"?—These are his words: "(10) I am here to charge the Home Office officials with placing a certain vivisector year after year beyond the reach of the safeguards erected by the Act' (that is the important sentence) "to protect animals from illegal treatment by licensees, by giving him permission to vivisect in private places, thereby placing him beyond the possibility of legal inspection; because by clause 10 of the Act of 1876 the Inspector has no right of entry into, or inspection of, any unregistered place." We will leave that, because that will come up again afterwards. Then the next question is: "What is the name of that particular person? (A) Victor Horsley." Then, again, here is further detail of it at Question 10645, on page 170, in the second column.

15733. And then the next day it comes on again?—Yes, at 11089, page 190. I should like to read the words in the first part of his answer to Question 11089: "We say that there is no reason in the world why Sir Victor Horsley should be placed by the Home Office outside the possibility of inspection." That is my point. I say that that charge against the Home Office is wholly and utterly baseless. There is no foundation for it in real fact. The facts are these: On the 10th November, 1893, I received a letter from the Home Office in answer to an application of mine for leave to do two experiments away from a laboratory, in a private house. These experiments were the inoculation of blood from a patient suffering from filaria sanguinis hominis. This is a particular worm which discharges its embryos into the blood of a patient at night time. The embryos do not come into the blood until about 10 o'clock at night, and they flourish in the blood when the patient is lying half unconscious or unconscious in sleep, and when he wakes in the morning and begins to move about, these embryos disappear again, hence the only time you can get the living embryos is in the night, so that it is necessary to do this inoculation in the night time. All the laboratories are closed at night, and Sir Patrick Manson, for whom I was going to do the inoculation, brought the patient to a private house for me to do it. As a matter of fact, we examined the man's blood, and we found that he had no embryos in his blood, or hardly any, the reason being that he had injured the lymph glands in which the parent worm was living, which had caused inflammation, and had caused the death of the parent worm, and there was an end of the experiment and there was an end of the whole matter, because I have never done any experiment in any unregistered place at any time in my life before or since. As regards the action of the Home Office, their action has always been perfectly clear. They have never given me any such leave as has been alleged to you. May I read their first letter? "I am directed by the Secretary of State to acknowledge the receipt of your letter of the 9th inst. respecting permission to inoculate two monkeys at your private house, if possible, in pursuance of the licence held by you for the performance of experiments under the Act 39 and 40 Vict., c 77, and, in reply, I am to acquaint you that the Secretary of State will, under the circumstances, consider the experiments proposed as experiments to be performed under your licence, which will be treated as current." "I am to add that a special condition will be appended to the licence, exempting these particular experiments from the condition restricting the performance of experiments to the specified places." In other words, the Home Office knew exactly what I proposed to do to inoculate

monkeys with this infected blood and nothing more, and they gave me leave to do that and no more, so that it was an absolutely restricted state of things.

15734. Was a certificate given with it?—No, I only obtained that letter.

15735. But there was a condition imposed, was there not, that you should report?—I am now coming to explain that. That was on the 10th of November, and on the 21st of November, my licence being due for renewal, I sent it in, and when the Home Secretary renewed the licence he said, "The following condition has been added to your licence. 'And whereas the said licensee was authorised on the 10th day of November, 1893, to perform certain experiments at 25, Cavendish Square, in the County of London, I hereby vary the first condition of the said licence, so that the said licensee may perform experiments at such place or places as may be necessary, provided he report to the inspector on each occasion that he performs any experiment at other than the registered places as authorised by his licence.'" The reason why that permission was given at all was that Sir Patrick Manson and I had not time between the 10th and the 21st of November to do the experiment. I mentioned that to the Home Office, and they gave me this condition, but you will observe that when I proposed to do an experiment at all I was to report it to the Inspector, and my original permission was only to do two experiments, and finally, no experiments have been done under these permissions at all.

15736. (*Chairman.*) The operation which you intended to do, if the patient had been suitable, would have been taking some blood from the patient?—Yes, and just injecting it with a hypodermic syringe into a monkey.

15737. And you would draw it out with the patient's consent?—Yes, and then just inject it into the monkey like a vaccination.

15738. That was the whole operation?—That was the whole thing, but what I wanted to do here was to produce documentary evidence that the Home Office had not given me, to use Mr. Coleridge's words, unrestricted leave to do experiments when I liked, where I liked, and what I liked. They never did anything of the kind, because, in the first place, the nature of the experiment was strictly defined, and, in the second place, I was to give notice to the Inspector if I did anything at all.

15739. (*Mr. Ram.*) Before you leave that, on page 170, Mr. Stephen Coleridge refers to this condition, and quotes a letter that he wrote to the Home Secretary in the middle of the second column on that page: "I wrote to the Home Secretary on the 25th of July, 1905, and I said: 'On page 27 there is a note' (I was then alluding to the yearly report), 'which runs thus: Sir Victor Horsley could also perform experiments at such places as might be necessary for the purpose of his experiments.'" Do you know whether on that page 27 which is there quoted there was the limitation that you have just told us of that you were to report?—No, there was not. Here is page 27, and there was no such statement of limitation (*handing in the same*).

15740. (*Sir Mackenzie Chalmers.*) That is the return?—Yes, that is a copy of the Inspector's return.

15741. (*Mr. Ram.*) Again on the same page, at Question 10654, it is suggested that your licence was withdrawn, or your leave was withdrawn in an illuminating way, as he puts it, just before this Royal Commission was appointed. Can you tell us when it was withdrawn?—No, I have not kept it. The matter was certainly referred to when the licence was renewed at the beginning of 1906, when the Home Secretary said that he did not propose to continue the condition. That is all so far as I remember. I have not kept that letter, but the leave was not withdrawn in any special way at all. With every letter that we receive the Home Secretary puts down such-and-such conditions, and if they are conditions which are no longer necessary or are not operative, he just withdraws them when the licence is sent in for renewal.

15742. But at this time, as I understand, there was no object in your having leave to perform experiments in any private house?—Not in the least.

15743. (*Chairman.*) Mr. Coleridge describes the withdrawal in this way: "It is a very remarkable thing that, contemporaneously with the knowledge in the Home Office that there would be a Royal Commission, this leave was withdrawn from Sir Victor Horsley to vivisect where he liked and beyond the reach of inspection"?—That statement, you see, is absolutely untrue. I never had leave to vivisect where I liked, or beyond the reach of inspection. The Home Office never gave me that leave, and that statement is wholly untrue.

15744. The leave that they gave you was for two operations only?—Yes.

15745. And you never used it?—It was for two inoculation experiments only, and I never used it.

15746. (*Mr. Ram.*) In respect of which you were to report?—In respect of which I was to notify the Inspector before I did it.

15747. (*Mr. Tomkinson.*) You never did those two experiments?—No.

15748. They were only inoculations?—Yes.

15749. They were not vivisections at all?—No.

15750. (*Chairman.*) Did the Home Office write to you and say that that condition was no longer in force?—The Secretary of State did not continue it.

15751. How long after it had been given you?—A long time—about seven years.

15752. (*Sir Mackenzie Chalmers.*) Did you come across any other cases of *filaria sanguinis hominis* afterwards?—No, we did not.

15753. I suppose the probability is that the condition was kept open because you might have come across another case?—Yes, quite so; I never took any more notice of it. It was only obtained for a special occasion, and I never bothered about it any more.

15754. I suppose these cases are very rare in England?—Extremely rare.

15755. You may wait some years before you get another suitable case?—Yes.

15756. Is that the disease which produces elephantiasis?—Yes, it is. That brings me at once to the certificate system, because, on looking through the so-called charges against the Home Office, and casting my memory back now over the Act—because, as I said before, it really extends over all my professional knowledge—I can trace any difficulties with the administration, and I can trace special cases in which the anti-vivisection party have made capital, in a large measure to the certificates, and if you please I would like to mention points on each certificate in exemplification of my meaning, and then I would like to conclude by pointing out why the certificates are not necessary at all, and why they only complicate legislation on the subject.

15757. (*Chairman.*) Will you please do so?—In the first place, Certificate A, the inoculation certificate, says that the experiment is going to be done without anæsthetics, and yet it is an inoculation experiment. When I was Secretary to the Government Committee on Rabies it was my business to inoculate rabbits through a little trephining operation. I gave them an anæsthetic in order to make that trephining operation to do the inoculation, but Mr. Erichsen, Sir John Erichsen, as he was afterwards, told me that was violating the Act, because I was giving these animals an anæsthetic, and I reported them under Certificate A, which said that the experiment was done without anæsthetics; and he said that my conduct under Certificate A ought to be that I should do the trephining operation on the rabbit without anæsthetics. I pointed out to him that it might be a breach of the law, but that it was an absolutely absurd position. I suggest that this is a difficulty that crops up from time to time. I know of other experimenters who have performed rather considerable inoculations, who have always given the animal an anæsthetic, but, owing to the hide-bound wording (unnecessarily hide-bound, of course) of the Act of Parliament, it happens that that action of the experimenter conflicts with the Act.

15758. Is that in the Act or in the form of certificate?—In the form of certificate. I beg your pardon.

15759. (*Sir Mackenzie Chalmers.*) The only thing would be that you would do it under Certificate B. If you had Certificate B there would be no difficulty?—But then it is not classed as an inoculation experiment.

15760. That may be scientifically wrong, but practically for you to do that under the existing Act you would be recommended to take out Certificate B as well?—Yes.

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15761. You would do the operation under anæsthetics, and the animal would be allowed to recover from the anæsthetic for the purpose you wanted?—Yes. I now come to Certificate B, and as regards Certificate B, I once more would draw attention to Question 10319. Mr. Coleridge in his evidence endeavoured to persuade the Commission that the practical administration of Certificate B was (as is shown in Question 10,319, page 150, in the middle of the second column) that the operator would do the cutting operation then the fixing on the apparatus, and then he would go on making his observations, not giving the animal any more anæsthetic. I have seen a great deal of experimental work in this country, and such an idea has never, to my knowledge, entered the head of any experimenter, and I am perfectly certain it was never in the mind of the Home Secretary in issuing the licence and certificates to us. The whole idea of administering of Certificate B was that an animal which was going to have a cutting operation performed upon it should be under an anæsthetic from the beginning of the operation until the dressing was applied and the animal removed to its cage, exactly the same as with a human being; and on that idea alone has Certificate B been worked. Then I will leave Certificate B. We come to Certificate C. Certificate C is, of course, a special certificate that is required for demonstration to students. As I have pointed out, I think it is absolutely unnecessary. A person who is duly licensed and registered is perfectly fit to demonstrate to students; he does not require any guarantee in the shape of Certificate C. Then Certificate D was constructed in consequence, apparently, of the original drafting of the Act, which made this extraordinary statement that "Experiments may be performed not only directly for the advancement by new discovery of physiological knowledge, or of knowledge which will be useful for saving or prolonging life or alleviating suffering, but for the purpose of testing a particular form of discovery alleged to have been made," and so on, "on such certificate being given as in this Act mentioned that such testing is absolutely necessary for the effectual advancement of such knowledge." That presupposes two things: In the first place, that you can repeat a former experiment absolutely; and in the second place, that such repetition ought to have the special sanction of the Home Secretary. Now, that is wholly contrary to science. No man has ever repeated an experiment without learning something additionally new. You cannot perform an experiment absolutely identically like the previous one. You perform it on a different animal; you invariably learn something new. However closely you attempt to approximate the factors of the experiments in the two cases, you do not get an absolutely identical result, and in that way a great many extremely useful discoveries have arisen. Then as to the necessity of having special permission to extend work, it seems to me that that is contrary to the whole spirit of the Act, which, after all, was to promote the further discovery of physiological knowledge. So that this certificate for so-called repetition experiments is unnecessary, and is contrary really to the progress of scientific work.

15762. (*Chairman.*) Would you suggest some amendment to this, or strike it out altogether?—I would strike it out altogether—repeal it altogether. It is not only not necessary, but it is also contrary to the fact. It was evidently inserted in the Act by someone who had no knowledge of physiological experimentation. That is what I mean. I suppose Lord Playfair was responsible in a great measure for the drafting of this Act, and he was a chemist—he was not a physiologist; but whoever was responsible evidently in many respects was not a physiologist—he did not know the actual conditions surrounding experiments on animals. Then we come to Certificate E, and although I have no charge to bring against the Home Office, I did not say that I had no charge to bring against the Home Secretary. Certificate E, I take it, is never applied for now. Certificate E proposed to allow experiments on dogs and cats without anæsthetics—those are the words. Now, when Mr. Matthews was Home Secretary he insisted on Certificate E being filled up by all the experimenters who wished to use dogs and cats, and we protested. We went to him as a deputation—Sir James Paget, Sir Andrew Clarke, and myself. We pointed out to him that this placed experimenters in an entirely false position; that they did not do experiments on dogs and cats without anæsthetics, that if the Home Office wished to know when dogs and cats were used they could be notified, but that to compel us to sign a paper which was not true seemed to us most unjust; and we asked him, therefore, to withdraw Certificate E. I am sorry to say that Mr. Matthews refused, and it was not until we came under the enlightened *régime* of Mr. Asquith that this question was settled; because we made exactly the same representation to Mr. Asquith, and he immediately said, "Certainly, I recognise the injustice," and he constructed the Certificate EE to take the place of Certificate E, which was exactly the same certificate only it left out the words "without anæsthetics." I mention this to the Commission to show that as regards the plenary power of the Home Secretary under the Act—although as a rule, of course, it is worked extremely wisely—there may be examples, like that of Mr. Matthews, in which it may be worked extremely badly. I think the Commission ought to have the fact before them.

15763. (*Mr. Ram.*) Did Mr. Matthews give any reason for adhering to this Certificate E?—No; he simply wrote a letter in which he regretted that he was unable to comply with the wishes of the deputation. We never had any reason from beginning to end. It was evidently simply a matter of internal administration, because Mr. Asquith drafted Certificate EE, and we have worked under it ever since. But I point out that that occurred in 1888.

15764. (*Chairman.*) You mean Mr. Matthews's refusal?—Yes, and that the statistics consequently of the experiments on animals then were not correct. For instance, when I was called upon (and I know many other experimenters when they were called upon) to send in this Certificate E, struck out the words "without anæsthetics," and I certainly had the document returned to me twice, and sent it in again twice. I would not sign the original Certificate E, and ultimately it was allowed with the words struck out by myself.

15765. (*Sir Mackenzie Chalmers.*) So that now in all cases the certificate shows the fact—namely, that experiments on dogs and cats have to be performed under anæsthetics?—Yes; the working now is perfect. There is nothing to complain of at all. But I only mention it to show how the certificate system as a Schedule to the Act of Parliament is liable to misapplication, especially in regard to any physical science, considering that physical science is always altering and advancing. Then the last certificate, F, is another of these special certificates. It is really absurd, of course, that a special certificate should have to be got for experiments on ungulates or special ungulates. It is contrary to science.

15766. (*Chairman.*) What is contrary to science there?—For any one class of animal to be selected in contradistinction to the whole animal world. We ought to have general permission to do experiments on any kind of animal. There is no reason why any one animal should be, as it were, removed from the sphere of the Act.

15767. (*Mr. Ram.*) You may do it on a cow, but not on a horse without getting Certificate F?—Exactly. It is absurd from a scientific point of view—I mean, of course, it was not absurd to those who drew it up. So, summing it all up, I suggest to the Commission that these certificates, which give endless trouble to fill up, are really of no use. Then, of course, the question arises, what use is made of them at the present time, and how are you going to meet it. Very important use is made of them at the present time. The Home Secretary must know, both for his own information what is going to be done in research—he must know what is going on during the research, and he must know what the results of the research are, for his own information, as I said, and also for report to the House of Commons. Now we need not trouble about the middle point, what is going on during the research, because that is provided for by the system of inspection; but as regards the first point, all that is necessary is to do what is actually done at the present time, to insert on the application for the licence that the applicant proposes to do such and such experiments on such and such animals in such and such a way. It covers all that. Then when it comes to the collection of statistics for report to the House of Commons, that depends simply and solely on the form. The present form that we fill up at the end of the year could very easily be expanded in this sort of way: You could have the first column "Class of Experi-

ments—(1) Animals killed before recovery from anæsthetic; (2) Animal allowed to recover from anæsthetic; (3) Animals inoculated." That really covers the whole ground as a matter of fact. Then the next column would be "Number of Experiments Performed"; the next "Number in which Pain was Observed to Occur." Third, "Animal Employed." In a simple form of that kind you have all the facts which are supplied by those certificates, and in a form which would make their being collected and reported upon extremely simple and easy. So that the certificates are wholly unnecessary. They have led to misunderstanding, and they give great trouble to the applicants who have to get them signed and filled in. And, further, Mr. Matthews added another difficulty; that was the restriction as to their numbers or their duration. It is perfectly ridiculous that the Home Secretary, who is very rarely a scientific man, should say whether ten experiments or twenty experiments are necessary for research, and yet that is done every time.

15768. (*Chairman.*) Of course he does not act without some advice?—Well, my lord, when Mr. Matthews put these restrictions of time and number on the certificate form I suppose he was advised to a certain extent by the Inspector, and so on; but, at the same time, the investigator himself does not know how many experiments may be necessary.

15769. I dare say that is so. I was only pointing out that the Home Secretary does not rely solely on his own scientific knowledge?—Yes, I beg your pardon. I see your point.

15770. (*Sir Mackenzie Chalmers.*) You mean that it has this disadvantage: that the person applying has to apply for an extra number of experiments, more than probably would be necessary for him to perform?—Quite so, and not only that, but one other condition that was devised (I have forgotten how long ago it was—it arose out of Mr. Matthews' regulations) was that the investigator might do, say, 20 experiments, but after ten he was to report to the Inspector, but the fact that the Inspector knew that those ten had been done made no difference to the research under the present system of record keeping that we have of experimental work; all this is very much simplified; because some years ago, when Mr. Ritchie was Home Secretary (Lord Ritchie as he was afterwards), the blue form on which the experiments were recorded was given up, really I think in answer to a protest of mine, namely, that the investigator ought not to be put to the trouble and annoyance of having to write all the records of experiments twice over; and Mr. Ritchie then made a standing regulation that the records of all the experiments were to be open for the visit of the Inspector as, of course, they are. Consequently these details, which are inserted on the certificates, and under which the certificates were allowed to be obtained are always available. Therefore that is another reason for regarding the certificates as unnecessary, a waste of time, giving great work in the office, and leading to misunderstanding.

15771. (*Chairman.*) You would have a licence and no certificates?—Yes, a licence drafted in the way I have suggested, and a form of return to be filled up by the investigator at the end of the year would cover the whole ground. Then as regards the plenary power of the Home Secretary, that would remain, of course, and he would insert any special conditions on the licence which he thought necessary.

15772. Permanent conditions, do you mean, or special conditions?—They would last for a year, because the licence always expires at the end of the current year.

15773. Suposing he wanted to give you a licence for inoculating two monkeys, how would he deal with that?—He would endorse it on your licence; he would ask you to return the licence, and endorse it on the licence.

15774. You must have some special application for it?—Yes, that is an ordinary letter.

15775. (*Sir Mackenzie Chalmers.*) Then you would eliminate the advisory bodies in that way. At the present time certificates are given by outside bodies of scientific people?—No, they would have their place in the granting of the original licence; and, of course, they would be the people to appeal to as to whether the investigator was a fit person to conduct the investigation which he proposed to do. Now their advice is sought really three times over for the same thing. If a man gets a licence and Certificates A, B, and E E, which is about the average, they really sign the same thing three times over. It is quite unnecessary.

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15776. (*Chairman.*) Does that exhaust your *précis*?—If you please.

15777. (*Dr. Wilson.*) What body would you recommend to advise the Home Secretary, then, as to the need of new experiments in the field of research, instead of the Association for the Advancement of Medicine by Research?—I really have not thought of it. I do not know of any body at this moment that I could name.

15778. Would you object to the General Medical Council, for example, as the advisory body?—I should object very strongly. I was a member of the General Medical Council myself. It is not at all a body that could do it. You cannot call them together, except at fixed times. Of course, the personnel, as personnel, would be all right naturally; many of them are teachers and know the requirements of education.

15779. But through a Committee of that body could it not be done?—Certainly. The expense question you know in the General Medical Council is always a very difficult one.

15780. Yes, of course, that would have to be met in some way. May I ask whether you are appointed by the Council, or a Committee of the Council of the Association to appear?—The Council.

15781. It may be conceded that the vast majority of the medical profession are in favour of experimentation on animals, but do you think that they take that view just because they thoroughly know the subject, or are they influenced by the experiments they have seen in the class rooms?—The vast majority of them, only from their general knowledge up to about ten years ago. Men qualified during the last ten or fifteen years have seen the experiments, of course, but there are many schools now that do not show experiments at all, and I do not think they teach well either.

15782. I see you allude to the huge total which has been expended by the Association during recent years in the form of grants in aid of research?—Yes, and scholarships.

15783. Do not these grants tempt some of our best students to go in for research, and some of the ablest young men who are just starting in practice?—They enable many who could not otherwise afford it to do so, the scholarships especially.

15784. Then there is the Ernest Hart Fellowship?—Yes, the Ernest Hart Scholarship for Public Health, three scholarships of £150 a year for general science, and then about £400 is expended in grants.

15785. With reference to these grants—I know that all the results of the investigations in the fields of research have appeared in the "British Medical Journal," but has any compendium ever been made to enable one to judge of the advantages that have been gained?—Not that I know of. I remember years ago some considerable number of researches were collected in a little volume, but that has not been done for many years.

15786. Is it not the fact that many of its students who obtain these scholarships go to Continental schools to complete their studies?—They often do.

15787. And there they become imbued, of course, with the doctrines there taught?—Yes.

15788. And is it not also the fact that the majority of the teachers in medical schools are selected from those young men who go to Continental schools?—Certainly.

15789. And should I be right in stating too that almost all the leading articles on questions of research that appear in medical journals, or even in the daily papers, are inspired by these young enthusiasts—because the older men can rarely speak or write on the subject?—I do not know what articles you refer to.

15790. I mean articles on research that you see constantly in medical papers and in the daily papers too—medical research?—Yes, I daresay. I do not know what articles you are referring to at the present moment.

15791. I want to get at this—how medical opinion with regard to these questions is moulded really?—Oh, I see. I cannot tell you. I should have said that the medical opinion was simply the expression of the

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general scientific knowledge of the profession at the time.

15792. I will put it in this way: Though the consensus of medical opinion may be in favour of continuing experimentation on animals, do not you think that the consensus of medical opinion is always a somewhat unreliable court of appeal?—Do you mean, is it possible that the medical profession are mistaken on this point?

15793. Some may be?—That, of course, is very difficult to say. I do not personally think that they can be mistaken on the point, because one sees the present position of scientific research is nothing but the logical outcome of all knowledge that has been going on for centuries. It has not begun recently; it has been going on since the time of Galen; it is all one continuous evolution. I cannot believe that the medical profession are mistaken on a matter like that, at least it does not seem to me quite reasonable.

15794. Talking about this consensus of medical opinion, do you think that if your proposal to operate on dogs to teach students practical surgery, as they do at the John Hopkins Hospital, Baltimore, were submitted to a plebicite of the profession you would obtain a large majority in favour of it?—I do not know. I think probably I should obtain a majority, certainly. And I have not limited the number of animals to be operated on to dogs at all; on the contrary, I have always held the view that the sheep was an animal that particularly lent itself for students' work. The arteries in sheep are the same size as our arteries, the circulation in many respects is the same. The difficulties of operating upon it are the same. The sheep is really a very good animal for research; it is only a question of cost.

15795. Should I be right in saying that beyond setting an occasional fracture or dealing with a rare accident the general practitioner nowadays is seldom called upon to display any surgical skill at all?—Pardon me, the discovery of the antiseptic system has completely revolutionised medical and surgical practice. In old days surgical practice used to be reserved to so-called operating surgeons. General practitioners nowadays do all operations—many of them do very large operations—such as extirpation of the kidney. That has all altered even within my time. General practitioners operate very largely in cottage hospitals all over the country.

15796. Yes, if they are connected with hospitals?—Naturally, if they have hospital practice they continue it in their general practice.

15797. I mean in general practice?—I mean in general practice.

15798. You contend, do you not, that the recognition of tissues, their texture and treatment in practical surgery can only be taught on the living body?—Yes, I say it can only be adequately taught in that way.

15799. And that for the purpose experiments on living animals are essential?—Yes.

15800. You have been talking about Listerism. Surely the revolt against the use of the carbolic acid spray which constituted the leading feature of antiseptic surgery was not brought about by experiments on animals. Because there was a revolt against the use of the carbolic spray?—No, there was no revolt against the use of the carbolic spray at all. The question was whether it was necessary to have this disinfection of the air. It was shown by experiments that air was not toxic to the degree it was believed to be: that organisms present in the air to a large extent were moulds and fungi, which were not particularly poisonous to animals; that the organisms which were dangerous to animals and to man were those which were carried in on instruments, and for that reason people began to risk leaving off the spray, and gradually it was left off altogether. But the spray was not the leading feature of the antiseptic surgery at all.

15801. It was one of the leading features, was it not?—No, it has often been described as part of the ritual, but that is by ritualists. Surgeons are not ritualists—at least, they ought not to be.

15802. Now, as to the value of experiments on animals in teaching or acquiring technique, is it not the fact that some of our leading surgeons, such as the President of the Royal College of Surgeons, who gave evidence before the Commission, and the President of the Royal College of Surgeons, Ireland, have never themselves operated on animals?—Yes, I believe it is. They gained their experience by learning on man.

15803. Then I am going to quote Sir Frederick Treves. Has he not stated that the experiments on animals that he himself made to improve his technique in abdominal surgery rather led him astray?—No; that statement of Sir Frederick Treves has often been quoted and misquoted, and his final correction of it in the "Times" was to the effect that in one particular case, one particular form of experiment, he learnt nothing from it.

15804. (*Sir William Collins.*) Can you refer to the date in the "Times"?—I cannot.

15805. You can put it in?—Yes, I will get it. He has often been twitted with it by the Anti-vivisectionist party.

15806. (*Dr. Wilson.*) Now as to experiments for teaching students physiology, do you agree with the views expressed by Dr. Waller in his address at the meeting of the British Association, not the British Medical Association?—I was there at Leicester.

15807. That "the province of vivisection, essential as it was, was a very narrow and restricted province indeed, in the domain of physiology," and that vivisection was, in fact, an infinitesimal fraction of practical physiology?—That passage from Dr. Waller's address, taken by itself, may be easily misunderstood. What Dr. Waller meant was that in the work of the physiologist in his laboratory and in his class rooms, the actual amount of vivisection he did was extremely small. And so it is; the total net work done by a physiologist in his ordinary routine rarely includes an experiment on an animal of a nature which might be termed vivisection, compared with the mass of his other work. But it all depends upon the investigator. If you are making a research, for instance, in the nervous system in the way I have just indicated, tracing out the track by the Marchi method, you would make, perhaps, ten vivisections; it would take you ten months to work up that material microscopically.

15808. You maintain, I suppose, that experiments on dogs are necessary for teaching?—Absolutely essential. Not necessarily for teaching alone.

15809. I mean for the physiological class-room, of course?—Do you mean for original research or for teaching? They certainly are necessary for demonstrating certain things.

15810. For teaching physiology?—For the students to use them as material, certainly for some experiments they must use dogs; for others, cats and rabbits will do; for others, frogs and so on; it depends upon the experiment.

15811. Because Professor Gotch stated here that for two years he had not used living dogs for teaching purposes, and Dr. Pembrey admitted before the Commission that during all this year, at least, he had not vivisected any animals at all before his class. From that you would rather criticise the efficiency of the teaching?—Certainly I would.

15812. You yourself have, I know, carried out a large number of experiments on animals in connection, first, with Pasteurism, and also as you say, with respect to brain surgery and the physiology of the thyroid gland?—Yes.

15813. Of course, after what you have said, I need hardly ask you whether you have ever had any reason to doubt the efficacy of the Pasteurian treatment for hydrophobia?—Oh, no, not the slightest—not since the Report of the English Committee.

15814. When they have to propagate the virus at the Pasteur Institute; do they propagate it from rabbit to rabbit?—Yes, from rabbit to rabbit.

15815. Do they use dogs at all now?—I do not know whether they occasionally do. But that is not the animal used for propagation of the virus; the animal used is the rabbit.

15816. From rabbit to rabbit?—From rabbit to rabbit entirely.

15817. So that there does not seem to be any need of introducing a new virus, as it were?—No.

15818. But the symptoms of rabies as seen in the rabbit bear no resemblance to rabies in the dog?—It is a very interesting thing—it all depends upon the rabbit. You may take twenty rabbits, as you know if you have kept rabbits, and one or two will be quite

playful and even bite, and when you inoculate those rabbits, a rabbit which is naturally excitable will bite as viciously as a rabid dog. And so will a guinea-pig. I have had a guinea-pig inoculated with rabies fly at me exactly the same as a rabid dog will. The majority of rodents simply become paralysed in the hind legs, they have the paralysed form of rabies and not the excitable form. But it depends upon the animal.

15819. Then would you go so far as to say that the Pasturian treatment of hydrophobia has been so firmly established on experimental and logical lines that it can never be successfully discredited; is that your strong feeling?—Certainly.

15820. That was my opinion at one time, too, but I have rather doubted it since?—I have not the slightest doubt. I devoted a very long time to working out the accurate statistics at the time of the Government Commission, and considering that the Pasteur system has reduced the death rate from 15 to ·3 per cent it is such a margin of difference that I never can have doubt.

15821. Statistics can be made to prove or disprove almost anything. Of course, you know a great many patients, over 1,200 cases have been enumerated, have died either during or immediately after treatment—I mean within the time stated by Pasteur?—Who collected those statistics? It was Dr. Luteaud. That evidence was certainly not received by anyone who knew the facts. But there is the evidence which the English Committee collected for themselves, you know. They went over to France, and I myself was over there for two months hunting up the cases which had been treated, and hunting up the authenticity of the rabidity of the dogs, and there is no doubt that the death-rate then that Pasteur obtained was about 1.5, if I recollect right. Since then it has been brought down to 0·3, which is a very usual death-rate of a preventive inoculation.

15822. And you believe that there is very little pain inflicted on the rabbit by it?—Yes. I have seen rabbits completely paralysed in their hind legs, and beginning to be paralysed in their fore legs, eating and looking about absolutely unmoved. It is a kind of painless paraplegia in the rabbit evidently. Human beings only suffer, of course, when they get the spasm and the apprehension of dying from an inevitably fatal disease, you know; that is the suffering of hydrophobia.

15823. A few questions with regard to the functions of the thyroid gland and the diseased conditions connected with it. Of course, you yourself have performed many operations on monkeys and dogs?—Yes.

15824. Am I right in stating that an exceedingly large number of experiments have also been performed by others engaged in this research?—Yes.

51825. Am I also right in assuming that though the initial operation, the removal of the gland or part of the gland, may be carried out painlessly, the conditions induced may be often accompanied with very considerable pain?—Oh, dear, no.

15826. When the animal wastes?—No.

15827. There is no pain at all?—No; but the animals run through the same category of symptoms as the myxœdema and cretinism patients; they become more and more stupid and idiotic; they are not sensitive to their surroundings at all.

15828. Can it be said that after all these experiments on the thyroid and parathyroid glands the functions of these glands are perfectly understood now?—Of course not; they will not be understood in our lifetime. It is impossible.

15829. Has it not been pretty fully established by the experiments of Professor Swale-Vincent and Jolly that the functions of these glands appear to differ very widely in different classes of animals?—I showed that myself a good many years ago. When you say that they differ very widely there must be no misunderstanding on this point. They differ in degree but not in kind. It is only a question of degree.

15830. And myxœdema is believed to be due to a diseased condition of these glands, is it not?—Yes, certainly.

15831. Did not Professors Vincent and Jolly find that none of the monkeys from which they removed the glands showed the slightest symptoms of myxœdema?—In Vincent and Jolly's experiments particularly, many of their animals did not show anything at all.

15832. No signs of myxœdema?—No, I am not sure, when I say no signs that they analysed the connective tissues. I do not think they did; but, at any rate, the animals did not present the very obvious signs.

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15833. May I ask whether that has been your experience, too?—Yes. I have had monkeys that did not show anything at all.

15834. Has there not all through these experiments been a great discrepancy in the results obtained by different observers?—No.

15835. And therefore in the inferences to be drawn from them?—No. There have been discrepancies as in every new subject. Every pioneering investigation is sure to arouse other investigations, and new facts come to light which were not explained by the conclusions of the first investigation, and consequently so-called discrepancies have arisen. That is the ordinary concomitant of the results of scientific work, and it only requires our waiting 20, 30, 40, or 50 years to get the real truth.

15836. Then the only outcome of it all, so far as any remedy is concerned, has been the discovery of the value of the thyroid gland or extract in the treatment of myxœdema and cretinism?—Yes, if you apply the term "only" to it. I should have thought it was a good deal for the sake of humanity to get rid of the symptoms of myxœdema and cretinism, if you have ever seen them.

15837. But are cases of myxœdema and cretinism ever completely cured?—Oh, certainly.

15838. Completely cured?—Certainly, and that is not the end of it. For instance, to cure cancer of the thyroid gland by an operation it is absolutely essential to take out the whole gland. If you take out the whole gland you would make the patient myxœdematous, but if you give them the thyroid they do not become myxœdematous. In other words, the thyroidal treatment under those circumstances has enabled you to remove the cancer, which was a fatal disease.

15939. Then, gain, with respect to demonstrations that sort?—Yes, I have grafted in a case of that sort successfully.

15840. After cancer?—Yes.

15841. After the removal of the cancer?—No, after the total removal of the gland for parenchymatus goitre. I have dealt with cancer cases in the way I suggest by giving thyroid, I have not grafted in cancer cases.

15842. Then in regard to brain surgery, have these numerous experiments on animals assisted largely in that respect?—The successful operative treatment of the brain and spinal cord is wholly due to experiments on animals. As I explained, the technique is simply Listerism, but the application of the technique to a particular spot in the nervous system is entirely the result of physiological research.

15843. But the results of an operation, for example, on a tumour of the brain or abscess of the brain are not very satisfactory still?—There is a great deal to be done in the way of improving them of course.

15844. That is to say the mortality is still very high?—No, but the surgical treatment is imperfect often because the cases come too late. Diagnosis is very imperfect still, and cases are referred for operation which ought to have been operated upon a year before, and that sort of thing.

15845. Did not Professor Oppenheim, of Berlin, state at the meeting of the Medical Congress at Moscow a few years ago that "brain localisation is often most doubtful; the diagnosis of brain abscess from brain tumour is often, if not always, quite impossible?"—Yes he may have made that statement ten years ago. He has recently published with Professor Borchhardt a certain series of cerebral tumours, in which he diagnosed them successfully, and Professor Brochhardt removed them successfully by surgery. I should think Oppenheim would not repeat now what he said at Moscow ten years ago.

15846. You are clearly of opinion, of course, that experiments on animals must still be carried on with regard to assisting in this field of surgery also?—Yes, very much, and assisting also, you understand, in the study of the anatomy of the nervous system, because the Marchi method is revolutionising our method of studying the nervous system and anatomy. We are, of

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course, not only employing experiments on animals for localisation in that way.

15847. Apart from cases of injury, does not the diagnosis of any brain mischief, such as tumour or abscess, rest mainly on the physician?—Yes.

15848. And when a surgeon is called in, such as yourself, of course you yourself diagnose and inquire into the case before deciding on an operation?—Yes.

15849. And share the responsibility in that way?—Yes, most essentially.

15850. Now just a question or two with regard to those experiments of Dr. Crile, and I must confess that they have always been rather terrible reading to me?—When you use the word "terrible," I think that any mutilation of a human being or an animal is conceivably terrible in one sense, but it seems to me unreasonable to apply the term to an operation on an animal, and to deny the term to an operation on a human being. If it is terrible to one, it is terrible to the other.

15851. But you see in the one case the operation is necessary?—I say it is necessary on the animal.

15852. You say it is necessary on the animal to enable you to operate on a human being. That is the position you take up?—Yes, quite, it is necessary in both cases.

15853. Then as some of those experiments were carried out in your laboratory, of course you would be perfectly prepared to carry them out yourself, if necessary?—Certainly.

15854. And, as you have said, you believed they were all painless?—Yes, absolutely.

15855. No matter how ambiguous the language Dr. Crile used?—Quite so, no matter what his language.

15856. And you are of opinion that more experiments even of this kind are still required?—Undoubtedly.

15857. But granting, for example, that a dog is endowed with the same relative amount of vital resistance as a healthy human being, is it logical to infer that a diseased man on whom an operation has to be performed with his vitality lowered often almost to breaking point by suffering, could survive the same degree of surgical shock which is inflicted on a dog which is not diseased at all? I mean that the two subjects on which you operate are in an entirely different condition. I am supposing a diseased human being and a healthy dog?—I do not quite follow you, I am afraid. They are not in an entirely different condition at all, because, for instance, taking the diseased human being, his heart is working along at the same rate as usual. His blood pressure is, as a rule, up to the average, and so forth.

15858. There is not the same amount of vital resistance to death as in the case of a healthy animal?—No, possibly not.

15859. Would not that rather tend to what is called somewhat daring surgery sometimes, just because one sees an animal survive a terrible experiment, would you expect that a human patient may survive the same amount of shock under an operation?—Certainly. Because there is not any operation that I can think of at the present moment that has been performed experimentally on an animal, such as the removal of the kidney, or the removal of a portion of the liver, which has not been equally performed on a human being. In other words, the parallelism between such operations on animals and man is absolutely close. Even through every stage of the operation, and the effects produced, the parallelism is absolutely close.

15860. But as regards shock, you do not know that unknown quantity of vital resistance in the patient and in the dog?—But, pardon me, that does not come in, because you start with a patient with certain vital resistance—a man, for instance—and supposing that during the operation you discovered that his blood pressure was falling, you recognise that, and you treat it.

15861. Is it not the practice to hold an inquest in all cases of death in hospital from anæsthesia?—Yes, it is in England; it is not in Scotland.

15862. They do not hold inquests at all there?—No, they do not hold any public inquiry. They do in England.

15863. Is it not sometimes very difficult to determine whether a patient dying on the operating table dies from the mere shock of the operation or from the anæsthetic?—That, of course, is purely a medical point. Personally, I have never seen anybody die from shock on the operating table. I have seen people die, as I considered, from an overdose of chloroform. That is my personal opinion. If you ask me whether it would be possible to differentiate between the two, that is a very difficult question.

15864. So that, of course, you contend that continuous experimentation is still necessary?—It is bound to go on. It will go on as surely as we go on round the sun.

15865. And also, in respect to drugs and doses?—Still more. There I think we have neglected pharmacology disgracefully. There is hardly a university that has got a pharmacological laboratory. They are beginning to found them now.

15866. Just to take one drug, as an example, about which you have spoken and written very strongly—alcohol—have there not been many experiments performed upon animals, to say nothing of experiments that man has performed upon himself?—Yes, a great many.

15867. And still there is no consensus of opinion in the medical profession about the dietetic or pharmacological value of alcohol?—These are two different points, of course. First, as regard the dietetic value, I should have said that there was a very considerable consensus of opinion in the medical profession that alcohol was not a food—I mean in the proper sense of the word, a food without any adverse effects—I should have said that that was an absolutely accepted position. Then, again, as regards the position of alcohol as a drug, as I have shown, the general consensus of opinion of the medical profession now is, not to use it as a drug, but to use other drugs instead, because they have a less injurious effect, like strychnine, and so forth, and heart tonics, and one thing and another, which can be employed with better advantage to the patient than alcohol. So that the general consensus of opinion of the medical profession at the present time is rather to give up using alcohol as a drug, because they have got other and better means at their disposal.

15868. (*Mr. Tomkinson.*) I think you began by stating that all pursuit of knowledge was moral, and that any obstruction raised to the acquisition of that knowledge was immoral?—I feel that.

15869. Do you carry the former to its logical conclusion, that, irrespective of all results, knowledge must be pressed forward. To give an illustration of what I mean, supposing there were no such things as anæsthetics, do you hold that we should be justified in experimenting in surgical and other operations on the animal world for discovery?—Yes. In old days, before anæsthetics were discovered, I think the old surgeons were quite justified in making the experiments that they did, and they made a great many useful experiments to discover new operations and new facts, in which I think they were quite satisfied.

15870. You think that we are justified in exploiting the animal creation in that respect?—Yes, provided that we treat them as humanely as we treat ourselves.

15871. You mean that, because in old days it was necessary to perform amputations and other terrible operations without anæsthetics, it was, therefore, justifiable to perform the same operations on animals for the pursuit of knowledge?—Yes, in those days, quite.

15872. You said that you considered that anti-vivisectors who accepted and availed themselves of the results of these experiments and of the knowledge gained thereby were themselves *participes criminis*, and at least illogical—in fact, were guilty of complicity with the very thing which they condemn?—I think so.

15873. But I suppose you would admit that probably in the majority of cases the patient is entirely ignorant of how the treatment has been obtained?—Yes.

15874. And moreover the assumption is from your point of view that the same discovery could not have been made otherwise than by experiments on animals?—Certainly.

15875. But I suppose you are aware that it is very much contested in some quarters whether the same results could not have been arrived at, and the same

discoveries made by other methods than by experiments on animals?—I know that that has several times been asserted, but the fact remains that the thing was discovered by experiments on an animal, and it is no good saying afterwards: "I could have discovered it in some other way." For instance, it was once said of the circulation by Mr. Tait that anyone with a syringe and a dead body could have discovered the circulation of the blood. A statement like that, of course, is monstrous in itself, but I rather think that is the sort of thing that you refer to. I say that is arguing backwards. We could always get up and say: "Oh, yes, I might have discovered it in some other way." The answer to that is perfectly simple: "Why did you not?"

15876. I do not know that I rightly understood you on the subject of rabies in England. You first of all said that it was an infectious disease?—Yes.

15877. Infectious only in the sense that it is communicable, as we know, by a bite or some contact?—Yes.

15878. But not otherwise than by contact and a puncture?—There is a very interesting thing about rabies. At the Brown Institute we used to have in old days when I was there, rabid dogs brought to us with the saliva dropping out of their mouths on to our pavement and the floor of the place. That saliva, of course, was virulent; but when the saliva of rabies has once dried the virulence is gone. The organism of rabies will not survive drying.

15879. It is innocuous?—It is innocuous. In other words, the only way in which the disease is kept going is by its being absolutely introduced into the living tissues of some other animal.

15880. Then how do you say that the practical extinction of rabies in England, which we now enjoy, was brought about?—By the muzzle entirely, because you can prevent, and it is the only way you can prevent, the one animal introducing the virus into the living tissues of another by preventing its biting.

15881. How does that bear on the subject of vivisection and research through experiments?—I quoted that as an example of the immorality of the anti-vivisectors. I quoted that against the anti-vivisectors because the anti-vivisectors strongly opposed the muzzle, They strongly opposed the extirpation of rabies by the muzzle. I say that was immoral.

15882. I do not know about that, but there was a very strong feeling that the muzzle was not effective, and I may tell you a very strange thing about it. In my county the muzzling order was put in force by the county authorities for a considerable time, and all dogs were muzzled, but after a time it was removed by several boroughs, and after a time our chief constable, who had to report to us every quarter so many dogs seized, suspected or condemned for rabies and destroyed for rabies, advised us (in view both of the fact that the Order was illogical, because there were dogs in the boroughs contained in the county without it, and also expressing the opinion that muzzling had nothing to do with the suppression of it) to repeal the muzzling Order and to go back to the simple Collar Order; and from that very day almost rabies has ceased in the county?—I have no doubt.

15883. And he explained it by saying that hundreds of these dogs he believed were given a bad name, were chased, driven into a great state of terror, and then were destroyed and returned as attacked by rabies, and he thought that it had more to do with hysterical and imaginary rabies than suppressing it?—Fortunately imaginary rabies does not exist, and hysterical rabies was amply met by the collar Order, but the diagnoses must have been remarkable. The Board of Agriculture sent all carcases and heads of dogs to the Brown Institution, until they had their own laboratory, and all the cases were tested experimentally. But as regards the condition of things in Cheshire, I think you forget that Colonel Moorson, the head of the Lancashire Police, was very strongly of exactly the opposite opinion to that of your head constable—that the Lancashire records proved absolutely that though all this isolating and muzzling was done in the counties just as they chose or not, the Lancashire records were very good, and the muzzle had a very marked effect. And so it was in London here.

15884. We may have been just approaching the end of it, but it was perfectly remarkable for years and years we have had a clean sheet now?—May I also point out that in 1889, when that meeting at the Mansion House took place, we arranged with Sir Henry Roscoe that he should introduce a Bill into the House for the universal collaring of dogs, which is the very thing you referred to.

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15885. That was for identification?—Yes, but that was not agreed to.

15886. You repudiate very strongly the charge, or the impression, that the witnessing of these operations on living animals brought about familiarity and, therefore, to some extent a contempt for pain on the part of students or the operator. You repudiate that idea altogether?—Absolutely, because the animals are not suffering any pain; there is no question of pain. On the contrary, it teaches the students how to prevent pain.

15887. I suppose you do not hold that in cases where, as abroad, operations are performed without anæsthetics?—All that idea of experiments abroad, if I may take up that point, dates from the barbarities performed at Alfort thirty years ago in the Veterinary School, but all that is ancient history. In the experiments that one sees abroad now the animals are anæsthetised as they are in our laboratories. Some investigators may not use anæsthetics, but as a rule they all use them abroad.

15888. You spoke of experiments in anæsthetics on animals being necessary before their administration to man?—Yes, I think that students ought to be taught to anæsthetise dogs.

15889. I do not think that even anti-vivisectors would object to that. I do not think that mere experiments in the administration of anæsthetics are looked upon as painful or cruel operations?—Some evidence has been offered to the Commission here—I see it was by Sir Thornley Stoker again—that monkeys when brought into the room exhibited fear and alarm, and so on, and that the giving of anæsthetics to a dog was a very painful proceeding, and Dr. Buxton repudiated it.

15890. I am not here to speak as to that, but that is not an operation?—No, certainly not.

15891. In most cases it is the administration of a drug?—Yes.

15892. Which is quite different from cutting operations?—Yes.

15893. Now, I must ask you a few questions about these operations of Dr. Crile's. They were performed in your laboratory in the College?—The first sixteen were.

15894. Not in your private house?—No, at the College. The rest he performed in America, at Cleveland.

15895. Sixteen healthy dogs were taken and made the subjects of what to the ordinary lay mind were terrible operations. I suppose that you would admit that to an ordinary person like myself they do convey a very terrible impression?—If there was any idea that the animal suffered at all it would be terrible, but surely the use of the word "terrible" is not justifiable in the case of an anæsthetised animal. I am only suggesting that.

15896. On the point of pain and suffering alone, quite apart from the justification of it, would you be quite happy in subjecting a favourite dog of your own to any such operation?—Most certainly. I have got a favourite dog of my own, and if an anæsthetic was given to it I would not care what was done to it—not a bit.

15897. Then you are absolutely satisfied in your mind that complete freedom from any kind of pain was enjoyed if one can use the word, by the animals under it?—Certainly, exactly as I am certain that the patients whom I operate upon in the hospital and in my practice are free from pain.

15898. (*Sir Mackenzie Chalmers.*) Have you ever had anæsthetics yourself?—Yes. I have been operated upon often.

15899. (*Mr. Tomkinson.*) Is it quite clear that human beings do not suffer. Is it not possible when there are those reflex motions that you spoke of that pain is felt, but that there is oblivion in the brain which creates forgetfulness of it afterwards. I ask the question because I have witnessed the case of a person who had a broken leg, and whose muscles round the ankle had stiffened during the process of healing.

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After a time the patient determined to have those fixtures broken down. I saw the operation, and the patient not only struggled, but moaned so as to give me a very painful impression, and I could hardly believe it, although I admit that afterwards he said he felt nothing?—I once removed an infra-orbital nerve, which is an extremely sensitive nerve, from the face of a woman who had had enormous quantities of morphia and drugs for neuralgia before. During the whole of that operation that woman commented on every remark that I made to the house surgeon, and when I tore the nerve out she burst out laughing, yet we went on with the operation, because she did not make the slightest purposive movement to avoid it. She obviously was not feeling it. We finished the operation, and the next day she knew absolutely nothing of what had occurred; she never felt any pain whatever, and she would not believe our statement of what she had done. I saw another case operated on by a colleague of mine, and the patient read my colleague a homily on the future life the whole time. He knew nothing of it. It was a large plastic operation for a burn in the neck—a most sensitive thing—in fact, it was the sensitiveness probably that stimulated the brain. But neither of those patients suffered anything at all. I am quite satisfied on that point. One's own knowledge of anæsthetics is that you go off suddenly and that as a rule you come to gradually, but you have no consciousness of pain during the time the operation is going on.

15900. Is the brain rendered insensible of receiving sensation?—No, the brain can receive sensation. It is only a question of paralysing the higher centres—the highest centres of all. You must only paralyse them. That is the object of all this modern research on anæsthesia. We want to know how just to abolish consciousness, and at the same time to enable all the lower processes to go on unhindered. When we have solved that question we have solved the whole thing of safety to life.

15901. All these experiments of Dr. Crile's were for the same object—to test the amount of shock caused by different operations, and the effect of it?—Yes, the effect of it.

15902. On the circulation of the blood?—Yes.

15903. And that was done sixteen times over?—Yes.

15904. Sixteen healthy, beautiful dogs—fox terriers—were taken. To use an ordinary vulgar expression, do you think the game was worth the candle?—Yes; I am perfectly sure of it.

15905. You admit, do you not, that those operations, published as they have been in that form, have created a tremendous sensation, and probably one of the strongest sources of opposition to the system and attack upon it?—Yes, because of the gross misrepresentations of those operations.

15906. But is not the description of them by the operator?—Yes, certainly; but not the description that has been given of them to the public. On the contrary, several times they have been made the subject of pamphlets which I have been advised legally I could take no action upon, because in an ingenious way the fact of anæsthetics having been given was not introduced; but the whole effect of the pamphlet was to convey to the ordinary man in the street that those operations were done without anæsthesia.

15907. Do you think so?—But the whole strength of the anti-vivisection party is the capacity for writing a thing and giving it a false impression altogether by means of innuendo. That is very glaringly shown in all this evidence which has been laid before the Commission.

15908. I cannot say that they have ever been described to me as having been without anæsthetics, but accompanied with an expression of opinion that complete anæsthesia was hardly possible through such tremendous operations, and was not likely to have been carefully supervised when the animals were kept alive the whole time?—Exactly. And on what authority has that last statement been made, that anæsthesia cannot be kept up under such tremendous operations? Why, operations that we do on human beings are far more tremendous by way of nerve stimulation.

15909. I think there has been more than one witness here who has expressed great doubt whether a dog is easily kept just on the border line?—That is exactly the point that I referred to before. I felt sure that evidence of that kind had been given, and I say that that is simply a matter of knowledge of giving anæsthetics. It is sheer ignorance to say that a dog cannot perfectly well be kept under chloroform, or any other dangerous anæsthetic that you like to mention.

15910. Is that system of introducing chloroform, or whatever the mixture may be, from another room, pumped through a tube, and brought under the mask, I suppose, perfectly satisfactory to you?—Yes, quite.

15911. Quite as much so as in the case of a patient having the chloroform held to his mouth?—Quite; in fact, probably more so. In administering the atmosphere you must give the atmosphere to the individual loaded with a certain amount of the anæsthetic. What we have found now is that during the operation it is quite sufficient to have only 0.5 per cent. of chloroform, an incredibly small amount, which is quite indistinguishable by smell in the room. If you blew that to the patient it would probably arrive at him more regularly than if he drew it in himself.

15912. Because it is properly mixed?—Yes. The only thing is that it involves so much apparatus that you cannot do it in practical working.

15913. Have you had much experience or seen much of the administration of curare?—No. I have used it myself, but I have not used it much, because my lines of research have not been very much on blood-pressure experiments.

15914. Do you approve of it?—It is most useful in blood-pressure experiments.

15915. Do you approve of it?—Of course I do.

15916. Do you not think it may be dangerous, as paralysing all power of motion without rendering the animal insensible?—You mean if it was used alone.

15917. No, I mean used along with anæsthetics?—Oh, no.

15918. Thereby rendering it possible for sensation to be felt, and yet without power on the part of the animal to evince it?—No, I do not think so at all, because, in the first place, taking curare alone, I believe that when we have given curare up to the point of producing paralysis of all the muscles of the body, then the animal or person would be insensible to pain as well. Although all drugs have a more or less selective action upon the different functions of the body, no drug has such a selective action as to be a paralysant of motion only, and the statements about curare which have been made are exceedingly vague, because, fortunately, it has been so very rare that any patient has been poisoned by the poison to the degree of paralysis of the respiratory muscles. The only good case on record is Mr. White's, and in that case the person did not know that the wound was being cut out, and that is exactly what I should have suspected, namely, that when curare by itself is given in such enormous doses as it is in experiments, it acts as a narcotic as well as an ordinary paralysant of voluntary muscles. The evidence so far takes us that way. Of course it is never given alon it is not allowed by the Act; it is not an anæsthetic under the Act. Consequently it is always given in association with narcotics like morphia, for instance. Then, again, I have read the evidence offered to the Commission as to whether morphia is an anæthetic.

15919. (*Dr. Gaskell.*) Can you tell us where White's case is published?—In the British Medical Journal. I do not know if I have the date here.

15920. Could you quickly tell us just something about that case?—Yes. It was a girl who was dusting a trophy of Indian arrows in the hall of her employer. One of these arrows fell and stuck into her arm, and then fell on to the ground. She took no notice of it, naturally. But in about a few minutes, suddenly, she felt ill, began to lose power, and by the time they called a medical man to her she was already unconscious, passing into the stage of asphyxia after paralysis of the respiratory muscles. The medical men recognised immediately what had happened, and started artificial respiration, while they sent for Mr. White, who came and immediately cut out the wound with the brown poison that came off the arrow still adhering to the flesh. They went on with artificial respiration, and in about another hour or an hour and a-half the girl began to breathe spontaneously, and then gradually recovered, and when she recovered consciousness she knew nothing of what had been done, and could not understand why she had the dressing on her arm.

15921. (*Chairman.*) Might it have been from shock?—No.

15922. Or might it have been from some paralysing of the sensory nerves?—It must have been from some paralysing of the nervous system, certainly.

15923. (*Mr. Ram.*) Did White cut out the poison and the surrounding wound without any anæsthetics?—Yes, because the girl was unconscious.

15924. (*Dr. Gaskell.*) At the time the wound was cut out do you think there was a sufficient condition of asphyxia to cause unconsciousness?—No, because they had already been doing artificial respiration for some time, and she could not then have been in an asphyxial state.

15925. What anæsthetics have you had in your own case?—Ether, chloroform, and laughing gas.

15926. Can you tell the Commission whether you have experienced any sensation at all during an operation?—None at all. And on the question of morphia (Mr. Tomkinson, I see, has been obliged to leave), I was about to draw attention to the fact that the question whether morphia is an anæsthetic has been raised here; but, of course, it all depends upon the dose. Morphia given in the dose that is given in the laboratory is an anæsthetic of a most pronounced kind. I could establish that if it was required by an observation on a human being, because a very few years ago, at University College Hospital, a patient of mine, a boy, was given an overdose of morphia by the nurse by mistake. That boy had to be roused, artificial respiration employed, and stimulated in various ways to keep him going and painful impressions made upon him, many of which he resented, but he knew absolutely nothing about it; he was absolutely unconscious to pain, absolutely unconscious of everything for about 12 hours. And in the laboratory the dogs or animals that are given morphia are generally thrown into the same state, that is to say, a poisonous dose is given.

15927. Can you say whether urethane is an anæsthetic?—I cannot say. I have no personal experience of urethane. I have no doubt that it is; it is only a question of dose.

15928. Do you consider that there are idiosyncrasies in the action of morphia. Some people are said to be very receptive of morphia, and it produces excitement; in others you get a much more narcotic effect?—There may be slight differences, but I have never seen a person who did not succumb to morphia, or an animal either.

15929. How would you distinguish whether a man or an animal, when under an anæsthetic, is feeling pain?—That is a very practical point, of course. In the first place, the movements that patients ordinarily make when they are under an operation are simple reflex movements. We endeavour to preserve all those so far as they do not bother the operator. Then anything like a purposive movement, anything having at all a purposive character, I regard as a signal that the return to consciousness is not far off.

15930. Do you not consider that in a brainless frog, for instance, you can get reflex action and purposive movement?—Yes, undoubtedly you can. But you asked me what was my criterion in operations on man. That is my criterion; of course, it is always on the safe side. I mean that my criterion is a safe one. I know perfectly well that persons, however much they may have moved, will not have felt pain, because I have asked them afterwards again and again, especially in this more modern way of leaving off chloroform. With the Vernon-Harcourt apparatus, one simply turns the tap and they breathe air for ten minutes and so on. I have asked them again and again afterwards whether they knew anything of the operation, and they say no.

15931. For instance, would you call a cry a purposive movement?—No.

15932. Can you distinguish between a cry that is a reflex act and a cry that is a cry of pain?—I think I could perfectly well.

15933. In an animal as well as in man?—Yes, most certainly. The groans of a patient during an operation, which one may often hear outside an operating theatre, are distinctly the laryngeal movements accompanying respiration. It is most definite and quite different from anything indicating a return to consciousness.

15934. Have you read through "The Shambles of Science?—Yes.

15935. I suppose you could hardly tell from the statements made there by Miss Lind-af-Hageby, with respect to movements and cries, whether they were reflex or painful?—One could hardly tell also whether they occurred; but, assuming that they occurred, I could not say what they really were.

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15936. With respect to the experiments which you suggest that students should be allowed to perform for the purpose of obtaining manual skill, there are two points, it seems to me. One is manual skill in the operation itself, and the other is seeing the success of the operation afterwards?—I quite agree.

15937. Would you think that the first alone would be of any use if this Commission considered that the animal must be put under anæsthetics and killed before it recovered from the anæsthetic? Would you still recommend that the students should be allowed to experiment on animals for the purpose of acquiring manual skill?—Yes, I think that half a loaf is better than no bread.

15938. But you would prefer the whole system as carried out at the Johns Hopkins Hospital, Baltimore?—Absolutely. May I say on this question of manual skill that one can only speak from personal experience in a matter like that, and I am quite sure that the delicacy of control of dissection, which is necessary in operating on a small animal like a cat or a rabbit, is an invaluable training for the coarser movements which you perform on man.

15939. Then, again, with respect to demonstrations to students, you suggest that demonstrations to show the activities of the cortex of the brain are essential. What animals would you employ for that?—The monkey, but you need not employ a monkey. You can show it all on a cat or a dog.

15940. A cat or dog would suffice?—Yes.

15941. But not any other animal?—No. A rabbit is a very bad animal for localisation experiments.

15942. That would be yet another reason for recommending that dogs should still be under the Act?—Yes.

15943. And in your opinion, rather a strong reason?—A very strong reason. Dogs are essential in so many ways to experimentation.

15944. Monkeys, of course, are not always to be had?—No.

15945. Then with respect to Certificate B, Mr. Stephen Coleridge's objection was to the words "initial operation." Do you think there is the slightest necessity, if this Act was to remain much as it is at present, to alter that certificate? Do you think there is any ambiguity in that term, "initial operation"?—There is not the slightest ambiguity in the certificate as it is worded at present for the purpose for which it is employed; but I should like to see all certificates abolished.

15946. I know; but, supposing that the certificate was to remain there you do not think it would raise any difficulty?—The difficulty has never arisen.

15947. The difficulty is an imaginary one?—Yes.

15948. (*Sir Mackenzie Chalmers.*) I think you have had read to you the answer to Question 10319?—Yes; and I said before that it was a travesty of Certificate B as it is in operation at the present time.

15949. It would be perfectly impossible, you would say, for any operator, after having done the initial operation, to allow the animal to recover and then stimulate by electrodes the cut nerves?—Absolutely impossible.

15950. (*Dr. Gaskell.*) Then there is one other point with respect to the Act that has not been mentioned. I understand that your suggestion is that under your suggested licence you would remove all certificates altogether. Would you be inclined to restrict in any way the animals under the Act? Would you draw any distinction between one set of animals and another?—No. Are you referring now to the whole animal kingdom?

15951. I am coming to that?—No, certainly not. The answer to your question is no.

15952. Would you be inclined to restrict them in the other direction—that is to say, exempting certain animals from the action of the Act?—Yes, up to the present time, as I have always understood by a mistake in an answer across the floor of the House of Commons, this Act has been made to apply to all invertebrate animals. That, of course, lands you in the most ridiculous position at times. Undoubtedly the Act ought

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only to apply to warm-blooded animals. I think that is the proper separation of the animal kingdom.

15953. Was not that what was originally intended?—I have always understood that it was, and I have understood that in a question and answer across the floor of the House, Dr. Playfair misunderstood it, not being a physiologist; he put it down, and now it stands "vertebrate." It is perfectly ridiculous for fish and so forth to be included.

15954. Do not you think that one alteration which might be adopted in the Act would be to restrict it to warm-blooded animals?—Certainly.

15955. You said that you would have a licence to cover everything and abolish the certificates?—Yes.

15956. I do not think it was quite clear whether the form of that licence was put out by you or not?—No, it was not. What I suggested was that all the facts which were detailed on the certificate application form could be, as indeed they are now, detailed on the application form for a licence.

15957. Have you any form of licence which we could get down on our notes so as to let us know what it is you want?—No, but I have a draft form. I am quite satisfied that it would be enough. What is not enough, in my opinion, to give the Home Secretary complete information, is the final form that we have to furnish at the end of the year.

15958. The present form of licence causes the animal to be killed before it recovers from the anæsthetic?—Yes, the present application for a licence.

15959. That is what the licence is?—Yes, but in a new Act, of course, that could be altered. In a new Act that would come to revising Section 3.

15960. I only wanted to know whether for our own information we could have on the notes your idea of what that form of licence ought to be?—I shall be very glad to furnish it in writing to the Commission.*

15961. (*Chairman.*) On the printed form, amended as you would wish it?—Yes, and also with the alteration necessary in the first part of Section 3 of the Act.

15962. (*Dr. Gaskell.*) There is only one other point which I thought I might put to you with respect to Mr. Lawson Tait. You know this little book by Sir James Thornton, "The Principal Claims on behalf of Vivisection"?—Yes, I know it well, it is full of contradictions of fact.

15963. I want to draw your attention to a statement on page 3: "Lawson Tait says, 'This is just one of the cases where vivisection has led us astray. If the carbolic ligature had never been tried on animals, where it seems to answer admirably, it would never have been tried on human patients, where it fails miserably, and has cost many lives. The fact is that the diseases of animals are so different from those of men, wounds of animals act so differently from those of humanity, that the conclusions of vivisectors are absolutely worthless. They have done far more harm than good in surgery.'" Is there the slightest foundation in fact, in your opinion, for the statement that the wounds in animals act so differently from those of humanity?—No, it is absolutely contrary to fact.

15964. Is that true about the carbolic ligature?—It is not.

15965. Did it fail absolutely?—It did not. We use it at the present day.

* See Appendix.

THIRTY-SIXTH DAY.

Tuesday, 19*th November* 1907.

MEMBERS PRESENT:

The Right Hon. the Viscount Selby (*Chairman*).

Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir J. McFadyean, M.B.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. G. Wilson, M.D., LL.D.
Capt. C. Bigham, C.M.G. (*Secretary*).

Sir Victor Horsley, F.R.S., F.R.C.S., recalled; and further Examined.

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(*Witness.*) May I put in an account which Dr. Gaskell asked me for, namely, the description which Mr. White gave of his case of curare poisoning on a human being? The reference to it is the "British Medical Journal"—I could not remember the date—of March 16th, 1889, and I wish to draw attention to the fact that with regard to the very important matter which Dr. Gaskell raised, namely, whether the patient was under the influence of curare or whether she was under the influence of the state produced by asphyxia, as a matter of fact she was still breathing, she had slow, shallowy, jerky respiration, and feeble pulse. The whole track of the wound was covered with a brown apparently vegetable material. It was therefore freely exercised, and stimulants were freely given by the mouth. So that she not only breathed still, but she swallowed; therefore she certainly was only I should have said under a rather moderate dose of curare. But, she was in a very dangerous state from the condition of the pulse, because curare lowers the blood pressure—you see that experimentally, of course, very markedly—and causes a very feeble pulse and ultimately death. Then, as I stated, they kept up artificial respiration until she recovered. The final point that I want to draw attention is "That Mr. White believed that the arrow had been poisoned with curare and cited"—this is an abstract of his paper—"the experiments upon animals made by Claude Bernard, Waterton, and Sibson, of some of which he had been a personal witness, in support of his view." So that he recognised that it was curare poisoning from the effects of curare poisoning that he had seen in experiments on the lower animals. I had forgotten that last point, and I thought the Commission would be interested to hear it. With regard to the other documents I was asked for, I will put them in after my cross-examination to-day, because, of course, although I have got my own views of the working of the Act, it is quite possible that the Home Office have a great many departmental requirements of which I am quite ignorant, and I should like to hear those before I finally formulate my statement.

15966. (*Chairman.*) Sir Mackenzie Chalmers, who is going now to ask you some questions, may touch upon that, and give you an opportunity of saying what you wish?—Thank you very much.

15967. (*Sir Mackenzie Chalmers.*) I do not think we have it in evidence, but you are a practising surgeon?—Yes.

15968. I think you operate at two different hospitals?—Until quite recently I was surgeon to the University Hospital, as well as the Hospital for Paralysis. I have resigned my University College position, I am now Consulting Surgeon to that hospital.

15969. May I take it that for the last twenty years at any rate you have been operating almost daily?—Yes.

15970. And have been continually in contact with the practical administration of anæsthetics?—Yes.

15971. I want to ask you one or two further questions about what we call the Crile case. In the first place, I want to read to you Dr. Crile's application to do the experiments which he did, and to ask you a question upon it: "25th April, 1895.—To the Right Honourable Henry Matthews.—I beg to enclose a form of application for a licence to perform experiments upon the causation and the prevention of shock in surgical operations. I am the Lecturer on Surgery in the University of Worcester, Cleveland, Ohio. My engagements are naturally pressing, and I would be much obliged if you would grant me the licence asked for within a few days. I am also informed as a matter of fact that the experiments I am about to perform do not come within the Act, inasmuch as they do not involve any pain to the animal.—I have the honour to be, Sir, your obedient servant, George W. Crile."

Then, accompanying that letter was the formal application in which the experiments were described as "investigation of shock in operations performed under ether. The experiments proposed will be conducted under the licence, *i.e.*, the animals will be anæsthetised with ether and killed before recovery from same." What I wanted to ask you was this: The object of these experiments was to examine the effects of surgical shock, not surgical shock pure and simple, but surgical shock under anæsthetics?—Yes.

15972. Would the supposed object of the experiments have been frustrated if anæsthetics had not been part of the procedure?—Certainly, because we wished in this research to imitate, as far as possible, the operations done on a human being.

15973. Perhaps I may also, to complete the case, read a letter from the Association for the Advancement of Medicine by Research. I do not know whether it is within your knowledge or not that after a report on Dr. Crile's application to Dr. Poore the matter was referred to the Association for the Advancement of Medicine by Research?—I did not know that.

15974. Then perhaps, to complete the matter, I might read their letter. It is addressed to the Under Secretary of State, Sir Kenelm Digby: "Sir,—According to your letter of the 29th ult., I have laid before the Council of this Association Dr. Crile's application for leave to hold a licence under the Act 39 and 40 Vic., c. 77. I beg to return Dr. Crile's papers. The Council wish me to say that they hope this application may be granted. Dr. Crile is Professor of Surgery in one of the American Colleges, and is therefore fully qualified to make the proposed observations with judgment and accuracy. At the Pathological Department of University College he would enjoy every aid for the success of his work, and would have Professor Victor Horsley's constant help. The Council are further of opinion that the proposed observations are calculated to give very valuable results. The occurrence of shock after surgical operations, in spite of anæthesia and of all the precautions that can be taken to avoid it, is a danger that cannot be too carefully studied, and the Council therefore hope that leave may be granted to Dr. Crile to make these observations, which would, moreover, be wholly free from pain, as the animals would be killed while yet under the anæsthetic." After that the licence was granted?—Yes.

15975. Did you see any of the operations yourself?—Yes, I saw about the first six or seven.

15976. In your opinion, were the animals that underwent those operations suffering or not?—No, I think they were not suffering at all. I think they were completely anæsthetised, and unconscious of any pain.

15977. Who is Dr. Goodbody?—Dr. Goodbody is the Assistant Professor of Pathological Chemistry at University College, and when I was not present he was present at these operations.

15978. Have you discussed the matter with him at all?—I have not spoken to him since 1902, when he wrote to the "Times" in the correspondence that ensued upon the exposure of the catalogue of instruments and Mr. Coleridge's action.

15979. Do you know what conclusion he came to from the operations he saw as to whether the animals suffered or not?—I remember that he stated that, in his opinion, the animals were quite unconscious to pain.

15980. Is he an experienced and skilled observer?—Yes; he has been a laboratory worker now for 15 years.

15981. Your physiological experiments have been carried on mainly, if not entirely, at University College?—And at the Brown Institution. I was Director of that Institution for several years.

15982. Has the Home Office Inspector always had free access?—Yes; and he is constantly in and out.

15983. Does he pay pretty constant visits to your laboratory?—Professor Thane does. He visits my laboratory almost every week. Of course his rooms are adjoining mine, you know—his Department, I mean. It is quite convenient for him.

15984. Does he give you notice of his visits?—No, never. The visits have always been surprise visits, It is rather difficult to fix the date; Mr. Erichsen used to give me notice at the Brown Institution; that was before 1890. But I should say that for the last 17 years, about, they have been surprise visits invariably.

15985. Have you not carried on to some extent Professor Ferrier's experiments on brain surgery?—Yes.

15986. These experiments, I suppose, involve the destruction of some portion of the brain?—They are of two kinds. In one set of experiments the animal is experimented on under the licence, and it is killed before recovery from the anæsthetic. Those are stimulation experiments, excitation of the surface of the brain by electric current. And then in order to control the results of excitation you must perform further experiments under Certificates B and E E, in which you destroy, as you say, part of the surface of the brain, and examine the symptoms afterwards.

15987. And then the animal is eventually destroyed?—Yes.

15988. In those operations, on recovery from the anæsthetic, does the animal suffer much?—No, it suffers remarkably little. It is quite a common thing to see a monkey, for instance, from which you have removed a portion of the brain take a banana or grape as soon as it really recovers from the operation. I do not consider that operations on the brain are really painful operations in that sense.

15989. (*Chairman.*) Are you speaking of animals?—I am speaking now of animals.

15990. (*Sir Mackenzie Chalmers.*) You do not think that the animals which are allowed to recover endure acute suffering?—No, I do not.

15991. Is the brain not very sensitive then?—The higher brain itself it wholly insensitive, so far as I know. There are portions at the base of the brain where sensory paths are coming in, which I am sure would be sensitive.

15992. How do you account then, for instance, for the intense pain of headache?—One sees a great deal of that in cerebral tumours; that is due to pressure. The skull is a rigid box, and the fluid within it is at a certain pressure; it requires therefore an exceedingly small increase of tension, a little more fluid injected into the skull to raise that pressure, and the moment that occurs then you have a sensitive membrane, the dura mater which covers the brain, pressed.

15993. It is the compression of the dura mater, is it?—That is a very difficult thing to prove, of course, scientifically, but it certainly seems to be so. The dura mater is supplied by the fifth nerve, the most sensitive nerve of the whole nervous system, and from the fact that the headache varies in cerebral tumour directly with the tension, it seems the most reasonable explanation of headache.

15994. (*Chairman.*) But are the nerves which convey, the sensation of pain nerves of the dura mater, whatever the cause is?—Yes, the dura mater is very richly supplied with nerves.

15995. (*Sir Mackenzie Chalmers.*) But you have to penetrate the dura mater to operate?—Yes, you simply incise it, and turn it aside.

15996. But that does not create pressure and cause intense headache?—The opening of the skull opens this rigid box, and from that moment you have destroyed all the intra-cranial tension.

15997. In the case of ordinary headache you think there is interior pressure?—Yes, interior congestion and pressure.

15998. In consequence of these brain experiments on animals have you been enabled to perform any operations on human beings which you would not have dared to have performed otherwise?—Certainly, both upon the large brain, the cerebrum, and upon the cerebellum the small brain, and upon the spinal cord.

15999. And what of the results?—The results are excellent where the cases have been diagnosed sufficiently early.

16000. With correctness?—With correctness, and sufficiently early. On that point I should like to draw the attention of the Commission to a fact which seems to me always an exemplification of Professor Lankester's statement that scientific investigation does not add to our knowledge by arithmetical progression, but by geometrical progression. Of coure the first operations upon cerebral tumour, now some 20 years ago, revealed to us the fact that this opening of the skull which destroys the intra-cranial tension was

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the mode *par excellence* by which the inflammation of the optic nerves which accompany so many diseases inside the skull could be arrested and the sight preserved, because there are not a few cases in which the patients can be recovered from a cerebral tumour, but remain blind all their lives, and this observation, which was simply made, you might almost say by accident, has been of the utmost benefit to humanity, because now, even in cases where the tumour can probably not be removed, what we call a decompression operation is done to save the sight.

16001. (*Chairman.*) Are you speaking of observations that you made upon a patient?—Yes.

16002. Not an experimental observation?—Not at all. I ought to amplify that, perhaps, by saying that attempts had previously been made to save the sight by opening the sheath of the optic nerve in the orbit, the operation of de Wecker, a great Parisian ophthalmologist; but his operation was not successful. We now know the reason—that the pressure ought to have been relieved inside the skull, not outside.

16003. (*Sir Mackenzie Chalmers.*) Have you discussed with any of your patients afterwards the effects as to whether there was pain consequent upon the operation?—Yes, often. Like all cutting operations, they have a certain amount of pain in the cut itself—a sore pain, which, like ordinary aseptic wounds, disappears in about 48 hours. A certain number of them suffer from headache due to the anæsthetic and to shock.

16004. The anæsthetic produces headache for some time afterwards?—Yes, very often. Not invariably, of course. Many people can have chloroform for an hour and have no headache.

16005. It is almost an irrelevant question, but one witness whom we have had before us objected to physiology on the ground that no advances had been made, and one special point that was taken by that witness was that, although we had been experimenting for a hundred years, nothing has been learned about any of the ductless glands. Is that correct?—No, it is absolutely untrue.

16006. Your own work on the thyroid gland is one example to the contrary?—Yes, it is only one example of a great many researches now on the ductless glands.

16007. Which are adding to our knowledge and to our power in dealing with disease?—Yes, it has all arisen since I was a student. It is an entirely new chapter in physiology.

16008. And a fruitful one?—Immensely, because you see it introduces us to the true chemistry of the living body, bio-chemistry. It is only, of course, by advance in research in bio-chemistry that we can really put internal medicine on a proper scientific basis. The University of Liverpool, of course, which is very advanced in medical matters, has founded a professorship of bio-chemistry.

16009. One other small point. You showed us a catalogue in which certain of the pictures, I must say, look very disagreeable. May I ask, as a matter of fact, are animals which have to be operated upon tied down for the purpose of preventing struggling, or are they tied down simply to keep them in position under the anæsthetic?—They are tied down simply to keep them in position.

16010. Not in any way to mask any purposive movement?—Not at all. In fact, you cannot mask a purposive movement. There was a statement made by Mr. Coleridge in his evidence about an experiment by Professor Starling about fastening down an animal on a board in connection with the Bayliss trial. It is at the top of the second column, page 205, at Question 11569. Mr. Coleridge is asked by Dr. Gaskell, "I want to know whether you do not consider, when a delicate registering apparatus is going on in the animal, which moves perfectly smoothly and gets real good curves, even although the operator says the animal is under slight anæsthesia, that does not show that the animal was not feeling pain?—Not necessarily. I have an answer to that, of Sir Victor Horsley, in the case against me on that very point that an animal can be held tight when it does not move." I suppose he means can be held tight so that it does not move. He is quoting from evidence which was given in Court, but the context of that evidence ought to be given.

16011. Was it your evidence he was quoting from?—It is my evidence he professes to be quoting from, but from my experience of Mr. Coleridge, and especially after what he has admitted to this Commission, I accept nothing that he produces in the way of evidence of what I have said unless I have the context. I will just read it, however, and then explain it to the Commission. "Mr. Rufus Isaacs: Would it be possible to perform the experiment or the demonstration which we know took place if the dog had been conscious? Sir Victor Horsley: No, absolutely impossible, unless the dog had been fixed up with all manner of apparatus, to absolutely fix every bone." I showed the Lord Chief Justice what I meant by fixing every bone with a rod passing through the bones. Of course, it is an absurd position, you observe. It is impossible to fix every bone in an animal. And the Lord Chief Justice asked me whether it could be fixed on the board—they had the board on the witness desk—and I said, "You can fix an animal with apparatus of *that* sort (pointing to the operating board). That is the answer to that—you can so fix an animal on that board, and he pointed to it; they had it in Court." The rest of that is true, but the inference drawn from it is wholly untrue. My point was that it was conceivably possible with an apparatus to fix every bone, but as you must fix every bone to make the animal absolutely immobile, so that it could not move at all, the thing was practically impossible. Mr. Coleridge's was quite a false rendering of my evidence in the way he presented to the Commission.

16012. On that we have been told by some witnesses that there is a sudden transition from pain to absolute anæsthesia. Is that so, or is there a gradual diminution of pain as the anæsthetic is administered?—There is a gradual diminution of sensitiveness to pain undoubtedly. The loss of consciousness is apparently sudden to the individual, but that is quite a different thing.

16013. Pain gradually diminishes as the anæsthetic is applied and pressed?—Yes, exactly like the disappearance of all other kinds of sensation.

16014. So that, assuming for a moment that an animal is incompletely anæsthetised, using it in the English sense of incomplete anæsthesia, there would be diminished pain, although there would be pain?—Yes, if you mean by incompletely anæsthetised, that it was still conscious of pain, it would have less pain in proportion to the amount of anæsthetic which it had received.

16015. I suppose, in certain cases, for instance, in midwifery cases, anæsthetics are given with the view of diminishing pain and not with a view of producing complete anæsthesia?—You want to avoid preventing the contraction of the uterus. If you give a very large, profound dose of chloroform, you would interfere with the normal contractions of the uterus and the expulsion of the fœtus.

16016. I am not quite sure whether there is any point we can discuss further as to certificates, but taking the Act as it stands, the licence and the certificates are framed, not with reference to the scientific quality of the operation, but with reference to the pain, are they not?—Yes, certainly.

16017. Is it not convenient, first of all, to have a licence under which an animal can be anæsthetised and killed without recovery; is it not convenient to have that by itself?—I cannot see that it is convenient except as a means of informing the Home Secretary what experiments are done, and the animal is killed before recovery from the anæsthetic.

16018. Is it not convenient in this way, for the purpose, of course, of assuring the Home Secretary and the public, that under licence alone a man should be only able to operate on an anæsthetised animal which is never allowed to recover, so that if anything further is wanted, a special certificate should be given?—I am sorry that I cannot see the convenience of the proposal, because my proposal embodies exactly the same facts, and it strikes me is simpler. I propose that on the application for a licence the applicant should state the different classes of experiments he is going to do. If he were only going to do experiments on animals killed before they recovered from the anæsthetic, he would simply state so, and there is an end of it. But I do not see why that application should be, in substance, different from any other application so long as he states what he is going to do. The Home Secretary must know, of course, the details of what he is going to do, and then, as I say, the Inspector would see that he was doing it.

16019. And he would report his experiments?—He would report his experiments on a form which would put these points down in detail.

16020. But you suggest that on his application for a licence he would so far indicate the precise nature of his experiments as to inform the licensing authority what amount of pain would be involved?—Certainly.

16021. That would merely come to this, would it not, that the certificate and licence would be on one document?—That is the whole point.

16022. It is merely saving paper?—No, it is saving more; it is saving office work going about and bothering people to countersign these papers.

16023. (*Mr. Ram.*) Then would you do without the signature in the case of all experiments in which the animal would die under the anæsthetic?—No, I would have every application for a licence countersigned, but at the present moment we have practically to have three documents filled up, and sent and filed.

16024. (*Sir Mackenzie Chalmers.*) By six people?—Yes, for only one thing; and that, I suggest, is unnecessary. One document ought to cover the whole thing.

16025. (*Mr. Ram.*) You would still have a countersignature?—Yes, for the assurance of the public, certainly.

16026. I have only a few points, as a great many of the matters I wanted to ask you have already been put by Sir Mackenzie Chalmers. You recollect that one of the Commissioners put to you, with regard to the suppression of rabies, that in Cheshire they had done away with the muzzling order and got a clean bill of health?—Yes.

16027. Was Mr. Long's order compulsory on the whole country?—Yes.

16028. Do you attribute to the fact that it was compulsory, and compulsory over the whole country, rabies having been practically stamped out here in England?—Yes. I would point out that the north-western district of England (Lancashire and Cheshire), was the focus of rabies, together with the Metropolis; it had been stamped out by muzzling in the Metropolis, and it was stamped out similarly in Lancashire, where the population, of course, is much greater than in Cheshire. Then, as regards Mr. Long's action, as I represent the British Medical Association, may I point out also that so far as I know, the British Medical Association was the only public body in this country that thanked Mr. Long for getting rid of hydrophobia.

16029. A good many individuals have thanked him, I think?—Yes, but I mean the only public body.

16030. Then as soon as Mr. Long's Act passed it became effective in Cheshire as well as in any other part of the country?—Yes.

16031. One or two general points first. There is a quotation of Sir Frederick Treves which you said you thought you could get for us. It is referred to in your evidence on page 719 as follows:—Sir Frederick Treves is reported to have said that he had learnt nothing, as you know, from experiments. Then you stated that he wrote a letter to the "Times" to the effect that in one particular case, one particular form of experiment, he learnt nothing from it, and you stated that you would be able to furnish the reference to the "Times." Have you been able to get it?—I will put it in now.

16032. With regard to allowing students to operate, would you, in any case in which a student was to be allowed to make an experiment for the first place, have the animal under a lethal dose of anæsthesia?—Yes, certainly, if a student was going to do the experiment.

16033. And, secondly, would you have it always under the supervision of some authorised and licensed person?—Certainly.

16034. And would you make that authorised and licensed person responsible for the fact that the anæsthetic was so administered as to be efficient at the time, and certain to be lethal?—Yes.

16035. And only under those conditions would you allow students to experiment at all?—Exactly, just in the same way as any practical class of physiology is conducted now.

16036. With regard to experiments for acquiring manual dexterity, in your opinion would it be distinctly beneficial to the human race, who have to be operated on in many cases by young surgeons, who are learning and in progress of getting perfect, that young students and young surgeons should be allowed to operate on animals under a lethal dose of anæsthesia for the purpose of acquiring manual dexterity?—Certainly. May I point out that in respect of these brain operations that have been mentioned, the appearance of the normal brain ought to be first learnt on an animal. It is extremely difficult very often, when you have exposed a diseased portion of the brain, to tell whether it is actually diseased or not, and the differential diagnosis of the condition of that brain ought, certainly, to be established by experience on animals and not on man.

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16037. Is it the fact that by seeing the brain of a living animal, under an anæsthetic, of course, you can learn more than you can learn from seeing the brain of a dead animal, by vivisection?—Yes, the brain has a totally different appearance, and a totally different texture.

16038. Had you only experience of the brains of dead animals, or even dead human subjects, would it be difficult for you, in an operation on a living subject, to decide whether the brain was diseased or not?—Yes.

16039. But by examination of what I may term a living brain under anæsthesia you can differentiate between a diseased and an undiseased brain?—Yes.

16040. With regard to the knowledge of blood pressure, has that been obtained entirely, or if not entirely, largely, through experiments on animals?—Entirely by experiments on animals; the fact that arteries contain blood under pressure was proved by the experiments of Galen, who opened arteries really to see whether they contained air or "spirit" as was supposed at that time, and found that they contained blood under pressure. Next, the first definite observations were made by Hales upon animals; and then, from that time onwards, the whole of our real knowledge has been derived by observation on animals. Now, of recent years, the measurement of blood pressure is being introduced clinically, and we are now accumulating records of blood pressure in man under different diseased condition.

16041. And is the knowledge of blood pressure under certain circumstances of vital importance, both clinically and in operations?—Undoubtedly. We have never recognised its importance until recent years. Like every other advance in medicine, it comes very slowly, but we appreciate it at last when it does come

16042. With regard to inspection, you have answered to Sir Mackenzie Chalmers many of the questions that I was going to ask you; have you ever found it at all hindering to you in your work?—Oh, no.

16043. And if it was thought desirable that the number of Inspectors and the number of their visits should be increased, would it in any way, in your opinion, hinder research or hinder experiments?—Certainly not, if the Inspector was a qualified man.

16044. Do you think that inspection, such as it has been hitherto, has been useful in any way at all?—No, I do not think it has, but there is no objection to it.

16045. Why do you think it has not been useful—for what reason?—Because my experience is that there is really no violation of the Act, and that these iniquitous statements which have been made respecting the indifference of experimenters to pain, and so on, are absolutely untrue; consequently no Inspector has ever, so far as I know, come round on surprise visits and discovered a vivisector callously inflicting pain. The thing is ridiculous.

16046. If it be desirable for any reason that inspection should continue, do you think that the number of inspectors ought to be increased?—I think that the present number of experiments done could be quite well inspected by the present staff, and if the public, the House of Commons, for instance, thought that the inspector ought to be going round more frequently, all they have to do, it seems to me, is to make him a whole time officer. At the present moment he is not.

16047. And to that you, as an experimenter, would have no sort of objection?—No; no experimenter would object in the least to the visits of the inspector; of course, as I say, provided he is a qualified man.

16048. Now, with regard to some of these experiments of Dr. Crile, you are aware that he is reported to have used the words "these animals being under incomplete anæsthesia"?—Yes.

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16049. You have already fully explained that, and stated that in your opinion the animals suffered no sort of pain?—Yes.

16050. It seems to me that the use of those words "incomplete anæsthesia" by him is really the whole cause of the trouble which has been raised with regard to his experiments?—Possibly.

16051. The word as he used it is apparently a misnomer, is it not?—No, we constantly use the terms "incomplete anæsthesia," "light anæsthesia," "very light anæsthesia," and so on, meaning degrees of the depth to which the body is poisoned; and, unfortunately, at the present time there is actually no technical word to describe the condition of a person who is conscious, and at the same time unconscious to pain. I have brought the question myself before anæsthetists, and asked them to draw up a terminology. The word narcosis, for instance, is used by the Germans; they talk about *tiefe narkose*, and *leichte narkose*, in the same way as we do of deep anæsthesia and light anæsthesia; but in both cases the word, narcosis or anæsthesia, we understand to mean unconsciousness to pain.

16052. But, of course, to the ordinary layman, who did not know of that distinction, the term "incomplete anæsthesia" might very well convey the idea that the animal was not wholly insensible to pain?—Yes, and an ordinary honest layman would write to Dr. Crile and ask him what the words meant before he commented on Dr. Crile's action.

16053. We are glad to have your explanation of what the words do mean and what you know them to mean?—Certainly, because those words, if they are translated in any other way, conflict with Dr. Crile's statements in the preface to his paper.

16054. One question about curare, because, although we have had other views on the point, I shall be very glad of yours. The number of cases in which curare is used is comparatively small?—Yes, very small.

16055. In curare, I suppose, one or two indications of anæsthesia are absent?—Yes.

16056. I do not know if you follow my question?—Yes, quite.

16057. Therefore, whether the anæsthesia is perfect or not, depends upon the skill of the person giving the anæsthetic, and his assurance that it is a sufficient and complete dose?—Yes, his observation of what is going on, with the changes, for instance, in the blood pressure.

16058. If it were thought desirable, in order, perhaps, to allay reasonable public feeling with regard to curare, that the inspector should be present in all cases when curare was administered, would that, in your opinion, be an impediment to research?—Not in the least if the convenience of the investigator is safeguarded.

16059. Provided that the inspector was present, he could satisfy himself about the depth of the anæsthesia?—Yes, and the correctness of its administration.

16060. (*Sir William Collins.*) I do not think you have told us how long you have held a licence under the Act?—I think my first licence was obtained in 1883.

16061. Have you held it continuously since then?—Yes, continuously.

16062. And I do not think you told us what certificates you hold now and have held in past times?—I have held, but I could not tell without actual reference, Certificates A B C and E E. I also held Certificate F at one time, when I was at the Brown Institution.

16063. That was for operations on ungulates?—Yes, special ungulates.

16064. You mentioned incidentally some of the animals that you employed for your experiments; could you name them all?—Yes. I have experimented on the rodents, on rabbits, and guinea-pigs. I have experimented on carnivora, the cat, and the dog. I have experimented on ungulates, as I have mentioned, on the horse, and on the donkey, and on sheep, and of higher animals, on the monkey.

16065. Could you give us any idea of the number of animals that you have employed in the course of your experiments?—No, I am afraid I could not do that. Experimental work varies very much. I daresay that I do, on an average, about 110 or 120 experiments a year.

16066. And that has been continuing over a period of 24 years, has it not?—Yes.

16067. The present Inspector, you have told us, is Professor Thane, who is on the staff of University College?—Yes.

16068. He was preceded by Dr. Poore?—Yes.

16069. Who also was connected with University College?—Yes.

16070. And he was preceded by Sir John Erichsen?—Yes.

16071. Who also was connected with University Collegs?—Yes.

16072. Do you think that University College has had an excessive amount of attention by way of inspection in consequence?—Oh, no, not a excessive amount, because, as I say, I think that inspection does no harm.

16073. I mean by reason of the facility afforded to the inspector by his connection with University College, do you think that there has been a greater opportunity, and that that opportunity has been used more by the Inspector?—No, I do not think so. Dr. Poore and Mr. Erichsen (as he was then) both came down pretty frequently to the Brown Institution when I was there. I should say that Dr. Poore inspected the Brown Institution as often as he did University College. I was only quoting Professor Thane's actual rooms—being next to mine.

16074. I suppose that University College does not have less attention by way of inspection than other places?—I do not think so at all.

16075. How many do you think of the 110 experiments that you perform in a year would be witnessed by the Inspector?—That is very difficult to say. Professor Thane has witnessed a great many of my experiments.

16076. Half of them, do you think?—No.

16077. Ten per cent?—Yes, certainly 10 per cent.

16078. Do you think more than that?—Yes, I should think even more.

16079. How much more?—You see he knows very accurately when I am likely to be doing an experiment. I have fixed days in the week which I give up to scientific work, and he knows those days, so that he very often comes in then, and finds me in the middle of an experiment as a rule. He has certainly witnessed quite 10 or even 15 experiments of mine in the year, personally.

16080. Do you think the Inspector witnesses as many as 15 per cent. of the experiments performed by other experimenters throughout the country?—I should think not. I was asking Professor Gotch how often they were inspected. I am sorry to say that I have forgotten at the moment how often he said that it was, but certainly not as much as that.

16081. That would rather suggest that University College does have more inspection than other places?—But then he is at Oxford you know, and Oxford is some distance from London. University College ought to be compared with some of the other medical schools in London.

16082. But the Inspector's duties extend over England, do they not?—Yes, they do, but you see he is a part time officer.

16083. I rather gathered from the replies that you gave to Mr. Ram that you looked upon inspection as superfluous, is that so?—Yes, I do, judging inspection from the popular meaning of the office of Inspector.

16084. Might it be regarded as harmless trifling?—I think it is a harmless non-necessity, certainly. I think the idea that the Inspector prevents pain to animals is trifling; it is perfectly ridiculous.

16085. The evidence which you have been so good as to give to the Commission has been largely concerned with physiology and surgery?—Yes.

16086. Physiology being the science of living things you would, *a priori*, suggest that it might be advanced by experiments upon the living?—Yes; I think it can only be advanced by experiments on living tissues.

16087. And surgery dealing largely with the making and healing of wounds you might also *a priori* suggest would be likely to be advanced by experiments on the living?—Yes, and inasmuch as the diagnosis part of surgery is quite as important as the operative technique, *a fortiori* it must be advanced by experiments on the living.

16088. I think you told us in your examination-in-chief that there is not a single function of the ner-

vous system, the principle of which we now know, that is not derived from experiments on animals?—Yes, not a single fundamental principle in the functions of the nervous system.

16089. The function of the sensory and motor nerves must, in your opinion, be determined by experiments on living animals?—Yes.

16090. You suggest that there should be no certificates required at all?—Yes, I do. I think they are unnecessary documents, and consequently give unnecessary work in the Home Office, and they give unnecessary trouble to investigators who have to obtain them.

16091. Are you opposed to the Act altogether?—Oh, no. I look upon the Act as necessary in view of public opinion. Public opinion has decided now for 30 years that experiments on animals should be conducted by certain persons under certain restrictions, and, of course, one quite accepts that. It is obviously a matter of satisfaction to the public, and there can be no objection to it.

16092. But from the point of view of physiology do you regard the Act as superfluous?—Quite, if you mean that the Act is really an Act to prevent cruelty to animals as it is entitled, I do object to it. I object to any Act appearing on the Statute Book as a "cruelty to animals" Act when it refers to scientific experiments. It is an unjustifiable use of the word cruelty, and it ought never to have been accepted by the Government of the day.

16093. You object to the title of the Act?—I object to the title of the Act *in toto*, but to the purpose of the Act, namely, that experiments should only be done in registered places, and only by persons who hold a licence from the Home Secretary there can be no objection whatever, at least, I cannot see any.

16094. But the justification which I understood you to suggest was the gratification of the public?—I did not say the "gratification" of the public. If the public is uneasy on a public question I think it has a right through the House of Commons to be satisfied, and this Act is the satisfaction to the public.

16095. Would you agree with a physiologist who was in the witness chair some days ago, who told us that the Act was entirely opposed to the advancement of physiology?—If he meant that the Act hindered the progress of physiology, I am not prepared to contest it at all, because I can give you an example at the present time. There is an American physician who has come here purposely to study the surgery of the nervous system. I advised him to apply for a licence to do some experimental work in my laboratory, and it has taken him six weeks to obtain that licence. Now here is a man whose time is limited, and all these modern processes of investigation take endless time; the anatomical investigation of the material, that you prepare in the experiments takes months, and so to cut off six weeks of that man's working time I think was unjustifiable. One, of course, does not expect a Department to go on without some official routine, it must follow a certain routine, but if that routine can be diminished by an amendment of the present Act, then most certainly it ought to be.

16096. You referred us to the Report of the previous Royal Commission of 1876. I see that in that Report it is stated: "In considering the question of legislative interference, we have found in some minds a decided prepossession against it. This appears to be connected, as in the case of Mr. Lister, with a notion that such interference implies an imputation of cruelty upon those who are engaged in those investigations—an imputation they are conscious they have not deserved." Do you agree with that?—Certainly. I have already protested against the title of the Act on that very ground.

16097. But Mr. Lister regarded any legislative interference as superfluous?—So do I. I think it is perfectly superfluous for the object for which it was designed. The object of the Act was to prevent the infliction of unnecessary pain. At the time when it was passed there was no infliction of unnecessary pain. I quite agree that such legislation then was superfluous.

16098. Then you do think that the Act of 1876 was superfluous legislation?—Certainly; but if the public demand it, I do not object to it.

16099. Now, Mr. Pembrey, in answer to Question 14090, said: "I think we ought to be given a licence to cover all experiments. I think that the Act is entirely antagonistic to the advancement of physiology. If we were given a licence for all experiments, there would be no more cruelty. There would be a great saving of time and a great limitation of work, and there would actually be in the long run a saving. Q. What do you mean by a licence for all experiments? A. I mean without any conditions. Q. With or without anæsthesia? A. Yes, without any limitation at all, and without certificates. Q. That is to say, you are to put yourselves in the condition in this country which, I understand, physiologists are in in some parts of Germany, where there is no limit? A. I think there should be no limit—that is to say, that a recognised physiologist should be given a licence to cover all experiments. Q. Without anæsthetics? A. Without anæsthetics, or with anæsthetics without certificates." Does that state your view?—Undoubtedly; in this way: I propose that an applicant for a licence should state on that application exactly what he is going to do, whether he is going to do experiments with anæsthetics or without anæsthetics, although no such application has ever been made since 1876; or whether he is simply going to do inoculation experiments. In fact, he should put upon his application the details of his proposed experiments, so that the Home Secretary should know exactly what he is going to do; but the Home Secretary, being thus informed of what is going to be done, would still, of course, under the Act, have the plenary power which he possesses at the present time, of deciding whether a licence should be granted or not.

16100. And should the Home Secretary, in arriving at his decision, be advised, in your opinion, by the Association for the Advancement of Medicine by Research?—Certainly.

16101. Are you a member of that Society?—I am. The Home Secretary cannot possibly, I think, be guided correctly in the administration of the Act except by experts on the subject, who alone know what is the probable value of the proposed experiment, and who alone know the qualifications of the investigator who is making the application.

16102. I rather gathered from the Report of the previous Royal Commission that they contemplated the Home Secretary calling to his aid persons skilled, no doubt, but not necessarily belonging to any Association?—I suppose that the paragraph relating to the countersigning of certificates includes that; that is to say, that they thought it would be quite sufficient to have the names of two individuals as guarantees. The Association for the Advancement of Medicine by Research was not in existence at that time, and consequently the Commissioners, I take it, did not think of an Association doing the work. As a matter of fact, both parties do the work now; the work is done doubly—it is done, as contemplated by the original Commissioners, by two individuals who countersign the certificates; but, in addition to that, all the papers go to the Association for the Advancement of Medicine by Research for further investigation.

16103. Is there anyone who plays the part of *advocatus diaboli*?—In the Association, do you mean?

16104. No, in the advice obtained by the Home Secretary?—Not that I know of. I take it that the Home Secretary himself is both judge and advocate in that matter. He certainly is the judge.

16105. You have laid great stress upon the importance of vivisection as necessary for the teaching of students?—I have.

16106. Does that apply to all medical students?—To every one.

16107. In physiology, first of all, you mentioned?—Yes.

16108. Then in pathology?—Yes.

16109. In both those cases would it mean practice by students as well as demonstrations?—Certainly in physiology. As regards pathology, I think it ought also to be so; but, then, I am a pathologist, and perhaps I think that pathology ought to take a larger place in the curriculum than it does.

16110. And is the same thing true in pharmacology?—Yes.

16111. And in forensic medicine?—The tendency now, you know, is for special classes of forensic medicine to be given up. There is much less time spent in forensic medicine than there was when I was a student, because pathology is taking its place; so that prac-

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tically forensic medicine you might group under the head of pathology.

16112. Then you also suggested that experiments on animals should be employed in a course of anæsthetics?—That is most essential.

16113. By every student?—Yes; I had proof of that this very morning in an operation.

16114. Do you care to amplify that?—No; it might be painful. It was a question of sheer ignorance from want of having seen the condition in an animal first, and inability consequently to diagnose movements which were asphyxial in character, as contrasted with movements which were purposive in character; and, as you can see, that might be a very fatal mistake.

16115. Then, again, in surgery you advocated that all students should be instructed by vivisection experiments, with a view to obtain manual dexterity?—Yes; first with a view of obtaining a knowledge of technique, and secondly with a view of obtaining manual dexterity—the words in the Act.

16116. And you have no reason, from your own experience, to think that the effect of witnessing experiments on living animals is demoralising to students?—I know that it is not. It is a sheer invention on the part of those who offer such evidence.

16117. You quoted a portion of the Report of the Royal Commission of 1876 on that subject?—I did.

16118. There is a preceding portion of the sentence which I do not think you quoted; may I call your attention to it: "But the tendency to demoralisation is connected, as the shadow with the substance, with the rightness or the wrongness of the thing itself"; and then follow the words you quoted: "And the evidence we have quoted above seems to show conclusively that at the medical schools where such demonstrations are exhibited under anæsthetics the sense of humanity in the students is not in fact impaired"?—Quite so. I do not see how the previous words in any way alter the sense of the part that I quoted.

16119. In an earlier paragraph of the same Report they say: "It is manifest that the practice is, from its very nature, liable to great abuse; and that since it is impossible for society to entertain the idea of putting an end to it it ought to be subjected to due regulation and control." And, further: "It is not to be doubted that inhumanity may be found in persons of very high position, as physiologists. We have seen that it was so in Majendie." Apparently the Commissioners had it in their mind that there was peril in the permission of these experiments, and need for circumscribing them and watching them?—Undoubtedly they had it in their mind, although they had no absolute evidence before them that such a thing as inhumanity actually existed in the schools in England.

16120. They called attention to Majendie?—Yes; but Majendie was in 1820; that was 50 years before that Commission.

16121. (*Chairman.*) In France?—Yes, and in France; he was not in England at all. The English medical profession had always discountenanced anything like the giving of unnecessary pain in experimental work, and certainly at that time, as I have already stated in my evidence, demonstrations had been given at University College for some years, and there was no idea of demoralisation of students or anything of the kind. There had been, on the contrary, a great accession of interest and of educational value in the course of physiology.

16122. (*Sir William Collins.*) Do you think that there is such a distinction to be drawn between the practice of vivisection abroad and its practice in England?—At the present time?

16123. Either at the present time or formerly?—Formerly, no doubt, the English medical profession, as a profession, like the public, were more humane than their continental brethren, but at the present time if you read any scientific record of experimental work, you will find that the animals are all anæsthetised.

16124. Then do I rightly understand that you do not draw any distinction between vivisection, as now practised on the continent and in America, and as practised in this country?—I cannot say that I draw no distinction, because we know perfectly well that the Romance Nations, for instance, have not the same natural regard for animals that we have. They do not value animal life or regard animals in the same way that we do.

16125. (*Mr. Ram.*) An English crowd would not stand a Spanish bullfight?—Certainly not.

16126. (*Sir William Collins.*) Do not the Spanish retaliate upon the English foxhunt?—Yes, they may. It is a question of degree. That is not a question really which has ever appeared to me to have any bearing on this subject. We are concerned here with our work in the English medical schools, and it does not matter what goes on on the continent; we have nothing to do with it from a legislative point of view, and therefore it always seems to me utterly irrelevant to the subject, so far as we are concerned as a profession.

16127. You referred us to the Report of 1876, and I was anxious to know whether these paragraphs were present to your mind?—Yes, that paragraph relating to Majendie was.

16128. And his "infamous" experiment, as Sharpey call them, according to that Report?—Yes.

16129. You spoke of there having been no evidence before the last Commission of "inhumanity' in this country; they had the evidence of Dr. Klein, had they not?—Yes.

16130. You said that you were unable, or you omitted, to give us that quotation which you kindly promised about Sir Frederick Treves. It was, I understood you to say, a contradiction of something which is frequently quoted from his works?—The original quotation, I think, was in a lecture.

16131. Is this the original quotation, that we may have it clear? This is from the "British Medical Journal," of November 5th, 1898, in the report of an address by Sir Frederick Treves on "Some Rudiments of Intestinal Surgery," in which he stated: "Dr. Halsted, by the way, expresses his conviction 'that there should be a law compelling all surgeons to practise on animals the operations for circular suture of the intestines and for intestinal anastomosis.' I hope this view will not commend itself to the legislators of this country. Many years ago I carried out on the continent sundry operations on the intestines of dogs, but such are the differences between the human and the canine bowel, that when I came to operate upon man I found I was much hampered by new experience—that I had everything to unlearn, and that my experiments had done little but unfit me to deal with the human intestine"?—Yes, that is the reference. I have obtained for you Sir Frederick Treves' answer to it.*

16132. I am much obliged to you?—Of course, Sir Frederick Treves could answer for himself, if the Commission desired it, naturally, but may I point out also that he did those experiments many years ago, and that is why I should not accept his experience any more than the personal experience of a man who had done experiments which were no use to him. I may suggest, with all respect to Sir Frederick Treves, that we have, many of us, done experiments which have been failures.

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* The witness subsequently forwarded the following letter:—

To the Editor of the Times.

Sir,—My attention has been drawn to a letter in the "Times" of April 9th, signed by Mr. Trist as Secretary of the London Anti-Vivisection Society. In this letter it is stated that I have testified as to the fallacy of vivisection.

My solitary utterance on the subject of vivisection is contained in an address delivered at Birmingham, in October, 1898 (Lancet. November 5th, 1898). Speaking of the suturing of intestine, I said that I had found out that operations upon the intestines of dogs were useless as a means of fitting the surgeon for operations upon the human bowel.

Those who are familiar with the controversial methods of the anti-vivisection party will not be surprised that certain of my remarks have been cunningly isolated from the context, and have been used (in advertisements, pamphlets, and speeches) to condemn all vivisection experiments as useless. The fallacy of vivisection can hardly be said to be established by the failure of a solitary series of operations dealing with one small branch of practical surgery.

No one is more keenly aware than I am of the great benefits conferred upon suffering humanity by certain researches carried out by means of vivisection.

I am, Sir, your obedient servant,

FREDERICK TREVES.

6, *Wimpole Street, W.*
April 14th.

16133. We have also had our attention called to a passage from Koch's work on the cure of consumption, in which he said: "Here, again, is a fresh and conclusive proof of that most important rule for all experimentalists, that an experiment on an animal gives no certain indication of the result of the same experiment upon a human being." That, I gather, you would not agree with?—I do not agree to that at all.

16134. Now, in your *précis* you refer to the morals of the question of vivisection and anti-vivisection?—Yes.

16135. You gave us the views of the British Medical Association on the morality of vivisection and the immorality of anti-vivisection?—Yes.

16136. I understand from you that, in your opinion, to experiment upon man is immoral?—Certainly. A modification in surgical procedure might be a trivial matter, in that it did not involve any risk to life, and that, if tried on a man first, would by many people be called an experiment. I should not object to that if there was really no risk to the individual on whom it was tried, and, of course, that has been the practice of all surgeons who have not first tried their methods on animals; it has been a common practice, I mean, up to the present time. But I suggest that as a general principle, it is undoubtedly immoral. If a new idea or a new method is to be tried, it certainly ought to be tried on an animal first, because, as I have pointed out in my previous evidence, it is not only that you do not know the result until you have made the experiment, but you actually do not know that your method of performing the procedure is going to be the best.

16137. Then you speak of the morality of vivisection as against the immorality of the anti-vivisectionists?—Yes.

16138. And you suggested that the anti-vivisectionists were immoral in utilising knowledge which had been obtained by means of experiments on living animals?—Yes; I suggest that they are immoral from two points of view. I suggest that they are immoral, in the first place, because they oppose means whereby suffering and disease may be prevented, and I think it is immoral to do that—to oppose means whereby we can obtain new procedures against disease and suffering; that is the first thing. Then, inasmuch as they hold that vivisection is immoral—that it is immoral to use animals for these purposes—I say that it is immoral of them to make use of knowledge gained by vivisection. And I think that is the general public view also. I repeat what I said before—that the money for a so-called anti-vivisection hospital in Battersea was collected on that very ground, and yet now, at the present time, evidence has been laid before the Commission by anti-vivisectionists—Mr. Coleridge, Dr. Stephen Smith, and others, claiming that they are justified in making use of the knowledge derived by vivisection.

16139. Mr. Tomkinson put it to you, when he said that, in your opinion, all pursuit of knowledge was moral, and any obstruction to the pursuit of knowledge was immoral?—Certainly.

16140. But human vivisection, in your opinion, would be immoral, I understand?—Yes.

16141. And if knowledge could only be obtained by experiments on man there would be a conflict between your morals and your pursuit of knowledge, would there not?—No. The moral question would then come in in the sacrifice of the man; and if a man was a scientific observer, who chose to sacrifice himself, I should say that he was not doing an immoral act, unless he caused some serious disability to himself, as some men have done.

16142. But take the case, not of experiments upon the man himself, but upon other persons—involuntary experiments?—I do not approve of them.

16143. For instance, when Lady Mary Wortley Montagu introduced inoculation into this country, the practice was started upon some criminals in Newgate, I think. Do you think that was moral or immoral?—I think it was immoral.

16144. Recently I think Dr. Garnault, in Paris, has inoculated himself with tuberculosis?—Yes.

16145. Do you think that is moral or immoral?—I think it is decidedly immoral, because practically it was one form of committing suicide, or it might be.

16146. That is to say, it would be immoral to expose a human being to peril or disease?—Yes, to disable him.

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16147. The knowledge obtained from experiments on human beings in that way would, in your opinion, be immoral knowledge?—I think it would be immorally gained.

16148. And you ought not to use it?—Oh, no.

16149. I understood you to say that the knowledge obtained by an immoral procedure ought not to be used; that it was immoral to do so?—What I said was that persons who held that vivisection was immoral were not entitled to use the knowledge thus gained.

16150. But you hold that human vivisection is immoral?—Certainly.

16151. Would you not be similarly barred from using knowledge obtained by human vivisection?—Certainly.

16152. That is what I put to you?—I beg your pardon. I did not understand your question. I certainly think so.

16153. Was not some knowledge obtained by the experiments of Herophilus upon human beings?—We are told so, but it is very uncertain really whether he actually did vivisect human beings. It is said that he anatomised them.

16154. Is it not said that he dissected alive some 600 persons?—The allegation is made.

16155. He was so charged by Tertullian, was he not?—Yes, I believe so, but we do not actually know what knowledge he did gain by those alleged vivisections of human beings.

16156. Have you not called attention to the work of Herophilus in your book on "The Brain and Spinal Cord"?—Yes. I have quoted him in my historical narrative.

16157. Do you suggest that he, by his investigations, added knowledge with regard to the nervous system?—He is alleged to have done so.

16158. Was he "the first to discover the peripheral nervous system or nerves, that these latter were connected with the brain and spinal cord, and that they conveyed sensory impressions"?—He was so alleged.

16159. Do you not allege it yourself?—No, I quote it, but I should be very sorry to vouch for its accuracy, because, as a matter of fact, the most detailed experiments on conduction are those of Galen, which I also quote.

16160. Was Galen not charged with experiments on human beings?—He was accused of it.

16161. He did use a good many animals for experiments, did he not?—Yes, the animal apparently that he used was the pig, and in the mediæval period they also used the pig.

16162. I gather from your book that you set a good deal of store upon the experiments made by Galen?—Yes.

16163. Have the results which he obtained remained absolutely correct until the present day?—Yes, they are the foundation of our knowledge.

16164. There was a piece of evidence given by the late Sir James Paget before the last Royal Commission, upon which I shall be glad to have your view. He said: "There must be reason and moderation in the use of these experiments"—that is, vivisection experiments. "I can quite believe that ardent physiologists put more trust in the experiments on living animals than I should; and certainly those studying therapeutics and diseases think more of them than I should. I think more of the advantage of clinical inquiry. But I am very anxious that there should not be a general condemnation of experiments on animals, since it seems to me that there are a number of things absolutely essential for the life of man that cannot be ascertained by other means." Do you agree with that?—Certainly. What Sir James Paget is saying there is that clinical observation is very useful and that experiments are useful.

16165. (*Chairman.*) You say that you agree with that, but I rather gathered from the evidence which you have given before that you place a good deal more faith on experiments on animals than Sir James Paget wished to express in that answer?—I cannot extract that from Sir James Paget's statement. Sir James Paget is saying there what everyone must agree with.

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16166. He says: "Some things," "in some instances." That was what I was referring to.

16167. (*Sir William Collins.*) It was rather a question of the relative importance of clinical inquiry and experimental inquiry?—I do not agree that there is any relative importance in any method of acquiring knowledge in our work. It has often been alleged, for instance, that anatomy is more important than physiology. It has been alleged by anti-vivisectors that experimenters, as they call them, have neglected anatomy for physiology, and in the same way it might be alleged, or it might be deduced from Sir James Paget's remarks, that persons like myself would neglect clinical observation for experiments in the laboratory. It is a monstrous suggestion altogether; gether; it is perfectly monstrous.

16168. (*Chairman.*) I do not understand Sir James Paget suggesting that; it may be true in some cases, but I do not think he is suggesting it. I think that what he suggests is that, out of the sum of human knowledge obtained, he inclined to the view that more has been gained from clinical observation and less from animal experiments than you would put it. That is all I understand him to say?—I suggest that an attempt to make a comparison of that sort is absurd from the scientific standpoint. Both avenues of knowledge, if properly pursued, give us new ideas.

16169. I think you hardly follow me. I am not suggesting that Sir James Paget says the reverse of that in the least; he says that both avenues will bring results, and they are not put in conflict at all?—No, I am sorry that I did not understand you. I know the point that has been raised again and again; it is suggested that clinical observation is better than experimenting in a laboratory. All I maintain is that such a statement as that is absurd from the scientific standpoint. There are some things that you can gain from clinical observation which you could not see in a laboratory. Of course, that goes without saying, and *vice versâ*. You must do both things; you must have all these avenues of work open.

16170. (*Sir William Collins.*) The President of the Royal College of Surgeons, when he was here, told us, I think, that he had never himself practised vivisection, and he did not advise the practice of vivisection for the purpose of acquiring manual dexterity. I gather from what you have told us that you disagree with that?—I totally disagree with that, of course.

16171. I gathered from some of his replies that he relied upon his experience in operations on the human subject?—He is obviously one of those who have learnt on man. He has gained his experience by operating on man.

16172. That you regard as immoral?—Certainly. I think the more moral proceeding is for a man to have learnt by experiments on animals first.

16173. (*Chairman.*) Do you mean by experimenting or by operating on patients?—By operating on patients.

16174. What you said was immoral was not operating on patients, but experimenting upon them?—That is what I meant.

16175. (*Sir William Collins.*) I understood you to say that knowledge obtained as the result of experiments upon human beings in the course of surgical operations was, in your opinion, immoral?—The use of the terms "moral" and "immoral" are relative; but what I say is, that I think it is, if you like to put it so, a more moral proceeding for a man to gain that knowledge of technique, and so forth, by trying first on an animal the experiment, or by being educated by operating on an animal, before he operates on a man.

16176. That introduces us into the sphere of relative morals?—Well, it is the basis of the whole of education. Are you going to gain your experience on the bodies of persons that come into your hands, or are you going to gain it in the laboratory by experiments on animals? That is what it comes to; you cannot shirk the question.

16177. I took the notion of relative morality from yourself?—Yes, certainly.

16178. We have many times had the question of reflex action and purposive movement before us, and I shall be glad to get from you, as a great authority on neurology, what you really mean by reflex action?—By a reflex action I mean an automatic, which may be, and usually is, an unconscious response to a stimulus.

16179. Is reflex action ever conscious?—That depends upon the sense in which you use the term. A perfectly conscious being may execute a reflex action. For instance, if somebody suddenly stuck a pin into my hand I should certainly draw my hand back, and of course I would be perfectly conscious; but there would be nothing purposive in that act of mine; it is purely reflex.

16180. Would it not be a purposive act?—Not at all; the time is too short. In my opinion, a purposive act is a conscious act.

16181. Is a purposive act always a conscious act?—In my opinion, a purposive act is a conscious act.

16182. (*Mr. Ram.*) What is a sneeze?—It is purely reflex.

16183. I am conscious when I sneeze?—Exactly; that is what I say. You must not introduce the connotation of consciousness in the discussion of reflex action, because the latter only means that the response in the nervous system is executed within an extremely short period as contrasted with a response which involves the higher centres, and therefore takes a longer time to execute. But if you introduce the question of consciousness or unconsciousness, then you must add a great deal of analysis of higher functions, especially in regard to this question of anæsthesia.

16184. (*Sir William Collins.*) You have called attention to that, I think, in your book on the "Brain and Spinal Cord," where you say: "A vast array of writers have discussed the question as to whether we are to regard the lower animals as possessing, as they say, consciousness, and from this have even proceeded to formulate ideas as to whether they have a soul; while owing to the remarkable discoveries of the pathologists about 40 years ago as to the functional activity exhibited by the spinal cord of the frog, when separated entirely from the brain, an animated debate was actually excited as to whether the spinal cord possessed the attributes of the mind"?—Exactly, that was so.

16185. G. H. Lewes, I think, took part in that discussion?—I had forgotten that.

16186. Is a frog deprived of its cerebral hemispheres capable of purposive acts?—It is capable of most complex reflex acts which might be regarded by an ordinary observer as purposive, but to argue from that to purposive acts such as are performed by warm-blooded animals, with an intact nervous system and fully conscious, is quite another thing.

16187. Are those acts of a brainless frog conscious acts?—No; in my opinion, there is no connotation of consciousness unless you have an activity of the highest centres present. The cortex of the human brain is undoubtedly necessary for the purpose of consciousness.

16188. In your *précis* you state that "no experiments on unanæsthetised animals of a painful character have been performed in the United Kingdom during the administration of the Act"? So far as I know, there never has been a cutting operation performed on animals without anæsthetics.

16189. I could not quite harmonise that with what Dr. Thane told us in answer to Question 457, where he is asked, "Are there any experiments that you can describe as being really painful"?—That is another thing. He means painful after an inoculation. I am not aware of any application ever having been made to the Home Office for a cutting operation to be performed without anæsthetics.

16190. Then, when you say that no "experiments of a painful character have been performed on unanæsthetised animals in the United Kingdom during the administration of the Act" you are prepared to allow that there were these painful experiments which were referred to by Dr. Thane?—Not at all. I do not know what Dr. Thane is alluding to except inoculations. I am sure, at least from my own personal knowledge, that he was not alluding to a cutting operation performed without anæsthetics. The question has been raised again and again.

16191. I was about to read his answer. I thought you knew what it referred to?—I beg your pardon.

16192. His reply was, "On the other hand, it is certain that in some cases of this group"—that is,

experiments performed under Certificate A——"—Yes, those are inoculations.

16193. "The infection or injection is followed by great pain and suffering. I may mention the injection of tetanus toxin and the infection with plague; also the insertion of certain drugs"?—Yes, none of those are what I stated. I was referring to cutting operations.

16194. Are they not performed on non-anæsthetised animals, and under the administration of the Act?—Yes, but they are not cutting operations.

16195. But I did not gather that the passage which I have read to you contained that proviso?—What I referred to in my *précis* was a statement which I have made again and again in the public Press, and which has never been challenged, that no cutting operation on an animal has ever been performed in this country without anæsthetics under the administration of the Act.

16196. I have the words of your *précis* before me, and they are: "No experiments on non-anæsthetised animals of a painful character have been performed in the United Kingdom during the administration of the Act"?—Exactly, and I have explained to you what that sentence refers to, namely, cutting operations.

16197. Without that elucidation it was not perfectly clear, to my mind at any rate?—Because you hardly realise that a *précis* does not necessarily convey everything that is in a man's mind.

16198. It shows the advantage of a little examination on the *précis*?—Quite so. But the anti-vivisection party have repeatedly alleged—in fact, it is their whole stock-in-trade—that cutting operations are done in this country on animals without anæsthetics; it is part of their business to show that; and, as I have stated, under the Act it is impossible, because no application has ever been made to the Home Secretary to perform such operations.

16199. Now, with respect to the experiments of Dr. Crile's to which you have called our attention, and which have been so often before us, they were done, I understand, under your guidance and at your suggestion?—Yes.

16200. And they were continued in America, I think, subsequently?—Yes.

16201. Did the series in America differ from those which were performed in this country?—I do not know.

16202. What was the conclusion at which Dr. Crile arrived as the result of his experiments?—That there were changes in the blood pressure of a marked character—changes which we ought to contend against in operations on man.

16203. I see that the conclusion of his paper is set out thus. Is this correct. "Surgical shock, then, is due mainly to a vasomotor impairment or breakdown. The cardiac and respiratory factors may be of considerable importance. However, the main effect is on the vasomotor mechanism. If the foregoing be true, it will be seen how much more important is prevention than treatment. Prevention of shock may best be accomplished by taking into account all the known physiologic functions of every tissue and organ of the body in a way that would suggest itself to any practical surgeon. While the cause may be local, the treatment must be general. It would seem to be desirable to direct special attention to the distinction made between collapse and shock; the result of action is reaction; of rest is restoration." That was the conclusion of the whole matter, was it?—Yes.

16204. And I understood you to say that these experiments of Dr. Crile exactly reproduced what was done on a human being?—Yes.

16205. Among those experiments was one—I think the first experiment—of cutting out both cerebral hemispheres?—Yes.

16206. Have we an analogue to that on a human being?—No, I did not say that all the experiments of Dr. Crile exactly reproduced what was done on a human being.

16207. That description would not apply to them all?—Certainly not. If you wish to analyse any physiological function you must introduce into the experiment various operative procedures to exclude the functions of different parts, which, of course, is not necessarily a part of the procedure of an operation on a human being.

16208. Tearing out the brachial plaxus was another?—Yes, that was to imitate a well-known fly-wheel accident which is not uncommon in factories.

16209. Crushing the foot extremely before the corneal reflex was abolished was another?—Yes.

16210. That, you say, tarsectomy represents?—I said that tarsectomy reproduced the crushing of the bones of the feet. I should say that a railway accident also correctly represented the experiment you are describing now.

16211. Tarsectomy had been performed long before Dr. Crile's experiment, had it not?—Yes, all the operations for divisions of bones had been performed, of course, long before anæsthetics even were given.

16212. Then crushing the testes and other parts?—Yes, that is a most important question, because the operation of castration has frequently been followed by very grave shock—fatal shock.

16213. I did not quite understand you. I thought you suggested that there was some analogous operation on the human body to the experiment of Dr. Crile of pouring boiling water on to the intestine?—Pouring boiling water into the abdominal cavity. It does not matter whether it is on the intestine or the abdominal wall.

16214. Any procedure in which boiling water was used?—Yes. I quoted the injection of scalding steam into wounds of the liver.

16215. Would the temperature at which that steam touches the liver be the same as that of boiling water applied to the intestine?—Certainly.

16216. Really?—Practically. You get the nozzle of the injector close to the liver; that is the whole object of the procedure—namely, to produce instant coagulation at a scalding temperature.

16217. How is the jet of steam produced?—By a boiler under pressure.

16218. Do you think that the temperature applied to the organ by that means would be that of 212 deg. Fahrenheit?—No, not absolutely, but practically the same thing. You must produce a scalding effect, and the question is whether it will produce special shock.

16219. Had that method not been employed before Dr. Crile's experiment?—Oh, yes.

16220. The President of the Royal College of Surgeons told us, I think, that he did not like many of these experiments of Dr. Crile. Tastes differ?—I do not know what anybody means by liking or not liking an experiment. To my mind, as I said the other day, in answer to Mr. Tomkinson, about the use of the word terrible, or disagreeable, and so forth, if an animal is anæsthetised all those expressions are absurd. If the animal were not anæsthetised, then, of course, it would be quite another question.

16221. Do you mean that if these experiments of Dr. Crile's had not been performed under anæsthetics they might be described as outrageous?—Certainly, because here we are armed with anæsthetics, and it would have been the infliction of terrible pain—well, wickedly.

16222. The introduction of anæsthetics into these experiments, in your opinion, made all the difference between their being outrageous and, shall I say unfairly, moral?—Certainly, at the present day, because the whole idea was to investigate conditions of shock as they arise in operations on the human being, and no one would dream of applying such procedures to a human being in a conscious and sensitive state.

16223. Did I understand you rightly to say that you would have no objection to your own pet dog, if it were under anæsthetics, being submitted to those same experiments?—Certainly, if my own pet dog was to be killed and it was put under anæsthetics, I should not care what was done to it as long as it did not recover consciousness to pain. Why should I?

16224. I am afraid that I am not in a position to make replies?—But you asked me a question, and I think there are deductions from questions which one is sometimes entitled to comment upon.

16225. Have you heard of some experiments which Dr. Watson performed in America, of dropping dogs from a height?—Yes. I know of those experiments.

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16226. Were they, in your opinion, valuable experiments?—I cannot at the moment call to mind whether they revealed any new conditions; I should have to look them up again to see whether, in my opinion, they really did add to our knowledge. I cannot at the moment express an opinion.

16227. Were they justifiable, in your opinion?—Certainly.

16228. Were they performed under anæsthetics?—I cannot tell you that, but I should say that the mere dropping of an anæsthetised dog from a height in order to obtain concussion of the spinal cord, would be a very useful experiment, if it was followed up by an anatomical investigation of the spinal cord, and I can see no objection to it whatever, if the dog was anæsthetised.

16229. Were those experiments of Dr. Watson's severely condemned by the "British Medical Journal"?—Quite possibly, if the dogs were not anæsthetised.

16230. That journal is the organ of the Association on whose behalf you are giving evidence?—Yes.

16231. With regard to the thyroid and myxœdena we have had a great deal of evidence on that subject, Should I be right in thinking that clinical investigation had, prior to your own experiments, may I say, suggested a close relationship between the diseases and defects of the thyroid gland and the cretinoid condition of a human being?—No. Sir William Gull, in speaking of those patients as cretinoid, had no pathological investigation of the case before him. The only pathological investigation that was made at all was Dr. Ord's. The word cretinoid was used by Sir William Gull simply from the appearance of the patients, but he, of course, had no idea whatever that it had anything to do with the thyroid gland. Dr. Ord's case was the first case in which the thyroid gland was shown to be degenerated, as well as other organs in the body.

16232. In Dr. Ord's case he showed that the diminution of the thyroid gland was associated with an almost complete annihilation of the proper gland structure by the degenerative material, did he not?—Yes, but all the clinical work, as I have pointed out, was subsequent to Schiff's experiments in 1859. Mine were not the first experiments on the subject.

16233. Had not Hirsch pointed out that goitre and cretinism were a pair of diseased types closely related in their pathology and etiology?—Yes.

16234. That would be prior to 1885?—Yes, certainly.

16235. And had not Hilton-Fagge contributed some cases on sporadic cretinism occurring in England as long ago as 1871?—Yes.

16236. Had he not pointed out there that there was no thyroid gland to be detected?—I do not remember that, but, of course, the connection between sporadic cretinism and goitre had been pointed out a great many years before that.

16237. Do you remember Curling's work in 1850, in which he published two cases of absence of the thyroid body connected with defective cerebral development, where he showed that there was not the slightest trace of the thyroid body in those cretins?—No, but it is quite possible. Many cretins, on the contrary, have a very large thyroid.

16238. Had not Fagge suggested that the presence of goitre was protective against cretinism?—I dare say, but the clinical knowledge on the whole subject was in a complete fog, of course.

16239. But through the fog was there not emerging a notion that cretinism was combined with absence of the thyroid gland?—No, I do not think so at all, because many cretins have enlarged thyroids.

16240. Did not Curling himself in 1850 call attention to the fact that cretinism was combined with absence of the thyroid gland?—I do not know. I do not know the case that you refer to—the paper of Curling's.

16241. When was the first successful operation in this country of transplanting the thyroid gland of sheep into a human subject?—That I do not know. I do not know who did the first transplanting of sheep's thyroid at all.

16242. Have you never heard of a case?—Oh, yes, I have a sort of idea, but I really cannot answer that question.

16243. You were, I think, on some Commission or committee that inquired into tuberculosis in animals?—Yes, it was a Departmental Committee.

16244. It was a Committee of the Privy Council in those days?—Yes.

16245. You are not, I think, a member of the present Tuberculosis Commission?—No, I am not.

16246. Do you accept the view that the so-called tubercle bacillus is the cause of tuberculosis?—Of course.

16247. And is it capable of being transmitted hereditarily?—Do you mean by the spermotozoa to the ovum, or in the ovum itself?

16248. The reason I ask is because of your own special report upon that former Committee?—My minority report of one, you mean?

16249. May I read the words, to remind you of what you wrote: "Tuberculosis is notorious, even among the laity, as a disease which is transmitted from parent to offspring. This is a fact with which cattle breeders are specially familiar, and which finds strong expression in the evidence attached to this Report. Further, this generally received truth has been completely confirmed by the results of scientific investigation, as is also duly set forth in this Report." That is to say the result of scientific investigation, prior to the report of that Departmental Committee, led to the conclusion that tuberculosis was hereditarily transmissible?—Yes, that was because Baumgarten had found the bacillus in the ovum in a rabbit, and Johns in a new-born calf. Of course, if that occurred, clearly you could say then that that individual if the embryo developed, in spite of the bacillus, would be an instance of hereditary transmission.

16250. Is the hereditary transmission of tuberculosis accepted by pathologists to-day?—Not in man, so far as I know.

16251. In animals?—That I do not know. But the question then before the Departmental Committee was the question whether certain infected prize bulls communicated the disease, and the evidence before the Committee was to the effect that they did.

16252. And you suggested that there should be legislation to prevent breeding from those tuberculous animals?—My Minority Report went a great deal further than that. The reason why I was in a minority of one was because I proposed the extirpation of the disease by stamping out, and by the compensation of the owners; but none of my fellow Committee-men would report in favour of compensating the owners.

16253. But there was also this point of legislation with regard to preventing breeding?—Naturally, but that was only a side issue compared with the enormous question of compensation.

16254. I should like to ask you a question in regard to the evidence given by Sir William Fergusson, who was a distinguished surgeon, of course, before the last Royal Commission. In their Report the Commission say: "Sir William Fergusson thinks that experiments which involve suffering are carried to a greater extent than they need be, and that there is continued and useless repetition. His own opinion is much less favourable to these experiments than it was when he was young, because he had much less grasp of the subject at that time. The more mature judgment of his later years has led him to say to himself that he would not perform some of the operations now that he performed in his earlier years. He thinks if the public really knew what was actually going on in this country at this time they would expect an interference on the part of the Crown and Parliament"?—I am perfectly well aware of that statement of Sir William Fergusson's. I always thought that it was a most unwarrantable statement for any man to make, from my knowledge, at any rate, of what went on in a medical school. I do not in the least degree know to what he was referring in the matter of detail, and I am not sure that the Commission pressed him on that point at all.

16255. (*Chairman.*) The operations in his early days of which he speaks were performed before anæsthetics were used?—Certainly. Sir William Fergusson was a student in Edinburgh.

16256. About what time?—I suppose about 1830.

and the description of his time, and the share that he took in resurrecting bodies, and so on, is what is called a romantic story. I cannot see any romance in that kind of thing, but I see a great deal of romance in that answer of his, so far as medical school work was concerned in 1876. I do not believe for a single moment that there is an atom of truth in his innuendo, at least, of that statement, that the physiological medical schools of this kingdom were committing cruelties by unnecessary experiments on animals.

16257. (*Sir William Collins.*) 1876 was, of course, long after the introduction of anæsthetics?—Long after; but you have the evidence before you; I do not remember his particularising in his evidence an instance of such cruelties. I do not believe that there is such an instance in the evidence you hold in your hand.

16258. Chloroform was discovered by experiments on a human subject, was it not?—The actual use of chloroform as an anæsthetic agent was practically discovered by Simpson inhaling it himself, and giving it to his friends.

16259. (*Sir William Church.*) It has been put before us several times in this room that the essential feature of Listerism was the use of the spray. Is that a total misconception?—Absolutely.

16260. Do you remember in what sort of cases Lord Lister first made use of his antiseptic treatment?—I think it was in regard to compound fractures, in which his celebrated putty was used, carbolic putty, in order to exclude organisms.

16261. Was it not his principle to try and turn a compound fracture into a simple one; that is to say, where the skin was not broken?—Exactly.

16262. And to cleanse any protruding part first with carbolic acid in order to destroy any organisms which had got into it?—Precisely.

16263. And then to protect the wound and make it into practically the same condition as if the skin had not been broken?—Yes, he covered it over with this antiseptic putty.

16264. And the spray was not used?—It was not used at all in those days.

16265. In your answer to Question 15811, you are speaking of the necessity for demonstration in teaching physiology, and you are asked: "Professor Gotch here stated that for two years he had not used living dogs for teaching purposes, and Dr. Pembrey admitted before the Commission that during all this year, at least, he had not vivisected any animals at all before his class. From that you would rather criticise the efficiency of the teaching?" and you say, "Certainly, I would." I want to be quite clear. Does that answer apply to Professor Gotch and Dr. Pembrey?—No, it applies only to the second part, because Professor Gotch gives physiological demonstrations.

16266. You were not criticising Professor Gotch's teaching, because he had only used rabbits instead of dogs in illustration of his lectures?—Not in the least. I was criticising Dr. Pembrey's statement that during all this year he had not vivisected any animals at all before his class. I think he ought to have done so.

16267. Then, in answer to Question 15952, you say: "Undoubtedly the Act ought only to apply to warm-blooded animals"—would you leave cold-blooded animals entirely outside the Act?—Yes, practically it come to this, that the frog, which, of course, is a cold-blooded animal, is used enormously in teaching physiology, yet everything can be shown on a frog after its decapitation.

16268. You say in the next answer: "It is perfectly ridiculous for fish, and so forth, to be included"?—Yes, if you take fish, for instance, I think that it is unnecessary to anæsthetise fish.

16269. I suppose you think that they suffer pain?—I am not at all sure of that. Now, I come back to an answer which I gave earlier this morning there is no pain without consciousness—that consciousness in man is undoubtedly associated only with the complete function of the cerebral cortex. As you descend the animal kingdom you at last arrive at the fish, and you find that the fish has no cerebral cortex at all. I do not, therefore, regard fish as possessing either consciousness in any way analogous to what we call consciousness or sensitiveness to pain.

16270. Shall we take the snake?—The snake has a very thin semblance of a cortex, but a fish has no cortical grey substance at all.

16271. Do you really think that the invertebrate kingdom suffer no pain?—Certainly. I do not believe that the invertebrate kingdom are at all conscious of pain. If you apply a powerful stimulus to an invertebrate, it shrinks up undoubtedly by a reflex or defensive act, but I do not think that that act is accompanied by any pain—certainly nothing in the least degree approaching what we have any idea of.

16272. Then you would not even apply an Act of Parliament dealing with cruelty to animals to cold-blooded animals and invertebrate animals?—To take a case in point, you mean that if some boys were found torturing a frog they ought not to be punished?

16273. Yes?—I do not agree to that. That point has already been frequently discussed, of course, in connection with the moral right of wild animals; I mean as regards the statutes. I do not think that point really bears upon my position at all. I am referring now to legislation affecting the performance of an experiment for the sake of gaining knowledge. I do not think it is in any way analogous.

16274. You would leave, then, cold-blooded animals, that is to say the animal kingdom, from birds downwards, outside an Act dealing with experimentation on animals?—Certainly.

16275. And you would not agree with Professor Langley, who thought it was desirable that they should be under a separate clause?—What is Professor Langley's statement?

16276. He said, in answer to a statement at Question 15399: "I think it would be better to have a separate certificate for cold-blooded animals, like fish, frogs, etc."?—I cannot see the advantage of it at all, because, of course, here again we have the question of the certificates, and may I point out once more that my proposed amendment of the Act would provide the Home Secretary with absolute knowledge of whether it was going to be done on a warm-blooded animal or a cold-blooded animal, and, if belonging to either class, which animal? All those details would be furnished to the Home Secretary, and consequently there is no reason to construct a special certificate for cold-blooded animals; it would be unnecessary. And, if Professor Langley's point was that a fish, for instance, was conscious of pain, as I have said before, I have given my reasons to the contrary.

16277. I do not wish to pursue it. Professor Langley's words were: "I think they certainly must suffer very little"—that is pain—"but I think, again, as a matter of public policy, it is difficult to press for their exclusion"?—I did not know that I was here to answer on questions of public policy.

16278. But those are the words that Professor Langley used in reply to the question, whether there was any need to include cold-blooded animals in the Act?—As I have stated, I do not think that there is any need, and in connection with fish, I have given reasons for supposing that they do not suffer pain. I see that Professor Langley says here: "The organisation of the part of the brain that is connected with pain in man is so small in those animals that it cannot have any very large function"—certainly as regards teleostean fish it has no function at all; consequently, I do not believe that fish suffer pain. There is plenty of direct evidence, of course, on that point, of fish that have taken a fly a second time, and so forth, when the hook is sticking in the side of their cheek. However, I need not waste the time of the Commission on that.

16279. You would agree with Professor Langley, but you would go a little further than he does; you say that you think that they should be excluded?—Yes, you must draw the line somewhere for the purposes of a statute, of course, and therefore I think the best line would be between the warm-blooded animals and the cold-blooded animals.

16280. (*Chairman.*) Are anæsthetics sometimes administered to animals through a tracheal tube?—Yes.

16281. And for that purpose it is necessary first to perform the operation of tracheotomy?—Yes.

16282. In those cases are the animals anæsthetised when you open the throat?—Yes, of course.

16283. They are previously anæsthetised?—Yes.

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Mr. F. Hobday, F.R.C.V.S., F.R.S.E., called in, and examined.

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16284. (*Chairman.*) You are a Fellow of the Royal College of Veterinary Surgeons?—Yes.

16285. And also a Fellow of the Royal Society of Edinburgh?—Yes.

16286. Have you been Professor of Therapeutics in the Royal Veterinary College, London?—Yes.

16287. And for six years, I believe, you were Examiner in Therapeutics at the Royal College of Veterinary Surgeons?—I was.

16288. Since you were qualified have you practised generally as a veterinary surgeon?—Yes.

16289. And you are now in consulting practice as well as in general practice?—Yes.

16290. And you have treated and operated on, as patients, a very large number of animals in your time?—A very large number.

16291. Have you experimented on them, too?—I have experimented on them, but not since I left the College. Practically I have done very little absolute experimentation.

16292. Your time has been taken up with practice?—Yes, absolutely.

16293. I am going to ask Sir John McFadyean, whom you know very well, to ask you questions about the matters on which you come to give us evidence?—Very good.

16294. (*Sir John McFadyean.*) You have for a number of years made rather a special study, have you not, of the administration of anæsthetics to domesticated animals?—I have.

16295. Did you investigate this by way of experiment, as well as by observation on animals that were to be subjected to surgical procedures?—I did.

16296. What was the motive for your undertaking the investigation when you were at the Royal Veterinary College?—When I was a student I was always taught, and I think it was generally understood, too, by the majority of practitioners, not only the veterinary practitioners, but also by the general public, that the dog in particular was a subject which could not be chloroformed with safety, and it was entirely with a view to attempting to do a little towards undermining that idea, or else seeing if the idea was absolutely a true one, that I undertook for many years a number of experiments for which I obtained the necessary license, and with various patterns of apparatus, to see whether the dog was, or was not, a good subject for anæsthesia, chloroform anæsthesia mainly.

16297. You had charge of the Out-Patients' Department at the Royal Veterinary College?—I had.

16298. I suppose there are some thousands of animals treated there annually?—Many thousands every year.

16299. Can you tell me what anæsthetics were in common use in connection with that and other veterinary colleges before you began your investigations?—If ever we had, on the insistence of the owner, to chloroform a dog, either my teacher, when I was a student, or myself, when I had the Professorship, always told the owner that the result might be fatal, and it frequently was.

16300. To what do you attribute those unfortunate results prior to your investigations?—Partly to a want of skill in administration, but mainly to a wrong method of administration.

16301. Your experiments were carried out on dogs and cats principally, were they not?—The horse is generally understood by the veterinary practitioner to be a good subject for chloroform, therefore I confined my attention to the dog and cat, because the horse was said to be a very good subject for anæsthesia, therefore I had no need to investigate it; in fact, I always used chloroform freely for the horse.

16302. Was there any sufficient experience to show that horses might with safety be anæsthetised?—Yes.

16303. Did you use any other farm animals for experiment—the pig, for instance?—I did use some pigs, but not any great number.

16304. Or sheep?—Yes, I have chloroformed lambs, but not any great number.

16305. And cattle?—Cattle I have practically no experience of. I have done a few odd ones. I have experimented mainly upon the dog and the cat.

16306. What may be said to be the general outcome of your experiments and your experience with regard to the safety of administering anæsthetics to dogs and cats?—I think I can claim that it has given a great impetus to the anæsthetisation of animals for surgical operations among veterinary practitioners; for I know that a very large proportion of the modern men, practically all the men of recent years, whenever they have any canine practice have continually used chloroform now for their surgical operations.

16307. Have you kept any record of the number of dogs that you have yourself anæsthetised?—I kept records of my first 1,200 consecutive cases, after once I had got my apparatus in working order. Since then I know that I have done many thousands, but I am in busy practice, and I thought the thing was proved sufficiently up to the hilt with 1,200 consecutive cases, and I am afraid that I have not kept definite records since.

16308. Can you give us any particulars with regard to the 1,200 consecutive cases with chloroform. Was it with pure chloroform?—Yes, or rather methylated chloroform.

16309. Not the A.C.E. mixture?—No, chloroform.

16310. What was the date of the 1,200 consecutive cases?—They have nearly all been published in the medical papers; 850 have been published. It was during the period of my Professorship at the College.

16311. Which terminated about five years ago?—Six years ago. It would be, I should think, between 1894 and 1896 roughly. It is about ten or twelve years ago.

16312. Do you remember in how many of these cases death occurred as the result of the anæsthetic?—Yes, I attributed the death to the use of the anæsthetic in five instances. But I want particularly to say here that these patients were taken as they came. By far the greater proportion of them were poor people's animals which came to the Out-patients' Clinique of the Royal Veterinary College, and we had not an opportunity to prepare them as one would do in ordinary practice with a patient brought to a veterinary surgeon for anæsthesia, mainly because we had not accommodation for taking in a number of animals to prepare them previously, partly because the owners themselves are usually very fond of their animals and will not allow them away from home longer than they can help, and partly because with a busy clinique such as we have at the College we have not time to do anything more than one considers to be absolutely necessary in the way of preparation.

16313. By preparing them you mean seeing that their stomachs were not overloaded?—Yes, I mean that they had not had any solid food that day, and were not anæsthetised with a full stomach. Observation proved on several occasions that if the animal was anæsthetised with a full stomach (like a puppy which had just gorged itself) it was excessively dangerous; it would often die.

16314. These 1,200 animals would be mainly diseased animals?—Most of them were diseased animals sent for some surgical procedure.

16315. Subjected to anæsthesia without any preparation?—Most of them without any preparation. Some of the 1,200 cases included patients either brought into the College for an operation, or which were operated on outside, in which case they were prepared, but by far the majority were absolutely unprepared. I mean that some of them were in a diseased condition in which from my experience now as a general practitioner I should have hesitated to give any anæsthetic; and the *post-mortems* were made by yourself.

16316. Could you say that your later experience, which I gather includes more than 1,200 cases, would be more favourable to the safe use of anæsthesia than in the first 1,200 cases— take your last 1,000 cases in general practice?—Yes, it would be much more favourable, I am perfectly satisfied, and I still use chloroform for any operation which I think necessitates chloroform, without hesitation.

16317. You performed very extensive operations on dogs, I suppose?—I did, and minor ones, too, for the same reason, because I was told particularly that in dental operations, for instance, on a human being, it was considered very dangerous to extract a tooth under chloroform, so that whenever opportunities occurred for that in a dog, whether they were loose or fixed I gave chloroform on every possible occasion.

16318. Have any of your operations extended over a period of an hour or two?—Yes, a number of them. The longest was five hours. That was not an operation; it was a case really of chorea. I wished to see whether chloroform anæsthesia would benefit chorea or not.

16319. Did the dog die?—No; the chloroform was given by students; they were men who had seen me give chloroform, but it was given by relays of students, because five hours is a long time for one man to be at it.

16320. I now want to read to you some evidence which has been given to us by previous witnesses bearing on this point. For instance, one witness told us, in answer to Question 812, that he thought it was "generally impossible to keep a dog alive for two hours under full anæsthesia, or for one hour." Do you agree with that?—Absolutely no.

16321. You think that is certainly erroneous?—I do not think, I know it is erroneous. It was the idea years ago, but it is erroneous. I will prove that to anybody.

16322. You still use chloroform and not the A.C.E. mixture?—Absolutely, except just in certain cases.

16323. Have you used the A.C.E. mixture pretty extensively, too?—Yes, I have used it extensively.

16324. Did you find that it was particularly dangerous?—No.

16325. But why have you abandoned the A.C.E. mixture?—I use the A.C.E. mixture still at times.

16326. But you also use chloroform?—Yes, I prefer chloroform, especially for abdominal or any serious operations, because it is so much easier for my assistant to keep the patient still.

16327. So that you are perfectly satisfied, as the result of your own extensive experience, that it is possible to keep a dog under the influence of chloroform to such a degree that it is quite unconscious for a period of two hours?—I am so satisfied of it, that I cannot understand any one who has had any experience at all making the opposite assertion.

16328. I am not sure that we have had any evidence to the contrary from anybody who has said that he had extensive experience, but it seemed to be a pretty widely-felt opinion, especially among laymen, that it was practically impossible to keep a dog under an anæsthetic for a long period without a very great risk of the animal being killed by the anæsthetic?—I know that it has been a common impression because I have many thousands of people to deal with in the course of a year, and a very great number of them are ladies, who have pet dogs, who have this impression, or who have had this impression.

16329. The next point I would like to ask you is, have you experienced any great difficulty, when anæsthetising animals, in satisfying yourself as to when they had become unconscious to pain?—No, I have had no difficulty.

16330. You do not think that there is any great difficulty in that?—I do not think that there is any great difficulty. I have purposely experimented in order to test that particular point by starting my operations soon, at a certain stage, and then, of course, leaving it until the animal is quite deeply anæsthetised, and in my own mind I have no hesitation in saying that I think I can tell when an animal feels pain and when it does not feel pain. I tell by the response to my stimulus.

16331. And in practice at the present time you are never in doubt?—I am never in doubt.

16332. (*Chairman.*) When you perform an operation do you administer the anæsthetic yourself?—I cannot do that. I must do the operation antiseptically. I cannot touch anything that is not surgically clean.

16333. It is administered under your supervision?—Yes, by my nurse generally. We have nurses for it, or it is done by an assistant, but mainly a nurse has done it lately; a lady especially trained to it.

16334. (*Sir John McFadyean.*) Do you think that the administration of chloroform to a dog involves any cruelty to the dog itself?—No.

16335. We have been told so here. I will read the answer to Question 808 in the evidence of Sir Thornley Stoker, and I think we have had other evidence to the same effect. The answer was: "The amount of terror that a dog feels, even in being put under chloroform, is rather painful to witness." Does your experience bear that out?—No, it does not. A dog will struggle against anything being put on its nose, but it is only struggling against a foreign body. If I put this mask which I have here on a dog's nose, or if I put it on the operating table it will struggle, naturally, because it is something foreign to it, but not from fear. It struggles to get away, as every dog will.

16336. You do not think that dogs exhibit signs of terror in anticipation of being chloroformed?—Not if they are gently handled. I purposely did something to test that question a little while ago in front of several gentlemen who had an idea that a dog would show a great deal of terror. They were gentlemen connected with a certain Society. I asked two of them to come one afternoon to see that what they had thought was not really the case. We chloroformed a couple of animals; we did one abdominal operation, and then, in front of them, we took a number of dogs purposely out of their kennels. I should think there were 12 or 15 dogs, one after another put on the table; we put them down on the table with the operating hobbles.

16337. Did you convince the persons for whose benefit it was done?—I think they were convinced; I believe so.

16338. (*Chairman.*) You mean that you merely put them in position without operating?—I may say that there were 10 or 12. I did two operations under chloroform. Those were all that I had that afternoon, and I took a number of dogs out of the hospital kennels and put them on the operating table with the hobbles. They were put there gently, as we always do put them, and when a dog is put on the operating table with the hobbles, if it is soothed and talked to gently, it does not show excessive fear.

16339. (*Mr. Ram.*) Those would be curative operations?—Those dogs were patients in the hospital.

16340. (*Sir John McFadyean.*) May I ask whether in operating upon dogs you invariably secure them to the table?—Not always, because occasionally, for perhaps some minor operation, a lady will insist on having the dog chloroformed in her arms, and that we do.

16341. But as a rule they are fastened to the table?—For our own convenience they are. I have some hobbles here.

16342. Have you ever seen an illustration of the way in which so-called vivisectors fasten dogs to a table?—I think I have seen a catalogue, but I forget what it was like. I think the position is similar to my own, but there are certain alterations which I think are safer for chloroformisation. I think that the table to which you refer is curved a little on the top. Mine is a flat table.

16343. In your case the dog is secured by the four legs?—By the fore legs and the hind legs, and it is stretched out in such a position that its chest gets free play.

16344. Is that in case it might be incompletely anæsthetised and be in a dangerous state when you begin to operate?—No, it is done because I think that is the safest position; it gives its chest the freest play. If its legs are extended it has the freest play for the lungs.

16345. You think that is the safest position for anæsthetising an animal?—I do.

16346. But I suppose it is also the most convenient position for the operator?—Certainly.

16347. Then I want next to elicit your opinion with regard to the possibility of inducing wounds inflicted on animals to heal directly, that is, by first intention. You succeed frequently, do you, in inducing even large wounds in the lower animals to heal without suppuration?—I do. I have a horse at the present

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moment with a wound about 11 inches long. Nine inches of it has healed without suppuration, and in the dog I should never bandage or dream of touching the wound again for from five to ten day. I never bandage wounds. Many thousands of dogs have had operative wounds made on them and have never been bandaged, and I have never bandaged an antiseptic surgical wound on a dog but once during the last six years, and that time I regretted it. I did it at the owner's request, and I regretted it. I do not always get a healing by first intention, but in by far the majority of cases I do. That means that I have no irritation there, and the dog has not licked his sutures out. If irritation is present, or if the wound is painful, there is no animal which is so eager as the dog to get at it with its tongue and spoil all my work.

16348. We have been told here that it is scarcely possible to apply bandages to one of the lower animals in connection with a surgical wound with any chance of the bandage being kept on; that the pain in the wound will usually induce the animal to tear the bandage off. What do you say in regard to that?—In reply to that I say that if my operation is done antiseptically, then, if the dog tears the bandage off it will not be pain in the wound that makes it tear it off, but the restraint of the bandage to which it is unaccustomed, and that is one of the reasons why I prefer not to bandage. I do bandage afterwards if the wound suppurates; but I always know that the dog has to be watched to see that it does not get the bandage off, and it is a continual nuisance to me.

16349. But do you ever leave an extensive surgical wound in a dog unbandaged?—Always.

16350. That is your rule?—That is my absolute rule.

16351. (*Mr. Ram.*) You put a dressing on?—Yes; the operations are done antiseptically, and the wound afterwards is dried properly with ether and various things. Time is taken over all this, and the wound is afterwards dressed with iodoform colloid, or some such preparation (we have several preparations of that kind), in order to hermetically seal it from contamination from the exterior.

16352. (*Sir John McFadyean.*) It has also been represented to us that extensive surgical wounds inflicted in physiological experiments on the lower animals must necessarily be painful when the animal comes out of the anæsthesia. Have you any evidence bearing on that question?—I have no experience of a physiological laboratory; I never worked in one; but my own experience is that the statement is absolutely untrue. I argue in this way: the wounds are the same whether the human surgeon makes them with the scalpel or I make them; if they are done antiseptically they are the same in principle. If my wound was painful I should expect, from my knowledge of the dog, or even of the monkey (for I have also had experience of monkeys), that the animal would immediately go for those sutures and pick them out, or, at all events, lick the wound continually. That is evidence of irritation in the dog, but I do not find that as a rule. I am annoyed when I find that a wound has got contaminated and my dog has licked the sutures out. By far the greater majority of my wounds heal by primary union.

16353. And apparently without pain?—Apparently without pain. I have done this repeatedly to prove it. I have done abdominal section, removed the ovaries, and perhaps the uterus of a bitch, and sent it away from the hospital repeatedly within 10 days, and frequently on the fourth, fifth or sixth day.

16354. Do you employ any restraint to prevent the dog licking the wound?—Absolutely none. The dog is simply put into a cage and given over to the nurse until it is out of the chloroform, and when once it is out of the chloroform it is simply put into a kennel like other dogs.

16355. What do you say with regard to the popular belief that licking is good for a dog's wound?—It is not good. I want wounds left alone. If a dog licks the wound it infects it.

16356. It is an indication that it is experiencing irritation?—Yes. That, again, I have gone into, because when the wounds have been irritable the application of antiseptics has caused that irritation to cease quickly.

16357. There is just one other question that I should like to ask you, and that is, what would be your opinion as to the wisdom and advisability of making it illegal to use dogs for experiments. I want, in the first place, to ask whether you think it would be in the interest of the extension of knowledge with regard to disease of the dog itself?—Do you mean to use diseased dogs for experiments?

16358. No. I mean, would you approve of its being declared illegal to make an experiment on a dog, even for the purpose of extending knowledge with regard to diseases of the dog?—It was particularly understood that I was not to be asked questions of that kind. You will quite understand me. For certain reasons, I was particularly asked that I should come in the character of an independent witness, neither on the vivisection side nor the anti-vivisection side. I came on the anæsthetic question, and for other reasons, too. I was given to understand that I should not be asked questions of that kind.

16359. I have no desire to press the question, but I ask you what your own inclination may be?—I will tell you this much, that if I had a diseased dog, and I thought that some experiment upon it whilst it is in a state of disease would help other diseased dogs (and I knew it was a hopeless case with our present knowledge), I should not hesitate to try that experiment, whether surgical or medicinal, upon that diseased animal.

16360. That is hardly what I wanted. I suppose you admit that there is a good deal of ignorance with regard to diseases of the dog itself?—Yes.

16361. Is an extension of knowledge with regard to such a common disease as distemper desirable?—It is very desirable.

16362. Supposing that you were commissioned to try to extend that knowledge, how would you set about it now?—I would be only too glad to act on a Commission which involved the use of a certain number of dogs in order to save the majority of dogs.

16363. Then, that comes to an expression of opinion that to declare experimentation on dogs illegal would tend to keep up ignorance with regard to diseases of dogs?—It is impossible to make progress without utilising some healthy dogs. Take your own example of distemper; it would be impossible to make progress in the knowledge of the disease without utilising healthy dogs.

16364. Without experiments?—Without experiments.

16365. You do not think it would be possible to investigate the question of distemper at the present time without experiments?—I do not, and, moreover, I am quite prepared to go to this extent, that to let a few dogs comparatively have distemper, whether given artificially or whether given naturally, would be a much better thing than to let hundreds of thousands die every year from this scourge, as they do at the present time.

16366. (*Sir William Collins.*) Do I rightly understand that you were Professor of Therapeutics at the Royal Veterinary College for seven years?—Yes.

13667. Did you illustrate your lectures by experiments on living animals?—To some extent. I got a certificate to permit me to use anæsthesia to demonstrate the methods of giving chloroform anæsthesia. I had to get a certificate to do that before my students.

16368. You held a licence?—Yes.

16369. Which certificate did you hold?—I think it was C; it is some years ago.

16370. Was it limited to the question of anæsthetics?—No; I wanted to demonstrate poisons, too. When an animal was to be destroyed I wished to do it before a class of students, explaining the different symptoms as one went on. But another object that I had, which I worked at when I was at the College, was to try to find out the most humane method and quickest method of destroying a dog.

16371. Then, you demonstrated the poisoning of dogs by various poisons?—The killing of dogs by various things; and as the result of that I had a little apparatus made which, I think, is very humane.

16372. Did you administer anæsthetics as well as the poison in those cases?—Not always.

16373. So that they were experiments without anæsthetics?—Yes; the dog had to be destroyed, and it was destroyed.

16374. (*Chairman.*) What was the poison that you generally used?—Prussic acid.

16375. That has always been the drug used, has it not?—Yes; but the reason of my experiments was that when dogs have prussic acid given to them, it is very rapid in its action and it kills within 30 to 45 seconds; but with a large proportion of them, and with cats too, you get a most horrid yell or scream, which is very objectionable and sounds as if it might be painful. It may be involuntary, but it sounds as if it might be painful during those few fractions of seconds.

16376. (*Sir William Collins.*) Was prussic acid the only poison that you employed?—No, I used chloroform a great deal.

16377. (*Sir Mackenzie Chalmers.*) And morphia?—I did not use morphia then.

16378. (*Sir William Collins.*) You spoke of certain ill-results which had occurred in the administration of anæsthetics to dogs. What did you mean by that?—I was always taught that if you gave a dog chloroform it never woke up again, it was killed. Those are the ill-results. Frequently one used to be giving a dog chloroform, or a student would give it chloroform, and suddenly respiration would cease, and it would be impossible or extremely difficult to bring the dog back to life again.

16379. Why was that?—Because it got an overdose of chloroform.

16380. Then what you have learnt of recent years has been to measure and graduate the dose?—Yes.

16381. Do I rightly understand that you regard chloroform as better than the A.C.E. mixture for animals that you have to deal with?—For the majority of dogs I do.

16382. Do you always administer it by a mask?—Always.

16383. Not by a tracheotomy tube?—Never. You cannot with a patient.

16384. Have you any experience of hypnotism as a means of anæsthetising animals?—I have none. I have tried it and failed.

16385. Why do you not employ a tracheotomy tube in cases that you have to deal with?—Because naturally my clients would not like me to make a wound in their dog's throat when I can give chloroform so readily without it. They would not like them to go home with a wound in the throat, and the trachea itself would be injured.

16386. Have you ever given an anæsthetic by means of a tracheotomy tube?—I do not think I have. If I have it is only once or twice.

16387. Would it introduce any complication, in your opinion?—I have had no experience of it. I should not dream of doing it on a patient. The complications would be the unfortunate necessity of making a wound and having to heal it afterwards again.

16388. I see that before the last Royal Commission on Vivisection a memorial was put in on behalf of the veterinary surgeons of Scotland to this effect: "We, the Court of Examiners for Scotland of the Royal College of Veterinary Surgeons, desire to express our opinion that the performance of operations on living animals is altogether unnecessary and useless for the purpose of causation." That is signed by "James Syme; James Dunsmure, M.D., President of the Royal College of Surgeons of Edinburgh; J. Warburton Begbie, M.D.; John Lawson, President of the Royal College of Veterinary Surgeons; B. Cartledge, M.R.C.V.S., Member of Council of the Royal College of Veterinary Surgeons; William Cockburn, M.R.C.V.S.; Charles Secker, M.R.C.V.S.; and James Cowie, M.R.C.V.S. I fully concur in the above, John Wilkinson, principal veterinary surgeon to the Forces." That was put in in 1876, but the document was earlier than that. Do you wish to say anything with regard to it?—No, it is so long ago, and medical science in five years is completely revolutionised. I know nothing of their opinions then.

16389. Do you wish to give any opinion now as to whether experiments on living animals are useful for the purposes of veterinary science?—I should say the same to you as I said to Sir John McFadyean. Take the case that he instanced. If you mention any specific instance, take distemper.

16390. Have we discovered the cause of distemper?—We want to do so.

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16391. Then that would hardly be an instance of definite value that had accrued?—No, but if you want an instance of definite value having accrued, so far as I personally am concerned, it was the knowledge that the abdomen was opened continually in a physiological laboratory which encouraged me personally to open the abdomen in a dog for removing certain things from it. I mean the knowledge that it was done with safety. When I was a student in the pre-antiseptic days, as the thing was practised when I was a student, I dare not open the abdomen of a dog without warning the owner that the patient would very probably succumb, not from the operation itself, but from peritonitis or something of that kind.

16392. The abdomen was opened, was it not, in pre-antiseptic days in veterinary surgery?—It was opened, but the veterinary surgeon never failed to warn his client that there was a prospect of grave trouble afterwards. Take, for example, the operation of spaying a mare, ovariotomy of a mare, which I think now can be claimed to have been revived again in England. It was done seventy or eighty years ago continually, and spaying a cow, too. Charlier, at the beginning of last century, proved that the abdomen of a cow could be opened with safety and the ovaries removed.

16393. That was in the pre-antiseptic days?—Yes, but Charlier, looking back over his methods, was antiseptic without knowing it. He boiled everything, and cleaned his hands and cleaned the cow's passage out before doing the operation; and that was the reason of his success.

16394. Did it answer to the antiseptic method?—He did not call it the antiseptic method, but he operated with great success certainly, because he boiled his instruments, and washed his hands clean and washed his cow. As I say, during Charllier's time, in his hands the operation was very successful. After his time it fell into disuse again, because other people tried it and they were not successful, and the same with the mare. In the present day I think that any modern veterinary surgeon will go forward and do these operations practically stating that his patient will live afterwards, but the primary ones must have been experimental.

16395. In that case the animals have benefited by improvements in surgery in the case of man?—Done by human surgeons on man.

16396. Done by surgeons rather on human patients?—Yes.

16397. (*Sir Mackenzie Chalmers.*) As regards these operations that you have told us of on dogs, taking out the uterus and the ovaries, is that done on account of disease, or is it done to prevent the animal breeding?—Sometimes for one thing, sometimes for the other.

16398. Does your practice extend to horses as well?—Yes, I have a great number of horses to treat.

16399. Can you give a horse anæsthetics?—Yes.

16400. Can you give chloroform safely to a horse?—Yes.

16401. When a horse is castrated what do you use, the knife or the ecraseur?—I do not use either. I use a special instrument generally. Sometimes, if I have a pupil who wants to see an ecraseur used, I have one, and I use it.

16402. What anæsthetic do you give to a horse?—Chloroform always, or, if it is local, cocaine perhaps, but choloroform always for castration.

16403. In the case of ordinary farmyard animals are anæsthetics ever used?—They are used.

16404. When a veterinary surgeon does it?—Yes.

16405. Not when the farmer does it, I suppose?—I should think that the farmers would hardly use it. Some farmers are very clever at it, but the majority would not dare to use it. It requires special skill.

16406. In the case of a horse, I suppose it is always done by a veterinary surgeon?—No, many horses are castrated all over the country by the castrators. Thousands of horses in England even.

16407. By an ordinary man—by a gelder?—By a castrator, whose father and grandfather probably have been castrators before him.

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16408. In that case are anæsthetics used or not?—Now and again we come across a man who perhaps himself started his career at the College—a man, I mean, of some education—who will dare to use chloroform, but it is very rare to find that. By far the majority use no anæsthetics at all. To begin with, they would have a difficulty in getting a poisonous agent like chloroform; they would have to get it through a friend who held a medical qualification.

16409. Is castration a very severe operation; is there great pain?—There is great pain for an instant, but it is very rapidly done, and the horse will go straight to its manger, in the majority of cases, afterwards. In fact, I have seen them repeatedly pick up grass off the ground before the hobbles have been removed, within a few seconds of the operation done without anæsthetics.

16410. They are cast, of course?—Some are cast, some are done standing. A very large proportion of horses are done standing up. Some men simply go in without even any method of restraint at all, or without a companion. Some men will go into a loose box and operate and come out again within four minutes, with the two testes in their hands, but in the majority of instances two men are used, one at the head and one at the hind quarters, and the operator himself will do the whole thing in a few moments. Personally I have done it in two and a half minutes, so that I know it can be done, and I am not an expert at it.

16411. (*Chairman.*) At what age is the horse operated on?—Some are done as yearlings, but it is usually done at two, three, or four years old. Some horses are done when they are very old, ten and twelve years old, but the majority are two, three, and four years old.

16412. (*Mr. Ram.*) Is the horse restrained in any way when it is done standing?—Sometimes a twitch is put on to its nose, and it is pushed into a corner. Sometimes, if the horse is troublesome, the twitch is taken off and a man just holds his head. It is a very quick operation.

16413. Does the operator get underneath the horse or between its hind legs?—No, he gets just in front of the hind legs, and takes hold of the testes; the instrument is used very quickly, and the testicles are removed in a few minutes, as I say. I think there is a man who does it between the hind legs, but the majority do not. As a rule the horse does not kick.

16414. (*Sir Mackenzie Chalmers.*) Is there more danger in giving a horse chloroform than there is in giving a dog chloroform?—I do not think so. In both cases it has to be done, of course, with a great amount of skill and care.

16415. You would not expect to have a larger percentage of deaths among horses than you would among dogs?—Not if I had the supervision of it myself.

16416. You told us that you had experimented as to the most humane method of destroying a pet animal?—Yes.

16417. Is prussic acid, in your opinion, still the most humane method?—No, it is the quickest, but it is not the most humane, in my opinion.

16418. You mean that the struggle may possibly be a painful struggle?—I can hear it now. I can hear it for so long afterwards, if the animal does scream. Some do not, some will tumble dead at once, but a few do scream, and I certainly should not care to do it in front of the owner; I should prefer to get the owner out of earshot. Whether it is a cry of pain, or whether it is purely involuntary, I am not prepared to say. I do not know.

16419. (*Chairman.*) Both with cats and dogs?—Both cats and dogs will sometimes give either a scream or a yell just when they appear to be going over the border.

16420. (*Sir Mackenzie Chalmers.*) What, in your opinion, is the most humane method of destroying an animal?—I think the lethal chamber, with one or other of the present-day gases.

16421. Carbon monoxide?—I myself use a mixture of coal gas and chloroform.

16422. That would be carbon dioxide and chloroform?—That is what I use myself, and that is what I think is very humane.

16423. Does it require long immersion?—No, as a rule the patient will become unconscious in four minutes; then you leave it for a few minutes, perhaps another five minutes, to be sure; because if you take them out into the air, then they may come round again.

16424. With the carbon monoxide there is no recovery, is there?—It depends upon the quantity given with carbon monoxide.

16425. When once you have stopped the breathing there is no chance of resuscitation?—When once you have stopped the breathing, with either of them, it would be highly improbable. I have never succeeded in resuscitating an animal when once breathing has stopped.

16426. (*Chairman.*) But suffocation that takes four minutes is rather painful, is it not?—If the anæsthetic is allowed to go in gradually into the box where the patient is I think that the animal will pass away without pain, and I say so from having watched many dogs and cats put into the lethal chamber, or, rather, into a lethal box with a glass top on it.

16427. (*Sir Mackenzie Chalmers.*) There is no struggle?—Now and again you get a struggle; but any dog will struggle against inhaling anything that is foreign to it, but I do not think that is a struggle of pain. It will try to get out of the box, of course. If it is left there in the box without anything put in, it will try to get out of it quickly—that is a necessitous thing.

16428. (*Chairman.*) You do not think that it suffers pain?—I do not think so, and I speak from a large experience now.

16429. (*Sir Mackenzie Chalmers.*) You think that is the most merciful and painless death which you can inflict on animals?—Yes.

16430. For instance, would morphia poisoning be painless?—I think poisoning with morphia would be painless, but I cannot say. I have never managed to poison more than one dog with morphia in my whole life, and I have tried with a large number.

16431. Does it require a very large dose to be fatal?—A very large dose.

16432. More so with a dog than with other animals?—I believe so. A dog is extraordinarily insusceptible to the toxic effects of morphia.

16433. Does it make any difference whether the morphia is injected or given by the mouth?—I have never tried by the mouth. I have always injected it hypodermically. It would make a difference, because everything given under the skin acts more quickly and is more drastic in its action than when it is given by the mouth; it is absorbed so much more quickly.

16434. (*Mr. Ram.*) In those cases in which you experimented yourself, and of which you took notes of consecutive cases of dogs under chloroform, did you say that there were only five deaths in 1,200 cases?—That is so.

16435. And did you in that series of experiments derive any knowledge as to the length of time for which it is possible to keep dogs under anæsthetics?—Yes.

16436. You have tried that?—Yes. Most ordinary operations can be done within half an hour quite readily, but a number of operations took me over an hour.

16437. And you had no difficulty in anæsthetising the animals?—I had no difficulty in maintaining the anæsthesia at all. I did keep several dogs under anæsthesia for over two hours, and one for five hours.

16438. You have known a case of a dog being under anæsthesia for five hours?—As a matter of fact, relays of students kept it under for me, but I was about, and knew that the animal was anæsthetised the whole time.

16439. During the whole of that time was the dog insensible to pain?—It was.

16440. We have been told by one of our earlier witnesses that the horse is not a good subject for anæsthetics, and therefore that many people are afraid to have valuable horses anæsthetised when they are castrated, or otherwise dealt with. Has that been your experience?—That has not been my experience.

16441. With regard to either a horse or a dog, do you find after ill effects in consequence of the anæsthetic—sickness or trouble of any sort?—It is most

rare to get sickness. The animals are stupid and dull for perhaps an hour, or it may be a couple of hours. A sensitive dog will be stupid for a couple of hours.

16442. Seedy?—Yes, it will be seedy because it will not feed. Supposing I operated this afternoon on a little dog, I should not think anything if my nurse told me to-morrow morning that the patient was not taking its breakfast, but at the same time I should know that it was rather against the rule. The next day I should expect it to take its food.

16443. Is a dog sick after anæsthetics?—It is most rare.

16444. Is it possible for a horse to be sick?—Only under certain diseased conditions, for instance, when it has a ruptured stomach, or when it swallows hawthorn berries. You cannot make a horse sick if you try.

16445. It is not subject to any emetic?—No, you cannot make a horse sick.

16446. Have you ever seen any animal under curare?—No.

16447. (*Dr. Gaskell.*) I should like to know, please, how you know when your animal has had sufficient anæsthetic not to feel any pain?—By its irresponsiveness to a severe stimulus.

16448. Responsiveness of what kind?—Either by a cry, by an expression of pain in that way, or by shrinking away from the stimulus.

16449. But might not movement take place?—When an animal is under anæsthesia movement is always taking place; the mere fact of the respiratory muscles working draws the legs backwards and forwards, and frequently the legs are quivering a little.

16450. You very frequently find in animals that you do get movements of the legs more or less synchronous with respiratory movements?—Yes.

16451. That is common under anæsthetics?—That is common under anæsthetics if the animals are not strapped down tightly.

16452. But that is no sign of pain?—That is no sign of pain.

16453. And if you have a dog chloroformed sufficiently to remove pain, can you tell us at all how far that dose is removed from a dose which would kill?—In figures do you mean?

16454. No, I mean in your general impression is there a large margin between the two things, or is it near?—There is a margin; when a dog has been under proper chloroform anæsthesia for some few minutes it is often very difficult with the apparatus which we use to give it a lethal dose. When once the system has become tolerant to chloroform, and it is still given in a rational manner with the proper percentage of 9 to 2 per cent., it is extremely difficult. It will often take half an hour to finish it off.

16455. The danger then is at the beginning of the administration?—The danger is at the beginning. I am quite satisfied about that, and if the anæsthetic is given in an excessive dose at the beginning, your dog goes under quickly, literally like the snuff of a candle, but when once I get the animal tolerant to it I can let my kennelman or groom come and give the chloroform, and I can attend to the operation. I know I am all right.

16456. Are you right in thinking that the danger at the beginning is due to an overdose?—I think I am. I think it is due to asphyxia.

16457. Have you read a paper by Martin and Embrey in the "Physiological Journal"?—I have not seen that paper. I would like to read it through.

16458. It is to the effect that the first administration of chloroform, especially to an animal like a dog, causes a slow stoppage of the heart through the action of the vagus nerves. That is well known, of course?—Yes.

16459. And that then if the animal is apparently dying, if you cut the two vagi nerves it will recover, and you can go on with the administration of chloroform quite happily?—That is interesting. I have not seen the paper, but I will read it. The other is only my own idea, of course. I am not a skilled physiologist.

16460. The danger is at the beginning, not necessarily because you have given too much chloroform, is it, but because of the action of the chloroform in the medulla oblongata at first?—I am only speaking as a clinician, and my experience is that the danger is only at the beginning.

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16461. That is also the physiologists' experience, I may say. When you say that it takes a large dose of morphia to kill a dog, you mean a dog as compared with other animals?—Yes.

16462. Would you consider that morphia is a narcotic to a dog?—Yes, I do consider it a narcotic.

16463. Do you use it in your practice in the same way as medical men do in their practice with human beings for allaying pain?—I use it largely for stopping pain.

16464. And you find no difficulty?—I find no difficulty in the dosage.

16465. Do you find any difficulty in stopping pain?—Sometimes a dog does not respond as I should expect it to do.

16466. Is that a question of dosage, or is it a question of idiosyncracy?—I think it is a question of idiosyncracy, but I am not prepared to give a definite opinion upon that point. I begin to think it must be an idiosyncracy.

16467. (*Chairman.*) Of all dogs, or of certain dogs?—Of certain dogs. I am speaking of dogs alone.

16468. (*Dr. Gaskell.*) I think you may take it as a rule that morphia, except in very large doses, will act in much the same way on a dog as it does on a human being; it will cause absence of painful sensation, I mean?—I presume so; I have that impresion, but I have no experience of it in human beings, except that people have told me that they feel comfortable after it, and dogs certainly give me the impression that they are comfortable after it in the majority of instances.

16469. Have you any experience of such an anæsthetic as chloral with dogs?—I cannot say that I have used it sufficiently to form an opinion.

16470. Or urethane?—I have not used it at all.

16471. Practically you administer chloroform?—Chloroform, the A.C.E. mixture, cocaine and the different local anæsthetics.

16472. (*Dr. Wilson.*) Do you ever use morphia before administering chloroform in an operation?—I have sometimes done so. It is not my general practice to do so, but I have done so at times.

16473. I think you said in answer to a question which was put to you, that you prefer to use chloroform because animals keep far more still during the operation than when you use the A.C.E. mixture?—They keep far more still for a longer period than when I use the A.C.E. mixture. When once an animal is anæsthetised by chloroform I know that my anæsthetist need not keep pressing the bellows so frequently as with the A.C.E. mixture. The animals waken up much more quickly from the stage of unconsciousness when the A.C.E. is used than they do with chloroform; that is to say, the stage of anæsthesia with the A.C.E. by some few seconds, or minutes probably.

16474. You have no experience yourself, of course, but would you conclude that if the A.C.E. mixture was used for keeping up artificial respiration, through a tracheal tube, for example, in preference to chloroform, there would be more risk of the animal becoming conscious to pain during a prolonged operation?—Not at all if the anæsthesia is kept up. It requires more pumping of the apparatus to keep the animal deeply anæsthetised with A.C.E. than it does with chloroform alone, and in the same way with ether, it requires more pumping than with chloroform alone. The stage of anæsthesia with chloroform is longer, that is to say, you can cease pumping chloroform for a longer interval than you can with either of the other two.

16475. Do you think it requires more careful supervision when using A.C.E.?—I think it requires less supervision than using A.C.E. or ether, but A.C.E. particularly. I do not use ether much because it sickens the dog so afterwards.

16476. Are your dogs so completely muzzled that they cannot cry out during the operation?—If they are under chloroform they cannot cry out. Do you mean when they are going under?

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16477. Yes, you say that you use a stimulus to test whether they are sufficiently under chloroform?—I do not say that I do now, but I have done so. They would have no muzzle on at all.

16478. The response to the stimulus would be a cry?—Probably, or the retraction of a limb.

16479. Then when you are operating on them there is nothing to prevent their crying?—Absolutely nothing. If they could not cry they could moan. They just have their heads in the mask and there is nothing to stop the jaws from opening, or they might have a piece of tape round the nose. We always put a piece of tape round the nose when we are handling dogs as a matter of precaution, because however fond a dog is of you, if you do something to it that it does not quite understand it may take hold of you, and frequently will have hold of you.

16480. But supposing you used a wire muzzle instead of tape?—You could not get a wire muzzle under the mask. Besides, I do not like a wire muzzle, it is not a safe thing to use.

16481. But you would not use morphia alone without either the A.C.E. mixture or chloroform for an operation, would you?—I should for certain minor operations. For example, the operation of removing a very small tumour or removing a small elliptical piece of skin from the eyes, which is very common in certain breed of dogs with certain diseases.

16482. I was referring to a rather serious operation?—No, not for serious operations. I should give either the two or chloroform itself.

16483. (*Sir William Collins.*) Did I rightly understand you to say that dogs are insusceptible to morphia?—They are very insusceptible to a toxic dose of morphia.

16484. Did I rightly understand you to say that you had not succeeded in poisoning a dog with morphia?—I have only succeeded in poisoning one.

16485. Have you made many trials?—I use it largely. I have injected it largely either before chloroforming or I have tried on several occasions, when the owners have thought morphia a humane method of putting a dog away. I have given large doses; I have given as much as 20 grains, and to my astonishment, because I did not know that the dog was so insusceptible as that to a toxic dose, the dog has been alive the next night, 24 hours afterwards.

16486. Is it an easy thing to give a lethal dose of morphia to a dog?—Personally I do not think it is. Some dogs will become perfectly comatose with a grain, with a small amount. The other day I had a small animal; I gave it, I think, two grains, but it acted on that animal just as much as I should have expected four or five grains to have acted on an animal of the same size. The idiosyncrasy of that particular animal was very great towards it. Frequently half a grain will produce a stupor in one animal when it will take a full grain or a grain and a half to produce it in another animal of the same size.

16487. Then morphia affects different dogs differently?—It does.

16488. Can you not regulate the necessary dose by reference to the weight of the dog?—Generally you can, but you never can be quite certain.

16489. I thought you suggested just now that the weight was not a guide?—Not to the toxicity.

16490. Or to the effect?—I say it is not a certain guide.

16491. You told us that you have given chloroform to dogs some 1,200 times?—Literally thousands of times. I kept notes of my first 1,200 cases.

16492. As an anæsthetic?—Yes, as an anæsthetic.

16493. How many times have you administered morphia to dogs as an anæsthetic?—I should say pretty nearly a hundred times.

16494. For considerable operations?—Not for major operations.

16495. Why not?—I have done so and given chloroform afterwards.

16496. But have you used morphia alone as an anæsthetic for major operations?—No.

16497. Why not?—I do not use it. I prefer chloroform. I always use chloroform.

16498. Why do you prefer chloroform?—Because, for one thing, I am in the habit of using chloroform, and have been for many years, and I like chloroform.

16499. Is it because chloroform is a better anæsthetic?—Undoubtedly, for major operations.

16500. (*Dr. Gaskell.*) In those cases where you have used morphia, do you consider that the animal has not felt pain during the operation?—I do not say that. I mean that I should not use morphia for any major operation.

16501. But in cases where you have used morphia, have you felt satisfied that the animal has not suffered pain?—No, I cannot say that. I mean just at the very instant. If you take that eye operation that I was speaking of, the snip is done instantaneously, but my animal will flinch, although it passes immediately into a state of dreaminess again, and does not appear to suffer, but just at the instant of the snip, the eye being, of course, a very tender part, it will just flinch, perhaps.

16502. (*Sir John McFadyean.*) What was the condition of that dog which survived a dose of 20 grains of morphia?—Carcinoma.

16503. But what was the condition of the dog two hours afterwards?—Absolutely dead to the world, literally.

16504. Do you think it was conscious to pain?—No; I could make it respond by perhaps lifting the foot up and dropping it down again; I forget what I actually did, but I know when the nurse went to it she patted it or did something, and it just lifted its head for a mere fraction of a second, showing that it did know her for a second, but it could not recollect.

16505. Was it susceptible to such stimuli as you have mentioned?—Yes.

16506. Have you had any other case approaching it in which a dog would stand 20 grains?—No, not when I have given as much as that. I have given 16 grains, though, once before, and nine and ten grains several times, and the animals did not succumb.

16507. So that you think it might be pushed so as to obtain its full effects as an anæsthetic without much risk of killing the dog?—That is my experience. I have been astonished at it.

16508. (*Sir William Church.*) Have you experience of laudanum in dogs?—I give it sometimes.

16509. Does it fail to act in the same way; are dogs tolerant of a large dose of laudanum?—My experience is that they are very tolerant of a large dose of laudanum.

16510. Have you any idea what would be a lethal dose for a moderate-sized dog?—No, I have not any idea whatever.

16511. Is it the same with cats?—I have no experience of its administration to cats, and I have not been able to find any experience.

16512. (*Chairman.*) You hold a licence, I suppose? I have not had a licence for seven or eight years now. I just held one whilst I was at the College. I got it for the purpose of doing this work with anæsthetics.

16513. You have held a licence?—Yes.

16514. And you know that if you hold a licence, and if you perform a cutting operation on an animal, it comes under the Act?—Yes.

16515. Are there many operations which you perform on a cat, or a dog, or a horse—I mean which would be a patient—without an anæsthetic, for which you would be obliged to use an anæsthetic in case of an experiment?—I do not quite understand the point.

16516. Are there any cases in which you operate on an animal as a patient without anæsthetics?—Yes, plenty of cases. I never give an anæsthetic for a mere lancing of an abscess. There are plenty of instances in which you cannot give an anæsthetic.

16517. I was asking you about tracheotomy. If you were going to cut an animal's throat for the purpose of putting in a tube, would you for that preliminary operation, in the case of an ordinary animal patient, use anæsthetics?—Yes, unless the patient was at the point of death. Tracheotomy is an operation that we have to do in a hurry sometimes. If I was doing tracheotomy on a hunter that is a roarer and must be made useful, I should give cocaine. I should not give chloroform, because cocaine answers the purpose,

It is a mere prick of a needle to begin with, that is the only pain that the animal feels.

16518. If it was at all a serious operation you would give chloroform?—Yes.

16519. But there are a good many cutting operations which you would require leave under the Act to perform, and would only obtain leave to perform with anæsthetics, which you would perform on a patient without anæsthetics?—Yes, I expect that I should. I do not use anæsthetics for every little thing. For one thing, there is not time, and for another thing, I do not consider that the inconvenience which the patient is put to for the time being in suffering afterwards from the anæsthetic is worth it, because the operation is done so quickly; but for any major operation I should always use anæsthetics.

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16520. (*Sir John McFadyean.*) Have you ever fired a horse in full consciousness?—Yes.

16521. Have you done it a thousand times?—Not quite that, but I have done it.

16522. Do you do it as a rule without anæsthetics?—Frequently.

16523. Have you any doubt as to its being an enormously painful thing?—It is a painful thing, but I generally use an anæsthetic, I generally use some cocaine. At the same time, I do not always do so.

THIRTY-SEVENTH DAY.

Wednesday, 20th November 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, M.D., LL.D.
Capt. C. BIGHAM, C.M.G. (*Secretary*).

In the temporary absence of Viscount Selby, Sir William Church took the Chair.

Mr. W. OSLER, M.D., F.R.S., F.R.C.P., called in; and Examined.

16524. (*Sir William Church.*) You, I think, are Regius Professor of Medicine at the University of Oxford?—Yes.

16525. You are a Fellow of the Royal Society, and a Fellow of the Royal College of Physicians, London, and you have been, I think, Professor at the Johns Hopkins University, Baltimore?—Yes.

16526. You wish to make a general statement showing that medicine offers no exception to the other sciences?—I put down very briefly these statements in my *précis*, particularly with reference to one or two points that perhaps have not been dealt with by other men who have already given evidence.

16527. You have read the evidence before the Commission?—Not all of it, but that of Professor Gotch and one or two others.

16528. Have you read Professor Starling's evidence?—Yes, Professor Starling indicated to me particularly the lines on which I, as a practical physician, could perhaps best give evidence, and I put down here these one or two points which perhaps have not been, as was thought, fully dealt with by others.

16529. You agree with the general line of the medical and physiological evidence, I suppose?—Fully, particularly with that of Professor Starling, the President of the Royal College of Physicians, and the President of the Royal College of Surgeons, whose evidence I have read.

16530. You would like to add a few points?—Yes, particularly these points that I have referred to in my *précis*. I do not think it is worth while to speak in regard to the influence of experiments on animals or in regard to the way in which we have learnt the etiology of acute infections through those means. But I think that the story of yellow fever illustrates, perhaps, more satisfactorily than any other, the remarkable way in which experiments, carefully devised and carried out, may influence not only our knowledge of the etiology of a disease, but may influence extensively the commercial relations of nations, and save not only thousands of lives but millions of pounds annually. Would you like me to speak of that particularly.

16531. If you please?—Yellow fever has been the great scourge of the regions round the Carribbean Sea, more particularly Mexico, the West Indian Islands, Brazil, and every few years it has spread into the Southern States of America, and occasionally has reached Philadelphia, and even as far north as Boston. In the early part of the last century on several occasions it reached Europe, and there were extensive outbreaks in Spain, costing some thousands of lives. Many attempts had been made to find out the cause of the disease, but all had failed up to the year 1900, when a Commission was sent to Havannah by the United States Government especially to investigate the cause of yellow fever. That Commission, composed of Drs. Watten Read, Carroll, Lazlear, and Agramonte, recognised particularly the relations of the mosquito to the disease, and they went out with the specific object of determining, if possible, to discover the germs of the disease. The experiments which they devised were carried out in a United States Army Camp in Havannah, and they are among the most remarkable that have ever been made. The camp was entirely isolated, so that there could be no possibility of communication with the outside. It was composed of a certain number of immunes, that is to say, persons who were no longer susceptible to yellow fever.

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16532. (*Mr. Ram.*) In consequence of having had it, do you mean?—In consequence of having had it—and of non-immunes. That is a common division of the population in Brazil and Havannah—namely, into immunes and non-immunes. A man is asked whether he is immune or not.

16533. (*Sir Mackenzie Chalmers.*) Does one attack confer immunity as a rule?—One attack confers immunity. In this camp a house was constructed with two compartments, divided from each other by a wire mosquito-proof screen. There were two sets of experiments made in connection with this little house. In the first place, into one side of this hut 15 infected mosquitoes were placed. Those were mosquitoes that had bitten a yellow fever patient within the first three days of the illness. Men were selected, partly from the Army and partly from civil life, who had expressed and signed their willingness to submit themselves to experiments. I may state that one or two of the medical men also volunteered. Into the compartment with the 15 mosquitoes a non-immune went in the morning, in the afternoon, and on the following morning and submitted himself to the bite.

16534. For an hour, or something of that kind?—I do not know how long, but long enough to get a number of bites. Within five days he had the disease. At the same time in the adjacent compartment, which was simply screened from these mosquitoes by a wire

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netting for 21 consecutive nights two non-immunes slept. They did not get the disease.

16535. Were any precautions taken to keep the ordinary mosquitoes from them?—Yes, of course; the compartment was double-doored and very carefully screened so that there was no possibility of that. I may say that experiments of that nature were repeated on several occasions, demonstrating quite conclusively that, so long as these infected mosquitoes were kept from biting, though there was only a screen between them, the individuals did not get yellow fever. Then experiments were made on a very extensive scale to determine whether the disease was conveyed by means of fomites, that is to say, whether, as was usually supposed, the disease was carried by infected clothing and by the excreta of the patients, and by the vomit. For that purpose the clothing and material soiled by the vomit and by the blood, and by the stools of the patients were placed in one of these rooms, and a group of non-immunes slept in contact with this clothing, in some cases between the actual sheets of the beds in which these patients had died, for 21 consecutive nights. That experiment was repeated with a second set of non-immunes sleeping, as I say, with the bed linen and with the soiled materials of patients who had died of the disease. Not one of them took yellow fever.

16536. (*Sir William Church.*) What were the numbers of the non-immunes that slept in connection with the fomites?—I think two or three, two soldiers and one surgeon, for 21 consecutive nights; and a second party for the same period. Then these men were subsequently experimented upon by placing them in the section of the house with the infected mosquitoes, and in each instance they took the disease. Altogether 22 soldiers subjected themselves to the experiments and 22 took the disease; fortunately none of those cases proved fatal. One fatal case was a former assistant, Dr. Lazear, who had been for several years in charge of my clinical laboratory. He submitted himself to the bite of an infected mosquito, and three days subsequently developed the disease and died.

16537. (*Dr. Wilson.*) May I ask what interval there was between the exposure to the fomites and individuals being put into the place with the infected mosquitoes?—Some days, possibly weeks' intervals; I cannot say exactly.

16538. (*Sir Mackenzie Chalmers.*) What is the incubation period of yellow fever?—From three to five days. The mosquito to become infective must bite a patient within the first three days of the disease.

16539. (*Mr. Ram.*) Of its having been in contact with the disease, do you mean?—I mean that the mosquito must bite the yellow fever patient within the first three days of the patient having the disease. The mosquito itself is not infective under a period of 12 days; the mosquito may bite an individual anywhere up to the twelfth day after receiving the infection without being infective; then it remains infective all through the rest of its life. Of course the interesting practical point comes out, that this series of experiments has already revolutionised life in those regions. Havannah within the next two years was cleared of yellow fever, the first time in the 300 years of its existence. The French Academy sent a Commission to Brazil to study the disease, and they have reported in harmony with the American Commission—namely, that the disease is transmitted by the mosquito, and by the mosquito alone, and only by a mosquito that had bitten yellow fever patients within the first three days, and that the mosquito did not become infective until after 10 or 12 days subsequently.

16540. (*Sir William Church.*) I think in yellow fever the transmission of any organism from the mosquito to man has not yet been followed out?—No, we do not know the organism; but it must be a protozoon, possibly a spirochoete, which undergoes a slow evolution in the body of the mosquito.

16541. But nothing has hitherto been discovered comparable with the plasmodium of malaria?—No, but it is possible that it is a very minute spirochoete, which passes through an ordinary filter. This is the kind of discovery that will revolutionise conditions of life in the tropics. The discovery of the malarial parasite and the discovery of relations of yellow fever with the mosquito will enable the Panama Canal to be built. Without those two investigations the probability is that it could not be built.

16542. (*Mr. Ram.*) Or if built, would cost a tremendous sacrifice of human life?—It would cost an enormous sacrifice of human life, just as happened with the French. Now there are 20,000 whites on the Isthmus at work; of course, nearly all of those are non-immune. There has been practically no yellow fever, and what is much more important, because it was not the yellow fever that killed the French to the same extent, there is no malaria.

16543. (*Sir William Church.*) In these experiments that you have been detailing to us, animals do not come in; the animal experimented upon was man?—That is so, only as man is an animal. I am referring to those experiments only as an illustration that it is through the experimental side of medicine, the experimental spirit in medicine, that these great revolutions have been effected, revolutions with which there is nothing else in human endeavour to compare from the standpoint of humanity. There is not anything else in the whole development of the British nation that is going to have so much importance as the discovery of the mode of transmission of malaria. It is going to make the tropics habitable. And all this has come about through the experimental method and the experimental spirit. Without these such investigations could not have been made, and these perfectly phenomenal results could not have been achieved. It was the same spirit that gave us anæsthesia, and the same spirit that has given up antiseptic surgery, and the same spirit which has given us preventive medicine—three things which stand out in the record of human achievement, with which nothing else may be compared—I mean from the standpoint of everyday, common humanity.

16544. Then your contention would be that this experimental investigation into the interaction between the mosquito and man producing yellow fever would never have been thought of if it had not been for previous experiments on animals?—Never. The men who made these investigations spent their lives in laboratories, and their whole work has been based on experimentation on animals. They could not otherwise, of course, have ventured to devise a series of experiments of this sort.

16545. (*Dr. Gaskell.*) Could you experiment with yellow fever on animals?—Yes, recently Wolferston Thomas, from the Liverpool School, working in Demerara, I think, has shown that it is capable of transmission to one of the higher anthropoid apes.

16546. Do the natives take yellow fever?—Yes, everywhere; but they often have it in such a mild form, and have it as children.

16547. Still, even in that mild form, the mosquito would transmit it?—Yes.

16548. (*Dr. Wilson.*) I understand that when the mosquito has once bitten a patient suffering from yellow fever, after the first 10 or 12 days it remains capable of transmitting the disease during its short life?—Sometimes it is a long life comparatively—many months.

16549. Has that been determined?—Yes, it will live through many months; it will live through the winter.

16550. As the fever has disappeared in Havanna may I ask whether the special stegomyia mosquito has also disappeared?—No, they have made an active warfare against it, but I think the mosquito itself has not disappeared.

16551. But if the mosquito is capable of transmitting the disease during its existence, after it has bitten a patient suffering from the disease, how can you get rid of the disease, so long as the mosquito lives?—That is only a matter, I think, of 8 or 9 months. I do not know definitely what the life of a mosquito is. I do not think it is more than over the succeeding winter. What do you say, Dr. Gaskell?

16552. (*Dr. Gaskell.*) I should think not.

16553. (*Sir William Church.*) Is there anything further you wish to add about yellow fever?—No, I think not. The work in Brazil lately has shown that even in large centres, like Rio, by the mosquito brigades thoroughly carrying out measures such as were introduced in Havannah by Gorgas, the reduction in mortality and in the number of cases has been very striking.

16554. By taking steps to prevent the breeding and multiplication of mosquitoes?—Yes, and by carefully reporting and carefully isolating the very earliest cases I look forward to the total abolition of yellow fever within five years, and that, from the commercial standpoint alone, will revolutionise the trade of those districts. Every few years that a terrible epidemic arrives and spreads New Orleans and the Southern

States, and the commerce of the whole country has been paralysed for long periods.

16555. (*Sir Mackenzie Chalmers.*) How do you account for the epidemic's rise and fall, because the mosquitoes were always there, and the yellow fever on which they feed is always there?—That is the difficult point; it is not easily explained. It depends very much upon how large the immune population is. The greater the proportion of non-immunes, the heavier will the epidemic be, because in a city like Havanna every non-immune within a year of his arrival there took the disease—he could not escape.

16556. (*Mr. Ram.*) Now that you have discovered that the mosquito is the cause, how do you ensure protection against it?—By screening the houses properly and scouring out all the pools and protecting the water tanks.

16557. By destroying the habitat of the insect?—Yes, and, particularly in Havannah, by covering up and protecting the watertanks, and by the active energetic fight against the mosquito which has been made so successfully.

16558. Then the object is the extirpation of the mosquito?—Yes.

16559. (*Sir Mackenzie Chalmers.*) The mosquito does not fly very far, I suppose?—Some of them go long distances, particularly when carried by the wind. Then, of course, they are carried by ships, and get into the holds, and get from one country to another in this way.

16560. (*Sir William Church.*) Is it not the case that in the mosquito which is connected with yellow fever the changes from the egg to the complete insect occur very rapidly?—Very rapidly.

16561. And also that its breeding places specially are not running waters or large pieces of water, but small pools, and even the water left in hollow utensils, which are not thrown away, around private houses?—Yes. I have in my pocket a recent little monograph on yellow fever by Goldberger, issued this year by the United States Yellow Fever Institute, giving a very good account of the mosquito (*handing in the same*).

16562. (*Mr. Ram.*) Might I ask has there been at present any distinct diminution in the multiplicity or numbers of mosquitoes?—Yes, in all these regions, particularly where an active crusade has been waged, as in Havannah, in cleaning out these places where they have been breeding.

16563. They have become diminished in number?—Yes.

16564. (*Mr. Tomkinson.*) I suppose they never swarm as the midge does in this country?—Oh, yes, they do, and even in the cities, in countless myriads.

16565. Is it held to be possible to exterminate them?—Yes, I do not think there is any question about it. It has already been done in many places. Ross worked at that in Ismailia; and even in the West of India under the most unfavourable conditions, the reduction in the numbers of mosquitoes has been very remarkable.

16566. Do you suppose that if it were a fact that disease of a serious nature could be communicated by the midge in England or Scotland, their numbers could be seriously attacked and diminished upon the moors and bogs, and places where they swarm?—In the immediate neighbourhood of houses it could be done. A thing which has been demonstrated, parallel with that, is the reduction in the number of cases of malaria. Take, for instance, a place called Sparrow's Point, in Chesapeke Bay; within the last five or six years, since we have known how malaria was transmitted by the mosquito, the number of cases has been diminished almost to vanishing point there, entirely by clearing up the pools and waging war against the mosquito. There has been a remarkable reduction in the cases of malaria there.

16567. (*Mr. Ram.*) The mosquitoes, of course, have been diminished too?—Yes.

16568. (*Mr. Tomkinson.*) Are those pools the breeding places?—Yes. This discovery is making Africa, of course, habitable for the white man. Many of the young men who have been out on the recent scientific expeditions have by carefully protecting themselves lived for years there without having an attack of malaria.

16569. (*Sir William Church.*) You desire also, I think, to give some of your personal experiences in the treatment of cretinism and myxœdema?—I thought it would perhaps interest the Commission (as there are diseases in which I have been specially interested, and have had an unusually large experience), if I were to speak of them, because our knowledge of the methods of cure has come so directly from experiments on animals. We have learnt through experiments upon animals that the total removal of the thyroid gland was followed by a group of symptoms resembling cretinism and myxœdema. Gull showed us and Ord showed us that a spontaneous malady occurred in man of the same character. The surgeons taught us that occasionally following the total extirpation of the gland for goitre, the same symptoms occurred. Then for very many years—centuries, indeed—this condition of cretinism has been known in certain regions in which goitre is endemic, as in Switzerland. All of these conditions, particularly the sporadic cretinism and myxœdema, may be cured completely and permanently relieved by the administration of a powder of the thyroid gland. I do not know that there is a single step in our knowledge of this method of treatment (which in some ways is the most phenomenal that has ever been introduced) which has not been the result of experimentation on animals. If you will allow me just to show you here an illustration, striking as this is, it is even more striking in a woman. Take a woman, for instance, who, with advanced myxœdema, may be reduced to a condition of imbecility, dementia, to a simply frog-like or a toad-like caricature of her former self, and in a hopeless, helpless condition. Within six months that woman may be perfectly well. And she stays well. Here, for instance, is an illustration (*producing a set of photographs*) of what may be done in eleven months. *Here* is a typical little cretin child, between two and nearly three years of age, unable to walk, unable to talk, with a pot-belly and prominent navel, with no sign whatever of intelligence about its face—in fact, showing the typical apathetic, dull condition that characterises these cases. And *here* you see the successive changes in that child in the course of eleven months. You see I have marked it *here* after two months' treatment, after three months, after five months, after seven months, and at eleven months (*handing round the photographs*). That is practically a miracle. That child was doomed to hopeless imbecility. And I have had a whole series of such cases; eighteen or twenty in all.

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16570. (*Mr. Ram.*) Is it the diseased thyroid gland which produces myxœdema?—Failure of the development of the thyroid gland in a child, or disease of the thyroid gland, sometimes following measles, for instance. The evidence has, I think, already been presented about the steps by which our knowledge has been gained in that way, I suppose by Sir Victor Horsley.

16571. (*Sir William Church.*) It has been stated that this knowledge could have been arrived at by mere observation of persons who were suffering from disease of the thyroid gland. What is your opinion of that?—I do not think so. We were struggling helpless until Sir Victor Horsley's experimental work, by which was demonstrated the identity of the condition produced by experimenting upon animals with the condition which Gull and Ord had described told us of.

16572. (*Mr. Ram.*) When the thyroid gland in animals was extracted the animals became like that?—The animal became exactly like that. I do not think there is any question about it.

16573. (*Sir William Church.*) Could you have found out in man what was the function of the thyroid gland. Has a normal thyroid gland ever been excised in man?—I do not think so. I do not know of any condition in which actually the normal gland would be excised.

16574. Excising a diseased gland is not the same thing, of course, as excising a normal gland?—Not at all.

16575. Therefore, until a normal gland was excised it would have been impossible to determine what influence the thyroid gland had upon the economy?—Quite impossible. What I feel, of course, is that such a thing as that, such a transformation as that which I have shown to you, is such a tremendous gain to humanity (just think what it must be to an individual who had a child in that condition), that it is impossible to put against it lives of a certain number of dogs sacrificed. I do not think that the two can be weighed together. And when you think that that has been done in hundreds and hundreds of cases, and will be done, and that these people remain well, I think it

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is one of the strongest evidences in favour of the benefits which have been derived from experiments on animals that we can possibly have.

16576. (*Mr. Ram.*) I do not think we have had explained to us exactly how the treatment is applied?—It may be applied in several different ways. Usually it is applied by simply feeding with thyroid gland, a certain amount of powdered gland of a sheep or pig. But these children must take it continuously. I have had one or two instances in which the parents have said, "We do not want to keep up this treatment. I do not want to send every three months to the chemist and get this stuff; it is expensive," and the children just begin to lapse into this same condition of stupor and imbecility.

16577. (*Sir Mackenzie Chalmers.*) After the eleven months?—Yes, in one case after five or six years.

16578. You still have to continue the treatment?—Yes, you have to continue the treatment practically for life.

16579. (*Mr. Ram.*) There is something absent in the child which has to be supplied?—Yes, it is that internal secretion of this gland.

16580. And you must permanently supply it?—Yes, by this method.

16581. (*Mr. Tomkinson.*) How was it discovered—through experimentation on animals?—Chiefly through Sir Victor Horsley's experiments.

16582. But how was it discovered?—By removing the gland, in monkeys particularly, and finding that this condition was produced. I have forgotten exactly who it was that first transplanted the gland into an animal from which the gland had been removed, and then these symptoms all disappeared, showing that the function of the gland could be substituted either by a portion of the gland put underneath the skin or if the powdered gland was fed to an animal.

16583. (*Sir William Church.*) Will you now go on to the third portion of your *précis*?—That is a very important one—namely, the question of how far animals should be used to train young men in the technique of surgery. Of course, we know nowadays that surgical operations, particularly on the abdomen, have increased in a remarkable way. If you take any one of the smaller provincial hospitals, for example, the number of abdominal operations to-day is perhaps three times as great as it was 25 years ago. It is a very difficult thing to teach young men operative surgery. The usual way is to operate on the dead body. That is all very well in certain directions; it teaches the man perhaps how to make a nice stump of a limb, but he misses a great deal. He is not taught technique. It is impossible almost to teach properly the finer operations in the abdomen; I should say that it is quite impossible to do it systematically and thoroughly. A few years ago, when I was connected with the Johns Hopkins Medical School, we instituted there a course of operative surgery on animals only for a select group of senior students who wished subsequently to practice surgery; that is to say, a man had to state that he had a special interest in it, or he was not admitted to the course. A laboratory has been built, called the Hunterian Laboratory, which is in its equipment an animal hospital—that is to say, there are special rooms, and every precaution is taken to make it just as clean and as sweet and as aseptic as a hospital ward. It was very soon found—in fact, in the second course—that the veterinary surgeons and the citizens brought their animals there to be operated upon.

16584. (*Mr. Ram.*) Their diseased animals?—Their diseased animals; and this gave the senior medical students an opportunity of performing special operations under the skilled direction of one of the junior surgeons. I have brought here a paper which shows the sort of work which is done there. Here are a series of cases in dogs with tumours and herniæ, as you will see if you turn over the pages (*handing in a book*), which have all been operated upon by the senior students, and who have reported the cases. You see a whole series. These cases of hernia particularly are most interesting. I would like just to say a few words as to how the class is conducted.

16585. (*Mr. Ram.*) Is this a curative operation?—Yes, a curative operation. Two of the men who are to operate, for instance, in a case of one of these big herniæ of the dog prepare all the dressings beforehand; they prepare everything just as if it was in a hospital, and as if they were the nurses in the hospital. They prepare the animal. The animal's belly is carefully shaved, and may perhaps undergo a preliminary day or two of treatment in a case of big hernia, just as a patient would. Then the teacher gives a brief lecture, with black board illustrations, or sometimes the young man himself who is to operate has to give to his fellow-students in the class a little clinique on the hernia, what it is, and what he is going to do. Then the men proceed under the direction of the teacher to operate on the animal themselves. Of course, that is a totally different matter in the way of teaching from operating on a dead body. They have to know how to control hæmorrhage. And every single point in the technique is all done by those men themselves; and you see in those pictures what excellent results have been obtained. These are by senior students.

16586. Is the animal always anæsthetised?—Yes.

16587. (*Dr. Wilson.*) Do they operate on healthy dogs when they cannot get diseased animals?—I was just going to speak of that. A course also consists of all the important abdominal operations—short circuiting for example the intestine, or operating for the removal of the spleen or operating for anastomosis of the bowel. Those are operations done under anæsthesia on healthy animals. Then, in this Hunterian Laboratory, all experiments for the physiological laboratory or for the pharmacological laboratory are done, so that in that way the animals are there, and the results are always satisfactory, because the things are done with the greatest caution and care, and the greatest care is taken of the animals. I would like to read to the Commission one paragraph by the man who has introduced this, Dr. Cushing, because I think we have all felt there that it would be very much better if we could get animals other than the dog to operate on. If pigs were cheaper, for instance, I think they would be much better, because the intestines of the pig are more allied to those of man. But this paragraph expresses very well the feeling which I think actuates the man in charge: "There is naturally a feeling of regret in the minds of many—of none greater than our own—that animals, particularly dogs, should thus be subjected to operations, even though the object be a most desirable one and accomplished without the infliction of pain, and did expense permit, we would gladly have used animals with which there an association of less acute sentiment on the part of all." Then he states: "This feeling in the past few months has been somewhat mitigated by the fact that, learning of our work, the owners" of diseased dogs "have begun to bring these animals to us for treatment." But I do not think anyone could visit the place and see the way in which the class is conducted there without feeling that there is just the same care taken of the animal really as of a human being, both prior and subsequent to the operation.

16588. (*Mr. Ram.*) Is there any statutory regulation of experiments in America?—I do not think there is any.

16589. (*Sir William Church.*) Can you tell the Commission at all what proportion the animals that are brought to the Hunterian Laboratory for operation bear to healthy animals which are got for demonstration?—I could not say that. There is a very large number given here for this year, but how far that has grown or not I cannot say.

16590. I should be glad if you could tell us a little more as to the courses of instruction that these men receive. Do they first of all see a number of demonstrations upon healthy animals, and then, when they have received that instruction, operate themselves upon diseased animals?—An important operation, for instance, like this of hernia, would not be given to a man unless he had already a certain measure of technique, and till he had had some training. Does that answer the question?

16591. Not quite fully. I did not know whether, for instance, in training these men, they all had to perform some operation upon a normal animal in the first instance?—I do not think necessarily. What we felt was that if possible it would be very much better to send out a group of young men who wished to practise surgery, knowing all this modern technique, and knowing it thoroughly, having learnt it upon animals, than that they should go and learn that technique in the first place on their patients.

16592. Technique can be taught on human patients in hospitals, and is now so taught, is it not?—No, not the technique, for instance, of stopping hæmorrhage. Let me read to you a statement that is made here by Dr. Cushing with reference to this point, because I think it will state quite clearly these four points: "The paramount call upon the instructors, in this Listerian era, is that he shall emphasise and drill into his students, not as mere onlookers or hearers, but as actual performers, the significance of that much-abused term, surgical 'technique,' of which to-day the all-important element is asepsis—the first and the everlasting thing to be indelibly stamped on the make-up of everyone who proposes to undertake operative work, whether as a surgeon or investigator. Surgical cleanliness, which must become a reflex matter—an operator's second nature—and which, like all other reflexes, must be learned early, is necessarily disregarded in the time-honoured methods of teaching this branch." That is the ordinary dissecting room course.

16593. I was not alluding to the dissecting room course, but rather to the practice in the wards and in the operating theatre?—If you will excuse me, I think that is met by these other points: "Next in importance to the acquirement of this reflex habit of cleanliness is the ability to dissect, and to gently manipulate living tissues without damaging them so as to interfere with perfect reactionless healing."

16594. That is what I am coming to next. That appears to me to be a point in which operation upon animals is of great assistance to a surgeon—that you are dealing with living tissues, and not with dead ones. And here——?—Yes. And, again: "A third great requisite, which cannot be learned upon the cadaver, is an acquirement of skill in the proper control of hæmorrhage, from the large as well as small vessels, for the old-fashioned rough methods of hæmostasis happily are still followed by few. Nor, finally, can facility in the particular technique of visceral surgery" (surgery of the abdomen and chest) "be properly obtained through practice on the lifeless body, a fact which almost all of those who have been pioneers in these fields of work have emphasised." I may state, too, in addition, that this laboratory has been used extensively for trying new methods of surgery. For instance, quite extensive researches have been carried on to test these new methods of surgical treatment of arterial disease, particularly aneurism, and these new methods of ligaturing arteries, and of suturing arteries, which possibly may have a very important influence upon the treatment of arterial disease.

16595. I suppose in your mind the great advantage is that the student in these courses of operative surgery on animals is handling and dealing with living tissues?—Yes.

16596. I do not see much in what you have said about the technique which I should have thought a student could learn in his clinical work, and in the operating theatre on man?—Possibly; in some places where, perhaps, he is allowed to operate; but it is a very different thing to do an operation yourself and to sit in the amphitheatre, and even to be close to the surgeon and pass sponges and pass lint to him.

16597. The circumstances would be very different, would they not, in many cases in which you operate on man and on animals—for instance, the length of the mesentery and all that sort of thing in abdominal operations would be very different?—Yes, and yet, if you take one of these animals that was operated upon—I happened myself to see one of those operations on the hernia—they are very similar in the dog and in man.

16598. Could you say from your own knowledge that what is now suggested to be a regular course of instruction for those who should become surgeons has been the practice for many years among surgeons themselves in their operations?—Nearly all great surgeons, of course, have operated on animals from John Hunter's day. I should think that nearly all the great surgical procedures have been tried on animals first, just as medicines are tried on animals before they are tried on man.

16599. Should you say that the Act which was passed in this country in 1876 had any effect in preventing surgeons acting as efficiently as they otherwise might do?—I think it has hampered a number of surgeons; I think it must hamper them. I do not know how far a surgeon would be allowed to perform operations on animals for a definite purpose.

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16600. He cannot without a licence?—But even with a licence, with the definite purpose of testing some surgical procedure which he desired to do. Do you say he cannot?

16601. (*Sir William Collins.*) For manual dexterity the Act forbids it?—Yes, but not simply for manual dexterity, but if, for instance, he had some new device like Hunter's device for curing aneurism, could he do so?

16602. It would be practically part of a scientific investigation, I apprehend?

16602A. (*Sir William Collins.*) For manual dexterity—Sir Mackenzie Chalmers will correct me if I am wrong—that you cannot get a licence for acquiring manual dexterity, but if the experiment is for a scientific purpose, like some new method of tying an artery, you can.

(*Sir Mackenzie Chalmers.*) Wherever there is an increase of knowledge to be gained by experiments you can get a licence.

(*Witness.*) Then that would cover it.

16603. (*Sir William Collins.*) There is no Federal law on the subject of vivisection in any of the States, is there?—I do not think there is any Federal law.

16604. In none of the States, either, is there such a law?—No.

16605. Do you think that experimental investigation prospers more under that system than it does under ours?—I do not know that it prospers more, but it is hampered less, I think; there are fewer vexatious restrictions.

16606. Having had experience of both systems, would you suggest that we should follow the method of the States?—Personally, I feel that the matter could be left safely in the hands of the men who are in charge of the physiological laboratories and the scientific men of this country. I feel, of course, rather strongly; I would not like, perhaps, to express my feelings as strongly as they exist on what I regard has been in some ways a standing insult to the humanity of these men. They have been hounded by a great section of the public in a way that I think is disgraceful to the English people. These are men who have lived lives of devotion and self-sacrifice, and belonging to a group of men whose service to humanity has been so incalculable that they ought not to be treated in the way they have been.

16607. You think that we might trust implicitly to the humanity of the physiologists?—Absolutely. I know these men; they are just as humane as any other men; and to place these vexatious restrictions upon them is an insult, I think.

16608. Referring to the special cases which you have brought under our knowledge, I understood that in the case of yellow fever the recent experiments have been on man?—Yes, definitely, with the specific consent of these individuals, who went into this camp voluntarily. They were volunteers, just like "forlorn hope" volunteers.

16609. We were told by a witness yesterday that, in his opinion, to experiment upon man with possible ill-result was immoral. Would that be your view?—It always is immoral, without a definite, specific statement from the individual himself, with a full knowledge of the circumstances. Under those circumstances any man, I think, is at liberty to submit himself to experiments.

16610. Given a voluntary consent, you think that entirely changes the question of morality or otherwise?—Entirely.

16611. There were a good many experiments upon animals in regard to yellow fever prior to these experiments that you have told us of, were there not?—Very many; there were, of course, the experiments by Sternberg and Sanarelli.

16612. Did Sanarelli claim to have discovered the cause of yellow fever?—Yes, a special organism.

16613. I see in Mr. Paget's book he states: "In 1896, at Flores, Sanarelli discovered the baccillus icteroides; and by October, 1897, he had prepared an immunising serum, which was able to give a considerable amount of protection to animals." "Furthermore, Sanarelli was able to show the preventive value of the serum" in a case, I think, at San Carlos, and Paget says: "Every

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prisoner, except one who had already had the fever, was therefore given the preventive treatment. At once the outbreak stopped; no more cases occurred, though only a weak serum was used, though the state of the prison and its occupants was unhealthy, though the fever, two months later, was still raging round the prison in the town"?—I do not think anybody places much reliance on that. I think Sanarelli's work has not been substantiated.

16614. I see in the paper by Joseph Goldberger that you have just handed in to the Commission, on page 7 there is a heading: "Experiments to show that the Bacillus icteroides Sanarelli stands in no causative relation to yellow fever"?—That was the first work that the American Commission did.

16615. Then if the bacillus of Sanarelli stood in no relation to the cause of yellow fever, I apprehend that this argument of a protective serum falls to the ground?—Entirely. I think it is not used at all.

16616. Did not F —— also describe an organism as the cause?—No, Sternberg.

16617. And did not Durham and Myers also describe another bacillus as the cause?—I do not know that they described it as actually the cause. Sternberg's organism was a mere suggestion. I do not think it was definite.

16618. I see on page 236 of Mr. Paget's book he refers to the work of Myers and Durham, and states that "The Report gives evidence that the disease is due to a bacillus, which is not the bacillus icteroides"?—Yes.

16619. Is this bacillus of Myers and Durham to be dismissed as also non-causative?—I think it is probably a spirochoete like the protozoal parasite—not an ordinary bacterium.

16620. Then I see "A later Commission," according to Mr. Paget, "to New Orleans, September, 1901, to January, 1902, reported an extensive series of investigations, which seem rather to support the belief that the bacillus icteroides is the cause of the disease." Were those investigations on animals?—I do not know, but I think these more recent observations have shown that we do not know the nature of the parasite.

16621. Then all theory and practice based upon a bacillus as the cause of yellow fever we may now dismiss as erroneous?—It is, in all probability, a spirochoete, not an ordinary bacterium. That that seems most likely is shown by the similarity to the cycle of development of the mosquito in malaria taking a specific period of time. It takes 12 days in the body of the mosquito before the poison becomes active. It is very unlikely when you judge from analogy of the filaria of the mosquito parasite, that that is anything but a living organism that passes through a definite cycle, just as the malaria parasite does.

16622. But morphologically are we able to find the cause of yellow fever?—No, but that is not surprising, when you remember that only quite recently the spirochoete of syphilis has been discovered. Probably it is an exceedingly minute organism.

16623. Is the method whereby you think that yellow fever may now be abolished that of immunising the community or destroying the mosquito?—Destroying the mosquito and isolating all early cases.

16624. By isolation and by the destruction of the mosquito?—Yes.

16625. I rather gathered that you considered that the case of the use of the thyroid gland for the purpose of the cure of myxœdema and cretinism was one of the strongest examples to be cited in favour of vivisection experiments on animals?—Certainly.

16626. Those of us who have witnessed the changes resulting in cases of myxœdema and cretinism as the result of feeding, or in the early days grafting the thyroid, cannot fail to be struck with the enormous advance that has been made?—Just so; and the cases were recognised as not uncommon. There were two patients, for instance, who had become hopeless dements following total removal of the thyroid gland, and who had been in that condition for years. At once, following Sir Victor Horsley's work and the discovery of the fact that the thyroid extract was potent, these patients were given it, and after many years of almost complete imbecility they were cured.

16627. Should I be wrong in thinking that prior to Sir Victor Horsley's work there had been a good deal of clinical work?—There had been a good deal of clinical work by Gull and Ord, and the men of Clinical Society's Committee here in this country. The disease of myxœdema was well known, and cretinism was well known; but there was no suggestion that it could be treated until the results of Sir Victor Horsley's work were known.

16628. Are you familiar with a paper by Curling upon the subject, published in 1850 or thereabouts?—Familiar at long range; I have seen it.

16629. Did he suggest an association between the absence of the thyroid gland and the presence of cretinism?—I think he did. I have referred to the paper several times, but it is some years ago.

16630. Do you remember one by Hilton Fagge in the seventies, I think?—Yes; but, of course, Gull's paper and Ord's work, and particularly the demonstration here in 1881 at the Congress of that remarkable group of cases, more than anything else stirred up the profession to the recognition of myxœdema as a special disease. On the Continent, before that, it was not recognised at all.

16631. Does not Hirsch in his "Historical and Geographical Pathology" cite a good deal of literature pointing to an association between cretinism, goitre and disease of the throat?—Between cretinism and goitre, that was long recognised, and was of course obvious, as so many of the cretins had a great big goitre. But the connection between myxœdema and goitre, and the connection between myxœdema and cretinism was not recognised until after Sir Victor Horsley's investigations. It was not really until Sir Felix Simon made the suggestion that these three groups of cases—cretinism, both endemic and sporadic, the myxœdema cases that came on spontaneously, and the operative cases that succeeded extirpation of the gland by surgeons were really one and the same disease, and all that came from loss of function of this thyroid gland.

16632. I ask you whether Hilton Fagge and Curling, prior to Ord and Gull, did not associate the absence or disease or defect of the thyroid with the cretinoid condition?—Yes, but it was long before then. I think that it was known that cretinism and thyroid diseases were associated.

16633. That they were a pair of allied diseases?—Yes, the cretinism and goitre had a definite association. That was manifest, of course, in Switzerland and in Savoy. I think all of those French and Italian Commissions lay great stress upon it. Whether Hilton Fagge suggested the connection with myxœdema I do not know.

16634. The name myxœdema was first given by Ord?—Yes.

16635. Did not Fagge suggest that the presence of goitre might be protective against cretinism?—I do not know. I would not be certain.

16636. In regard to the Johns Hopkins' Hospital at Baltimore, you would advocate the introduction of that system, I gather, into this country?—Under certain conditions. I think, for instance, that if certain of our senior students who were to work at surgery could be taught it on diseased animals at the Brown Institution or at the veterinary college, it would be of great advantage.

16637. Is there any proposal on foot at Oxford to introduce the Johns Hopkins' system there?—There is no practical medical school at Oxford. The men are only taught the scientific branches.

16638. You have called attention to an article by Professor Harvey Cushing in which he expresses regret that it has been necessary to employ the dog, and even though anæsthetics were used he still apparently regrets the use of the dog?—I think we all do.

16639. On grounds of sentiment?—Yes.

16640. Not sentiment in any disparaging sense, but in a lofty sense?—Yes, I think we all do.

16641. He puts in the words "did expense permit." Apparently it is a question between the sentiment and the expense?—I do not know that it is altogether. I suppose the pig would be a more suitable animal in some ways.

16642. Then apparently he suggests that the difficulty has to some extent been got over by the fact that the public bring diseased animals to the Johns Hopkins Institution?—Yes.

16643. Do you think that it would be possible to limit the system to the use of animals that are diseased

and not employ those which are healthy?—Yes, in a large city like this, when you think of the number of animals that are diseased, and that really require operation, I do not think there would be a serious difficulty.

16644. Would it necessitate keeping the animals alive after an operation?—They would be operated on to keep them alive—animals with hernia, for instance.

16645. I am afraid I have not made myself clear. I was suggesting would the scheme be practicable if animals which are suffering from disease, and possibly a disease which would require the destruction of the animal, could be employed for the purpose of operation under anæsthetics?—No. I think such animals should be destroyed without being operated on. I think it would be much more humane to destroy those animals, especially when you consider the list that is given in that article of all sorts of diseases that are curable by operation, and with the greatest relief to the poor animals, and the greatest relief also mentally to the owners.

16646. But I understand that one of the chief reasons why you advocated this system was in order to familiarise those intended to become surgeons with such operative processes as anamostasis, short circuiting of the intestine, and so forth?—Yes.

16647. Do you think that opportunities arising for performing those operations only on diseased animals would be sufficient for your purpose?—I think not, except in cases perhaps of hernia, where very often there has to be a resection of the section of the intestine that is being tied in the hernia.

16648. Do you think that those comparatively rare operations would be adequate for your purpose?—I do not know that.

16649. So that it would necessitate the utilisation of normal animals for the purpose?—Yes, if it was carried to any great extent. With a very large number of students it would, of course, be impossible.

16650. Would you advocate it for all medical students?—I would not advocate it for all medical students, but for the special group of men who wish to become practical surgeons. I think it would be very much better for these men to learn a special technique on animals, than that they should learn that technique at the risk of their fellow creatures subsequently. Take, for instance, the case of appendicitis; there is a great deal of very delicate technique often required in the manipulation of the stump of the appendix, and if that is not attended to properly the risks are considerable; and there is all that finer manipulation of such a delicate structure as the bowel.

16651. Would you advocate it in the case of all those medical students who are going to take up residence on medical appointments?—I think it very important, particularly for the resident surgeon of a hospital.

16652. For all intending to become house surgeons?—For all definitely purposing to become practitioners of surgery.

16653. (*Sir Mackenzie Chalmers.*) I want to ask you a few questions—first as regards the Act in England. The Johns Hopkins University is in Baltimore?—Yes.

16654. At any rate in that State there is no Vivisection Act?—I do not think there is such an Act in any State.

16655. At any rate there is not in that State?—I do not know of any.

16656. From your experience in the States, do you think that the consequence of there being no such Act there is a somewhat lower standard than there is in England of care for animal suffering in experiments?—I should not think so; not that I have ever seen.

16657. If you have no Act is there not the danger that while you might perfectly well be able to trust experienced medical men and experienced physiological teachers there is nothing to prevent the youngest and wildest student undertaking painful operations?—Oh yes there is. In any well-managed laboratory no student could do it.

16658. But outside the well-managed laboratories, there is nothing to prevent it?—No, but there is nothing now in this country. No Act is going to prevent a group of boys from torturing animals; they have been able to do that at any time.

16659. Except that they are punishable?—A man is punishable in any case under the Act for the Prevention of Cruelty to Animals which deals with it. There is a very active society in Baltimore, and as a rule in the United States animals are treated with just as much humantiy as they are in this country, particularly in the cities. I think in the large cities it is surprising how little ill-treatment of animals you see. I should say animals are just as well looked after there as in this country.

16660. You do not think that there is a risk of painful experiments being done without proper care and by people who are not properly qualified to perform them?—Not in well-organised laboratories.

16661. Take Baltimore for instance; would a student performing a painful operation *bonâ-fide* for some purpose of increasing his knowledge or improving his technique, be liable to prosecution under the ordinary law of cruelty?—I do not think so. I do not see under what circumstances he would do it except in a laboratory.

16662. To give a possible instance, he might try some of the, I was going to say, feeding or rather starving experiments in his own rooms?—I do not know how far he would be liable; it is quite possible he might be liable. He probably would be.

16663. Do not you think it is better that in order to perform experiments which may be painful, there should be some formality gone through to impress upon people the need of care and the need of humanity?—Yes, if you wish to act at the dictation of a group of people who distrust the greatest benefactors humanity has had.

16664. Even apart from that?—I do not know apart from that that it would be done.

16665. Now, then, coming to the use of animals to acquire technique; I suppose that a great deal of good could be done if animals were dealt with by students only for curative purposes; they would learn a good deal?—Yes.

16666. If students before obtaining hospital appointments operated upon animals merely for curative purposes, treating the diseased animal exactly as a diseased patient, there is nothing to prevent it in the existing law of England?—No, I do not suppose there is.

16667-8. Then as regards what you were saying about yellow fever, these particular experiments that you referred to were animal experiments, but the particular animals happened to be volunteer men?—Volunteer soldiers.

16669. If other animals had been available you would have preferred, of course, that the experiments should have been performed upon them?—Naturally.

16670. But as a fact, no other animals except volunteer men were available at the time?—That is so; I do not suppose the higher anthropoid apes could be had there, and it was not known that they are susceptible.

16671. And their susceptibility probably is not so great as that of man?—No; and there has only been just this one observation that Dr. Thomas has recently made.

16672. Still, there is no essential difference between that experiment and other animal experiments you would say in principle?—Certainly, given the definite consent of the individual.

16673. But I mean, putting aside all other questions, for scientific purposes this was an animal experiment?—Of course it was.

16674. How long have you been in practice? You have been in large practice as a physician, not only as a teacher, but as a practising physician?—I have been in practice for thirty-five years.

16675. You saw the introduction of what is popularly called Listerism?—Yes.

16676. You have watched the effects of it?—Yes. I started in pre-Listerian days in the old Montreal general hospital, where amputations nearly always suppurated, and a great many cases, of course, died.

16677. A great many gangrened and died?—Yes.

16678. After a severe operation in those days did you rather expect bad symptoms?—We always expected trouble after nearly every operation. A case

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that healed by first intention, we all looked at with great surprise.

16679. And now?— Now a case that does not heal by first intention we all look at with great surprise—exactly the reverse condition.

16680. Has the introduction of what is called Listerism had any effect on another troublesome thing, hospital sore throat?—It has removed all that condition which was known as hospitalism previously—a whole group of infections to which surgical cases were liable.

16681. We were told by a witness the other day who has had considerable surgical experience, that Listerism is now broken down and discredited?—Where did you produce that gentleman from—Hanwell?

16682. I will not mention his hospital, but he said that the antiseptic treatment had been absolutely discarded, and that the aseptic treatment was a reversion to the old pre-Listerian days?—It is the difference between tweedledum and tweedledee. The antiseptic surgeons practised aseptic surgery, and it is aseptic surgery.

16683. They are both applications of the same principle?—Yes.

16684. There being a slight difference in the one, due to increased knowledge?—Yes, it does not make any real difference.

16685. The witness, Dr. Bantock, also denied that the bacilli or protozoa which are found in inseparable connection with certain diseases have any causative effect, and he put his view rather strikingly. He said you might as well say that trout or salmon in a river were the cause of the river as that the bacilli or protozoa were the cause of, say, Malta fever, or malaria, or sleeping sickness. Perhaps I might read you some words of his?—I think you might spare me. I decline to listen to twaddle of that sort. I would not answer a question of that kind.

16686. I will put it in this way. In America, as well as in England, are these minute entities now universally recognised as causes and not merely concomitants?—Oh, yes, by everybody who has paid any attention to the subject.

16687. I should like to read you one or two words in which he puts his theory: "The diphtheria bacillus, again, is associated with the septic or membranous throat, the typhoid bacillus with ulcerated intestine, the cholera vibrio with the inflamed intestinal canal, and the plague bacillus with the inflamed bubo or pneumonic lung. In all these instances it is not logical to conclude that these micro-organisms, instead of being pathogenic, are playing the part of Nature's scavengers"?—He is quite entitled to his opinion.

16688. But it is not the opinion of the medical profession?—It is not the opinion of any man whose opinion is worth listening to, who has paid any attention to the subject, who has studied the subject, and who knows the conditions as they exist.

16689. Does his opinion represent anything of what might be called a school of medical thought?—Not at all.

16690. Or is it a merely individual eccentricity?—It represents the school of the back numbers.

16691. Just one casual point with regard to myxœdema and cretinism. You mentioned Switzerland. In consequence of the recent discoveries is cretinism being reduced in Savoy?—I think they have been making endeavours. There are great difficulties about it, though. They have not, I think, done as much as, perhaps, we could have expected. I cannot speak positively about it, because it is some years since I looked up the question of endemic cretinism—in fact, I know nothing at first hand of endemic cretinism.

16692. You have never seen a case of endemic cretinism treated?—No, but I think cases have been treated, and some treated successfully.

16693. Is anything known of a local cause in Savoy which predisposes to cretinism?—I do not know definitely. I think a good deal of work has been done recently.

16694. (*Mr. Ram.*) What was it that first led to the suspicion of the mosquito as the cause of propagating yellow fever?—It was recognised in the Southern States very many years ago that yellow fever was worse in the seasons when the mosquitos were abounding. A very remarkable man named Nott was one of the first to suggest that the insects acted as carriers. Then I may state also that Mr. Finlay, a Scot, who has been practising for many years in Havannah—for 25 years—has been an ardent advocate of the mosquito theory, and he deserves a great deal of credit really for having insisted that the mosquito was the carrier; and it was largely on his suggestion or based on his strong belief, that the United States Government Commission carried out this series of investigations.

16695. Are animals in the infected districts liable to attacks of yellow fever?—No.

16696. Is any animal other than the anthropoid ape, so far as you know?—We only know it of the anthropoid apes experimentally; it is known that they suffer. They do not exist in the regions where yellow fever is.

16697. I take it that this great discovery of the mosquito as the author of yellow fever, and of malaria, too, is not due directly to experiments on animals?—It is not absolutely, unless you speak of man as an animal.

16698. I meant animals other than men?—No, not the domestic animals.

16699. You told us in your evidence, and I think we quite appreciated it, that it was the research spirit to which we owe this most important discovery?—Yes, these things would not otherwise have been attempted and done. These are the descendants of men who have made modern medicine what it is to-day, and that has been altogether through the experimental spirit.

16700. So I gathered. But otherwise than that, the mosquito theory for malaria and for yellow fever is not directly due to experiments on animals?—No, not directly.

16701. With regard to malaria being carried by mosquitoes, have you been able to ascertain in that case, as in the case of yellow fever, that the mosquito must have bitten an infected patient within a certain time?—No, I do not think there is any day limit, as there is in yellow fever. The probability is that at any period in the course of malaria—that is to say, when the parasites are circulating in the blood, the mosquito may become infective.

16702. But is it your opinion that in that case also in some period, perhaps undefined as to malaria, the mosquito must have bitten an infected patient?—I think that is definite, and a special variety of mosquito; it is not any ordinary mosquito.

16703. And ultimately it is to the destruction of the mosquito that you look for immunity from these two diseases?—Yes, the destruction of the mosquito and isolation of the cases.

16704. Either destroying them or keeping them away from the human subject?—Yes, but you must have an infected patient and the mosquito to keep up the chain.

16705. I think other witnesses have told us with regard to malaria that in their opinion (and I think they said it was based on experiments), given a district in which no mosquito could get near the infected patients, given a certain time over that district, the disease might be wholly stamped out in that district even though mosquitoes subsequently existed there. Have you any evidence as to that? You follow my question?—No, I did not quite catch it.

16706. It was this: Taking a certain area at present infested by mosquitoes whose bite gives malaria, if you were to clear that district for, say six months, of all human creatures—at any rate of all human creatures that had any malarious poison in them at all—even at the end of that period, though the district might be full of mosquitoes, those mosquitoes, having lost the source of contamination, would not infect other human creatures?—No, they would not.

16707. You think that is so?—I do not think; there is not any question about it.

16708. With regard to experiments by pupils, would it be possible in the case of those experiments which you have advocated should be allowed to senior students, as to abdominal sections, and so forth (I am not speaking of curative operations, but experiments), to enact that in every case the animals shall be under a lethal dose of anæsthetic?—Yes.

16709. And that in no case need the animal for the

purpose of the experiment be allowed to live?—Exactly.

16710. Therefore would you go to the length of saying that it would not be unreasonable to enact that in every case the animal so experimented upon should be under a lethal dose?—Yes, there is no reason why that should not be so.

16711. And that would not put any fetter upon scientific experiment?—No, you do not mean animals operated upon for curative purposes.

16712. No, for experimental purposes only.

16713. (*Dr. Gaskell.*) Do you not want to keep the animal alive afterwards?—Not always. There is no reason for it except for physiological reasons.

16714. I thought for the whole purpose of your surgery it was necessary to see what happens afterwards?—It would not be absolutely necessary, to obtain the technique for instance. In doing a gastric fistula for a physiologist the animal would have to remain alive; but for securing the technique itself it would not be necessary that the animal should come out of the anæsthesia.

16715. I was thinking that it was your usual custom at the Johns Hopkins Institution to keep animals alive?—Yes, but it was not absolutely necessary.

16716. (*Mr. Ram.*) I limited my question, as you know, to experiments by students for the purpose of gaining either technical knowledge or manual dexterity?—Yes.

16717. And in neither of those cases would it, in your opinion, be a hindrance to scientific research to enact that the animals must be under a lethal dose?—No, it would be no hindrance to obtaining actual technique in the performance of the operation.

16718. You have had experience both in American laboratories, where there is absolutely no statutory enactment controlling you, and you have also had experience here in England under the statute which exists to-day Putting out of question what you were saying as to the want of trust in men of noble life, have you found that the English Act has embarrassed you at all in any of your operations?—I can only speak personally upon that. I am not a physiologist. I am a practising physician.

16719. But from your knowledge of others do you consider that the English Act, as applied to-day, is embarrassing to legitimate experiments in authorised and well-regulated laboratories?—I do not think that I know sufficiently the conditions to give an opinion about that which is worth expressing; it is only just a sort of general impression one has that a number of one's friends complain of vexatious restrictions.

16720. As to experiments by students on living animals under a lethal dose, you instance that they would be able to learn control of hæmorrhage, for example?—Yes.

16721. At present, I suppose, the control of hæmorrhage can only be practically learned by a man, whether a student or in a more advanced stage, who first operates upon a living man?—Yes.

16722. Does this control of hæmorrhage require actual technical and manual dexterity?—Yes, it very often requires a great deal of skill. But it is not so much in the control of the hæmorrhage that I should say it is necessary; but within the past 20 years you see entirely new surgical conditions have come up, particularly in operations on the stomach and intestines, and for a man who has not had a good training in technique it is a very difficult thing at first to get it. I have seen it over and over again. A man who has had technical facility and practice handles his needles and everything in a totally different way. My contention is that it would be a great deal better for the community to have a group of highly-trained young men who could have at any rate opportunities to learn that technique on animals instead of having to learn and to perfect that technique on their fellow creatures.

16723. Of course, in the absence of such technical experiments on animals, the best operator has to begin in what I may term a fumbling way?—Yes, they all begin in the same way.

16724. On the human subject?—On the human subject. It is not as if they began without seeing many men who operates. A man has seen usually scores of operations before, and has helped at them; so that if he has nimble fingers he can usually imitate the man he has seen operate before, and he gets a good deal of preliminary knowledge. He begins with opening abscesses.

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16725. There must be a first time for him?—Yes.

16726. Could not this technical skill and manual skill, which you say is so desirable, be obtained by experiments on animals, the animal in each case being wholly insensible to pain under a lethal dose of anæsthetics from which it can never recover?—Yes, in the majority of instances I think that it could. These, however, are surgical questions, and my surgical brethren will smile at my evidence perhaps. I am not a surgeon, only I have been in the habit of watching a great many surgical operations.

16727. On that, may I ask you, have you ever practised vivisection yourself?—Yes, to a slight extent years ago, when I was teaching physiology for a few years.

16728. Not since then?—No.

16729. Was that in this country?—No, it was in Canada.

16730. With respect to the evidence which has been given to us by some witnesses, and some witnesses of great distinction, as to the experience gained by operating on animals for experimental purposes, is the difference between animals and man so great that such experience is useless or practically even misleading when applied to the human subject?—I should say decidedly not. For instance, an operation on the arteries in animals and all the delicate operations on the blood vessels are practically the same.

16731. You would not agree with that view then?—No, but there again that is a surgical point on which my opinion is not really worth anything.

16732. You said that in many cases you thought that pigs were much more instructive animals to operate upon than dogs, apart even from the natural and wise sentiment with regard to dogs?—Yes.

16733. Is it then merely a matter of money with regard to the pig, that it would cost more than a dog?—The pig is a very difficult animal to handle, of course. It has got stomach and intestines much more like those of man.

16734. To that extent it would be a more useful animal?—Yes, it would be better. The pig was used, of course, in old days. I suppose that Galen's experiments were largely upon pigs.

16735. So we have been told. Is there any difficulty in anæsthetising a pig?—I do not know. I have anæsthetised a great many animals, but I never tried a pig.

16736. At any rate, given a pig under an anæsthetic it would be more nearly allied to man in its viscera than any other animal?—Yes.

16737. And to that extent it would be a more useful animal to operate upon?—Yes, I should say very useful.

16738. You have told us that in your opinion it might be perfectly safe to trust to the humanity of the operators, and that no legislative enactment is necessary with regard to them; but do you not think that there is an advantage at any rate in an Act, which limits and makes criminal the performance of any scientific operation otherwise than in a licensed place, with a view to restraining students and others from operating, say, in their own rooms, and in places with no restriction?—There is no objection to having certain places licensed.

16739. And with regard to the other restrictions such as are at present in the Act you have said that you regard them as unnecessary or even injurious to the reputation of men of noble lives, who have been great public benefactors; but do you not recognise that there is such a thing as a sane and reasonable public opinion of persons less informed than yourself and those associated with you, and less informed perhaps than this Commission are, who still have to be regarded with a view to the restrictions which may be put on possible cruelty to animals?—But why should they not act as they act in other cases through the Society for the Prevention of Cruelty to Animals? I see no reason why that sentiment should not receive its expression through the ordinary channels.

16740. But apart from that, unless there were certain enactments as to the performance of operations, as to the place and as to the method and as to the sur-

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rounding circumstances and limitations, it would not be possible, would it, to apply a general act of cruelty to animals?—I do not know enough really about the conditions here to speak definitely about that.

16741. At present the Act for the Prevention of Cruelty to Animals is directed to wanton infliction of pain, without any ulterior cause or any good ulterior motive, and if you relied only on that, would it not be certain that there would be a great number of vexatious prosecutions of people who, from the purest motives and in the most humane way, carried on experiments which might be thought to be painful?—I suppose there would be, unless you had certain places, as you suggest, definitely licensed.

16742. Just one question as to the difference between what has been called here antiseptic surgery and clean surgery. Is there a difference? Is aseptic surgery, that is to say the abandonment of the spray treatment during an operation, tantamount simply to a reversion to the practice of cleanly surgery before the days of Lister?—In all ages there have been a few men who have had clean hands and have appreciated clean surgery; but the difference between the ante- and the post-Listerian days is the difference between chaos and order, and between a colossal surgical mortality then and nowadays, there is a progressively diminishing mortality. There has also been a great diminution in actual human suffering by the introduction of the Listerian methods and this has perhaps been the greatest of its benefits. When one thinks that before the days of Lister every operation case was dressed every day, and the poor children particularly were howling and weeping, and now a case is operated on and there is but a single dressing as a rule.

16743. (*Sir Mackenzie Chalmers.*) May I put it in this way. Is the foundation of Listerism (which is now applied universally), that contamination of a wound is contamination from without, and not from within. Is that in popular language correct?—Yes.

16744. (*Mr. Ram.*) Do you agree with this evidence, which one witness gave to us. I will read you the questions and answers: "Now with regard to this aseptic or antiseptic treatment, you have said that you were one of the pioneers of that which is practised so much now, namely, the protective measures taken by being cleanly?—(A) Yes. (Q) By being cleanly do you mean anything more than merely the use of water? (A) And soap." Is that the way in which in any great operation, abdominal section and so forth, safety is procured? Is the Listerian system sufficiently carried out if merely soap and water are used for the instruments and hands of the operator?—Not for the instruments, but often for the hands of the operator. But it varies in different cases; it is not simply one thing. Aseptic surgery includes a whole group of things—besides that the cleansing of patients, cleansing the doctors, cleansing the nurses, and the absolute protection of instruments and gauze from any contamination.

16745. And in the treatment of the wound afterwards no dressing?—The only difference is that the wound is not treated afterwards; there is no subsequent treatment. A good clean incision does not require any treatment.

16746. A good clean incision keeps clean; it cleans itself?—Yes

16747. (*Dr. Gaskell.*) When you spoke just now about malaria and Ross, would you not say that Ross's work was largely done by experimentation on birds?—Entirely; it was entirely experimentation.

16748. It would be a little false to say that the cause of malaria was discovered without experiments on animals?—Certainly, because there were experiments on birds.

16749. It was the life history of the organisms found out by Ross by experimentation on birds which really settled the question, was it not?—Yes.

16750. Could you tell me when those surgical experiments upon animals began at the Johns Hopkins Hospital; do you remember the year?—I do not remember the year definitely. I should think about 1904—the session of 1904-5.

16751. But they were going on when I was there with the Mosley Commission in 1904; was that the first year?—Yes.

16752. I was wondering whether any of those students who had that course were now in practice?—Yes, a number of them.

16753. Could you tell us at all whether there has been any noted difference in those students, when they first began operating upon man as compared with others who have not had that training?—I could not tell you any specific instances, but there have been two or three men who have gone out from the school extremely dexterous operators; I mean men who have had sufficient operative experience to tackle almost anything in the way of an operation, experience gained altogether on animals in comparative surgery.

16754. What I was rather wanting to know was whether the older people belonging to the Johns Hopkins Medical School, like yourself and others, have come to the conclusion that there is a distinct advantage in this training?—I think so. I must say that I was very greatly impressed with what I saw when I was out there last year, and saw the course.

16755. Not theoretically at all you understand, but actually whether the men turned out have shown themselves better men?—Yes, I think so. It is a course only given to a group of men specially interested in surgery.

16756. Quite so, the best men?—Yes, the best men, and the men who wish to practise subsequently.

16757. I was wondering rather whether any comparison could be made between the men who have been sent out since that school has been started, and previously to that. It is very difficult, of course, to make any statement of that kind?—Yes.

16758. Can you tell me at all about Professor Howell's Physiological Laboratory? Are you acquainted with the work going on there?—Yes.

16759. Is there not something of the same sort there also in physiology? Do not the students do physiological experiments for themselves very largely?—Yes, but they are done now largely in the Hunterian laboratory.

16760. Was it not the habit there rather (I remember when I was there Professor Howell showing it me) that a group of students would perform an operation for physiological purposes under the supervision of the head of the laboratory?—Yes.

16761. And that still continues?—That still continues, but I think now that all the important operations are done in the Hunterian laboratory, because the animals are better looked after, as it is just like a hospital.

16762. In your opinion that is a good method of teaching?—I think it is impossible for certain aspects of physiology.

16763. It is not done, of course, in England?—I think very greatly to the detriment of the teaching of physiology.

16764. So far as I understand there is this sort of difference: that in England the teacher demonstrates an experiment before a class of, say, 20 to 30 students; in the Johns Hopkins Hospital that same experiment is demonstrated by the students to the teacher, the students being only, say, three or four?—I think only in suitable cases; I do not think all experiments are so performed.

16765. I mean in suitable cases. And you would rather think that that is a better method of education than the mere demonstration that we have?—Yes, in a certain limited number of cases; I think particularly demonstrations of blood pressure and demonstrations of the effect of anæsthesia are practical experiments which it is very difficult to get students taught properly, unless they are either shown or do these experiments themselves.

16766. Have you any knowledge at all with respect to those experiments which are done in that way in Professor Howell's laboratory of any inadequacy in the method of experimentation, in the anæsthesia or anything of that kind?—I think not. I think they have been very careful. Dr. Howell is a most humane man in every way. So far as I have ever seen things there—and I have been in the laboratory at intervals—I have never seen anything that was in the slightest degree indicative of cruelty to animals.

16767. But I mean carelessness; whether the students have in these operations evidently been extremely careful?—Yes, I should say so.

16768. That was my experience when I was there; they seemed to me extraordinarily careful?—Certainly.

16769. (*Mr. Tomkinson.*) Do I correctly understand that it is believed now that malaria and such com-

plaints are only communicated by infection carried by insects?—Yes.

16770. And that the extirpation of the insects would mean the extirpation of the disease practically?—Yes, and the isolation of the patient in the case of malaria and treatment with quinine, not allowing the mosquitoes to get to him.

16771. But it must have originated some time or other from climatic conditions?—That is the old story, whether the owl came from the egg or the egg from the owl. I do not know that we have settled that yet.

16772. Does myxœdema exist in animals?—I do not think it does. I would not be certain. I have never read of an instance.

16773. Do I correctly understand that the thyroid gland in animals has been utilised for the purpose of injecting the fluid into the gland of a person in whom the function was wanting?—No, not injected into the gland; it is just simply eaten; it is taken as a powder.

16774. If, as I understand, there is no Act in America, and yet there is still a stringent law for preventing cruelty to animals, how is the profession protected against prosecution?—I think public sentiment trusts the men who are in charge of laboratories.

16775. (*Sir Mackenzie Chalmers.*) Is there no active anti-vivisection society in Baltimore?—I think they are active in many places. I do not know that there is one in Baltimore.

16776. They do not take any action in Baltimore, at any rate?—No.

16777. (*Mr. Tomkinson.*) There have been no prosecutions for cruelty that you know of?—I do not know of any.

16778. Although there is no exemption from the provisions of that Act given to the faculty?—No.

16779. (*Dr. Wilson.*) Was it not really through the strong representation on the part of the medical profession that the Vivisection Act was passed in this country?—I could not say. I do not know about it. I should not have thought so.

16780. Are you familiar with Dr. Leffingwell's writings?—Yes.

16781. I think he points out that it was through the strong attacks that appeared in the "Lancet" and the "British Medical Journal" that the Vivisection Act was passed?—That is news to me.

16782. You do not know that?—No.

16783. The Hunterian laboratory is an endowed laboratory, is it not?—It is supported by the University.

16784. There is no special endowment?—No, I do not think so.

16785. Do you not attach the same importance in teaching surgical students to what has been called living pathology?—I am not a teacher of surgery.

16786. But you do recognise that the question of sentiment, the ethical side of the question, is a question that has to be faced. With regard to experimentation on dogs, for example, you yourself have said that it is rather regrettable?—Yes.

16787. Then with regard both to the extinction of both yellow fever and malaria, is not the final recourse had to sanitation, drainage, and so forth?—Yes, but sanitation which has been taught, remember, by these very things. What sanitation would there have been in the Isthmus to-day if it had not been for the work of Manson and Ross in malaria, and of those men who worked at yellow fever? Sanitation would not exist if it had not been for those men. Those are the essentials which precede it.

16788. But still those essential sanitary conditions were believed to contribute largely to the extermination of the disease before the mosquito theory was started?—But there was no knowledge how to carry out that sanitation effectively.

16789. Is there not very little analogy between yellow fever and malaria, as regards what you call the immunes and non-immunes?—I do not know about that, because in every malarial district a large proportion of the population, particularly children, really have malaria without having any symptoms of it.

16790. That is to say they get the plasmodium in the blood?—They get the plasmodium in the blood, so that they practically are immune and yet their danger is because they are capable of infecting mosquitoes and in that way conveying the disease to others.

16791. I am referring to Europeans suffering from the disease. Do they often have second attacks?—Yes.

16792. But with yellow fever you do not have second attacks?—No.

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Lieut.-Colonel EDWARD LAWRIE, M.B., I.M.S., called in; and Examined.

16793. (*Sir William Church.*) You are a Lieutenant-Colonel in the Indian Medical Service?—Retired.

16794. You are a member of the Royal College of Surgeons?—Yes.

16795. Have you any other degree that you wish to have mentioned?—I am an M.B. of Edinburgh University.

16796. I hope you understand that this Commission is only concerned, in so far as anæsthetics go, in the giving of anæsthetics to animals which are under experimentation, and that we have nothing to do with the question as to how anæsthetics are best administered except to animals under experimentation?—Yes.

16797. I think you have certain points that you wish to bring before the Commission on that head?—Yes.

16798. The first is that anæsthesia and vivisection are, or ought to be, inseparably allied, and until the administration of chloroform is established on a uniformly sound basis the question of vivisection cannot be set at rest. Would you wish to make any remark upon that? I hardly follow it. I should like to know quite what you wish us to gather from it?—That until chloroform is given in the way that has been proved to be correct, vivisection cannot be carried on without pain; that the present position of chloroform leads to its administration in the wrong manner, and that leads to the administration of other anæsthetics which are thought to be better than chloroform.

16799. And which you consider are not anæsthetics at all?—No, I do not say that at all. I say they are not so safe as chloroform.

16800. Still, I do not see what the immediate connection between the two is?—What I mean is that vivisection ought not to be practised unless it is invariably done under anæsthesia.

16801. That is the law?—But the anæsthetic is now given through a tracheal tube, which necessitates a painful operation before anæsthesia is commenced.

16802. On the contrary, the animal is anæsthetised before the wound is made?—In experiments I saw, the operation of tracheotomy was done first.

16803. (*Sir William Collins.*) Without anæsthetics?—Yes.

16804. (*Sir William Church.*) Where was that?—In Edinburgh, in Professor Rutherford's laboratory. I have narrated them in my *précis*.

16805. What date?—1890.

16806. I suppose there was no necessity for it?—No necessity whatever that I know of.

16807. And you wish the Commission to infer that that is ordinarily what is done in physiological laboratories?—I can only speak from my own observation. I understand that that is the way that anæsthetics are usually given in physiological laboratories.

(*Dr. Wilson.*) It would be a violation of the Act.

(*Sir Mackenzie Chalmers.*) It is a violation of the Act certainly; an offence is committed if that is done.

(*Witness.*) I have seen it done.

(*Dr. Wilson.*) Then it was a violation of the Act.

16808. (*Sir William Church.*) Then you go on to say that in consequence of the erroneous principles which govern the present teaching of anæsthetics, deaths occur, and the employment of such principles in vivisection experiments involves cruelty. But as the animal has to die before it recovers from the anæsthesia I do not see that it matters to the animal?—But if the tracheotomy is done first?

16809. That you have told us; we can pass on from that. It is certainly news to the Commission and contrary to what we have heard is the practice.

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16810. (*Sir Mackenzie Chalmers.*) Have you ever seen it done in any other laboratory except by Professor Rutherford in 1890?—I have seen it done without any chloroform at all. I did not see it done, but I know it is done.

16811. (*Mr. Ram.*) Where?—I would rather not say where if I am not obliged.

16812. When?—In 1894.

16813. In this country?—Yes.

16714. (*Sir Mackenzie Chalmers.*) In Scotland?—No, in England.

16815. (*Dr. Gaskell.*) What was done?—Cross circulation experiments.

16816. But what was done; was tracheotomy performed without chloroform?—The whole dissection was performed without chloroform.

16817. Do you say that it was performed without anything in the nature of an anæsthetic?—It was performed with a small dose of morphia.

16818. (*Mr. Tomkinson.*) What was the operation?—What is called a cross circulation experiment. The necks of two animals are dissected and the large blood-vessels cross-connected.

16819. (*Sir William Collins.*) What animals were they?—Dogs.

16820. (*Sir William Church.*) But it was under an anæsthetic?—I do not call morphia an anæsthetic.

16821. (*Mr. Ram.*) In no case?—Not unless it is given in a poisonous dose.

16822. (*Sir William Church.*) Then you go on to say that the chief fallacy which throttles anæsthetics at present, in the operating theatre as well as in the laboratory, is the unfounded doctrine that one of the dangers of chloroform anæsthesia is death from heart failure?—Yes.

16823. You deny, then, the fact of there being such a thing as heart failure in chloroform?—From the direct action of chloroform I do.

16824. I hardly know what the direct action of chloroform means?—Chloroform has no direct action on the heart. That is what I mean.

16825. I think you agree that chloroform lowers the blood pressure?—Most certainly it does. It is the first effect of chloroform.

16826. And how is the blood pressure lowered?—The blood pressure is lowered by narcosis of the vasomotor centre of the brain.

16827. What condition of the heart does that lead to?—It rather eases the work of the heart.

16828. Does nothing else happen to the heart at all?—Nothing else whatever until the respiration stops.

16829. But when respiration is impeded?—When respiration stops, then the heart runs down and fails.

16830. In what way?—From want of nutrition; its nutrition is cut off and it runs down and fails.

16831. Is there no other cause?—In the complicated action of the drug there is no other effect whatsoever.

16832. Its contraction is not interfered with?—Not a bit.

16833. It does not become dilated?—No.

16834. Then you go on to say that the Hyderabad Commission on Chloroform proved that the uncomplicated action of chloroform, which may be called its normal action, is to cause (1) the normal fall of the blood pressure, with (2) unconsciousness, (3) then anæsthesia, (4) then stoppage of the respiration, (5) then failure and stoppage of the heart, and (6) then death, I suppose that is agreed to by all anæsthetists, is it not, in physiology?—They do not teach it. They teach that it has a direct action on the heart.

16835. Is not that the order in which the symptoms appear?—That is the exact order in which they occur.

16836. You also on your *précis* state that, owing to the rejection by physiologists of the results of the work of the Hyderabad Commission on Chloroform, vivisection experiments have been brought into disrepute in Great Britain. I fail quite to see the bearing of that?—Because they are not done painlessly, as I take it they ought to be. Vivisection experiments are not done painlessly.

16837. (*Mr. Ram.*) Is that your opinion, or your knowledge from facts?—It is from what I have seen myself.

16838. (*Sir William Church.*) Am I to conclude from that that you think that an animal which is receiving an anæsthetic through the trachea does not become unconscious to pain?—It does, of course.

16839. Then in what way is the animal not unconscious?—In that it has had tracheotomy first, which is a painful operation. To get the tube into the trachea you must perform tracheotomy.

16840. But so far as our information goes that is not so. You have given us information which may be very valuable, that you yourself saw it done once, or heard of it; I think you said that you did not see it done?—I saw it done a great many times, as it happens, in Rutherford's laboratory in Edinburgh.

16841. In 1890?—In 1890.

16842. And you inferred from that that when an animal is kept under an anæsthetic with a tube in its trachea the initial operation is never done under anæsthesia?—I do not say it is never done, but I say that I know from my own observation that it is done without an anæsthetic.

16843. (*Dr. Wilson.*) When you say "is done" you mean that it was done when you saw it. You could not say that it is done now?—No, I cannot say that it is always done from my own observation, but I understand, of course, that it is done constantly in that way.

16844. Now?—Yes, I understand so; but I cannot say from my own observation.

16845. (*Mr. Tomkinson.*) In the case that you saw, how was the animal restrained?—It was fastened down on what was called the rabbit board.

16846. Did it cry?—No, I do not think it made any particular noise.

16847. Was it muzzled?—It was tied up.

16848. Was it muzzled?—Yes.

16849. Was it prevented from crying?—It was absolutely tied down so that it could not move.

16850. And prevented from making a cry?—I do not remember about crying at all.

16851. Did you say that it had morphia?—No, it had nothing at all before this.

16852. (*Sir Mackenzie Chalmers.*) Did you know that it had no anæsthetic of any sort or kind?—Yes, I saw it done without any anæsthetic of any sort many times.

16853. (*Dr. Wilson.*) The skin, I think you say in your *précis*, was snipped with scissors?—Yes; I took it, of course, from seeing that—I was fresh home from India—that it was the usual way of doing vivisection operations.

16854. (*Sir William Church.*) But you have had plenty of opportunities since 1890 of becoming acquainted with what is the habit in physiological and other laboratories?—No, I have been in India since then.

16855. Is that the way you do it in India?—Never.

16856. (*Dr. Gaskell.*) Who was present in Professor Rutherford's laboratory at the time?—His laboratory assistant.

16857. And himself?—Yes, and myself.

16858. Unfortunately he is dead?—Yes.

16859. Are you quite sure that nothing was given to the animal beforehand?—Yes, quite certain.

16860. (*Sir William Church.*) You go on to say that with stoppage of the respiration the intake and action of chloroform cease, and that if the animal does not then recover spontaneously the circulatory mechanism shuts down and it dies from failure of the heart?—Yes.

16861. Therefore you agree with other anæsthetists that death occurs in one of two ways: either from failure of respiration or from failure of the heart?—No, only in one way; I say from failure of respiration, and that leads to failure of the heart. The heart must fail if the respiration stops.

16862. Sir Victor Horsley (you say) has shown that death occurs in the same way by hanging when the neck is broken, and by bullet wound of the brain; and you say that in all three cases the respiration stops,

more or less rapidly in the case of chloroform, instantaneously in the other two, and the heart afterwards runs down and fails. That you think is only possible when respiration has failed first?—Yes.

16863. In all three cases you say cardiac failure is attributable to the cessation of respiration, and chloroform has no more direct effect on the heart than the bullet or the hangman's rope. Then you have been good enough to furnish some photographs; but really that is a scientific question that we have not to go into at this Commission here?—Of course; but I have seen the same effect myself in a bullet wound of the brain, and I know of its happening in hanging.

16864. (*Sir Mackenzie Chalmers.*) What happens in hanging?—I know of two cases of hanging this year, in one of which the heart did not stop for 15 minutes after the hanging, and in the other for five minutes.

16865. Although the neck was broken?—Yes.

16866. Would sensation be gone?—I think so, certainly.

16867. (*Sir William Church.*) Then pages 2 and 3 of your *précis*, unless there is something you wish to draw our attention to, have not much bearing on experiments on animals until you come to the point on page 3 where you are describing the difficulty of making an incision into the trachea because of the fur?—Yes.

16868. That we have gone into. You say that that is what you saw done once?—Yes, many times.

16869. And you also say that you saw another physiologist perform prolonged vivisection experiments on dogs under a ten-drop dose of morphin solution, this dose being equal to one-twelfth of a grain of solid morphin. Had they no other anæsthetic of any sort or kind?—I believe not until the observations began on the action of chloroform.

16870. (*Sir William Collins.*) Were those dogs insensitive to pain when they had the morphia only?—They could not have been.

16871. (*Dr. Gaskell.*) Why not?—It is quite impossible that a ten-drop dose of morphin could make a dog insensible to pain.

16872. What is the usual dose given to man?—As a sedative to lull certain kinds of pain, 30 to 60 drops.

16873. One-twelfth of a grain?—No, that is much too small a dose.

16874. That very frequently is the dose?—I have never seen it given in that dose to relieve pain.

16875. One-eighth of a grain is frequently given?—That is very insufficient.

16876. Is it not sufficient in renal colic to stop pain?—Morphin has no effect whatever on renal colic. I have had experience of it myself.

16877. (*Sir William Collins.*) Would it make a man insensitive to a cutting operation?—No, not in the least; it would have no effect on the sensibility.

16878. (*Mr. Ram.*) You mean in the dose that you have just named?—In any dose I believe, short of a poisonous dose, if you take renal colic as an example. It has no effect whatsoever on renal colic.

16879. (*Sir Mackenzie Chalmers.*) You mean that with a lethal dose pain is not stopped until death occurs?—A lethal dose is a different matter. I am talking of a medicinal dose.

16880. In the case of a lethal dose, when does insensibility to pain come in?—When the animal is narcotised.

16881. (*Dr. Gaskell.*) Is it a mistake then for the medical profession to suppose that morphia is a good thing to stop pain?—It does not have much effect if there is much pain. It does lull pain in a big dose.

16882. (*Sir William Collins.*) Does the conclusion in the question just put to you arise out of anything that you have stated to the Commission?—I do not think so, but I can tell you my own experience about renal colic and morphia. I passed a kidney stone soon after coming home from India, and I was in such fearful pain that I had to send for a medical man. I sent for a great surgeon who lives close to me, Mr. Cheatle, whose name everybody knows, who gave me morphin, but it had absolutely no effect. In about 20 minutes he proposed to give me another dose. I said I was very much afraid of morphin, as it appeared to increase the collapse, but not at all afraid of chloroform. He then gave me chloroform for five and a half hours until the stone passed. But the morphin had no effect whatsoever, and it was given in a full dose.

16883. (*Mr. Tomkinson.*) But I suppose that even if morphin can be said to lull pain it need not be an anæsthetic for cutting operations?—It is not an anæsthetic in any sense of the word.

16884. (*Mr. Ram.*) Not even in a lethal dose?—In a lethal dose it makes a man comatose. It takes an enormous dose to have any effect on a dog.

16885. (*Dr. Gaskell.*) Have you tried it on dogs?—Yes, we have. I have the notes of it here if you like to have them.

16886. (*Sir William Church.*) I think we have had a great deal of evidence that it requires a large dose of morphia to affect a dog. I think page 4 of your *précis* is chiefly what I might call matter which does not concern us; it is chiefly a scientific question. Then on page 5 you say that in the vivisection experiments which have been conducted by physiologists for the last 15 or more years, either to uphold the heart theory of chloroform or for scientific purposes, the anæsthetic has been pumped into the lungs with a motor through a tracheal tube; and that the meaning of this is:—(a) that no physiological experiments on the action of chloroform during this period possess any true scientific value, and (b) that owing to the anomalous position of anæsthetics, all vivisection experiments have been accompanied by unnecessary pain. Would you please explain a little what you mean there?—The essential point in giving chloroform is never under any circumstances to interfere with natural respiration; but if you give chloroform through a tracheal tube you must do that—you must interfere with the natural respiration, and you must vitiate the experiment so far as the action of the chloroform is concerned.

16887. I do not follow you there. It is received by all physiologists that it interferes with natural respiration when the animal is anæsthetised through the intentional opening of the trachea?—It is a matter of ordinary common sense that if you pump the chloroform in through the trachea it must interfere with the natural breathing.

16888. (*Sir Mackenzie Chalmers.*) But it produces anæsthesia?—It produces anæsthesia, and it produces other effects as well caused by interference with the natural respiration.

16889. (*Mr. Ram.*) The animal does not suffer any pain?—No.

16890. (*Dr. Gaskell.*) If it is not pumped is it not natural respiration if the animal is breathing the air mixed with chloroform through the tracheal tube?—That is a different thing. I have seen that done, but this remark does not apply to that. It applies to pumping.

16891. Do you think that pumping in the way you mention is the usual method?—It is a very usual method, I take it; I understand that it is.

16892. If artificial respiration is not required. You mean artificial respiration, I presume?—No, I mean the way of giving chloroform pumped in by a motor.

16893. I have never seen it done?—It was done in those experiments described in the trial, Bayliss *v.* Coleridge.

16894. It was passed through a Woulff's bottle?—No, it was done by a motor in the next room in those experiments.

16895. (*Sir William Church.*) Do you know of any physiologists of repute who take the same view with regard to that, that if air charged with chloroform is administered through the trachea to a dog it interferes with the correctness of their experiment?—I cannot tell you what other people think about it. I can only tell you my own view of it.

16896. Then can you tell me of anybody else's views that coincides with yours?—Everybody's view must coincide with mine. It is actual fact; it is not a matter of opinion. If you pump air into the trachea it must interfere with natural respiration.

16897. (*Sir Mackenzie Chalmers.*) You are only speaking of the effect of chloroform, not other anæsthetics?—Yes.

16898. And experiments under the action of chloroform?—Yes.

16899. (*Sir William Church.*) But it does not cause

animals to have unnecessary pain?—Not if they are tracheotomised under an anæsthetic.

16900. You would withdraw, I suppose, your statement No. 2, on page 5, that it necessitates a painful operation before the administration of the anæsthetic begins?—If the tracheotomy is not done first it does not apply to those cases, of course.

16901. (*Mr. Ram.*) Or if it is done under anæsthetics?—If the tracheotomy is done under anæsthetics. But if it is done under anæsthetics, why should they change the mode of administration? And of course nothing can be worse for students than to see chloroform given in that way.

16902. (*Sir William Church.*) I suppose you are aware that chloroform is given in that way in a considerable number of operations on man?—Through the trachea?

16903. Yes?—No, I am not. I have never seen it given in that way. It may have to be done as a very, very rare and exceptional thing.

16904. And you consider that it teaches students wrong methods of administering anæsthetics which must tend to vitiate the whole of their careers?—Yes.

16905. Do you really mean the Commission to infer that the teaching of the administration of anæsthetics is seeing the physiological experiments in the laboratory?—I do not quite understand what you mean.

16906. You say, "It teaches students wrong methods of administering anæsthetics, and this must tend to vitiate the whole of their careers"?—Yes.

16907. I asked whether you think that teaching the administration of anæsthetics consists in seeing the physiological experiments on animals in laboratories?—They ought to be taught the right way of giving chloroform in those experiments, which ought to be utilised for that purpose. It is just as important as showing them other physiological effects.

16908. You do not mean the Commission to infer that that is the way students are taught to give anæsthetics?—So far as the laboratory is concerned it is.

16909. The physiological laboratory?—Yes, the physiological laboratory.

16910. But what proportion of the experiments which are done in physiological laboratories are done in this method of having the trachea opened and a tube inserted?—That I cannot tell you.

16911. Do you wish the Commission to infer from this evidence that you have given that it is the usual method?—I understand that it is the usual one; but of course the Commission can find out quite easily whether it is or not.

16912. You would not like to express an opinion yourself as to whether it is the usual way or not?—I imagine that it is the usual way, most certainly.

16913. You have not made any inquiries before you made this statement?—It is from what I have seen myself and from the manner in which all the experiments on chloroform have been described that I say chloroform is always given in that way through the tracheal tube.

16914. In cases of prolonged demonstrations before students, you mean?—Yes.

16915. You do not think it is the usual way in ordinary experimentation in a laboratory?—I understand that it is, but I may be wrong about that, of course.

16916. (*Dr. Gaskell.*) The point is the pumping. Is that what you imagine is the usual way, pumping the air and chloroform?—Yes.

16917. That is not true?—That was described as having been done in the Bayliss trial, and it was done in Embley's experiments.

16918. (*Dr. Wilson.*) That is counteracted by artificial respiration, is it not?

16919. (*Dr. Gaskell.*) It would be necessary for artificial respiration—not natural respiration?—But it does both.

16920. It is for artificial respiration that pumping is done?—And for the giving of the chloroform.

16921. In artificial respiration. In natural respiration the animal breathes air saturated with the right amount of chloroform?—This remark does not apply to natural respiration.

16922. (*Sir William Church.*) Then you go on to state that to make sure of painless vivisection, two things appear to be necessary; first, that it should be made illegal to give chloroform to animals in any other way than by the mouth, on the principles which have been proved to be sound by the Hyderabad Commission. That is what you would recommend to this Commission?—I think that certainly ought to be done in order to put a stop to giving chloroform in this way.

16923. Has nobody else worked at the administration of chloroform except the Hyderabad Commission?—Hundreds of people have, so far as I know.

16924. Do they all agree with the conclusions of the Hyderabad Commission?—The conclusions of the Hyderabad Commission have been absolutely ignored.

16925. It appears in other words that the majority of those who have studied the subject do not agree with them?—Certainly they do not.

16926. Would it not be putting a considerable difficulty in the way of experimentation if animals could only be kept under anæsthesia by what you call the natural way?—No, I do not think it would at all.

16927. In what you call the natural way, is there any method by which you can actually measure the quantity of chloroform that the animal receives?—No, there is not.

16928. I suppose you know that great attention has been paid to the actual amount of chloroform that the patient receives now?—It has been said that the chloroform has been measured, but no one can possibly take measure of the amount that is respired—the amount that is taken in by the patient.

16929. Are you acquainted with Mr. Vernon Harcourt's method of measuring chloroform?—Yes, I am. I have not seen it done.

16930. You do not think it reliable?—It is very reliable in this way, that it dilutes the chloroform; but it cannot measure the amount that the patient takes in.

16931. Then you recommend, secondly, that the General Medical Council should have the power to treat the infliction of unnecessary pain by experimenters on animals as a disgraceful act. I do not quite understand what you mean by that?—Some such power should be given to the General Medical Council in order to put a stop to unnecessary pain in vivisection.

16932. But the General Medical Council only have power over the medical profession?—Exactly.

16933. You would leave the physiologists, who appear to be in your mind the chief offenders, untouched?—But they are part of the medical profession, I take it.

16934. (*Chairman.*) Not necessarily, unless they have a medical degree?—Of course, some such power ought to be given. Someone ought to have the power of putting a stop to unnecessary pain. And, of course, there is the important point, the teaching of anæsthetics.

16935. (*Sir William Church.*) You say that chloroform given on the principles advocated by the Hyderabad Commission is a totally different anæsthetic from chloroform given on the heart principle; and you say that the former is always safe, the latter never. That in your own opinion?—Well, it is proved by statistics, and anybody can be taught to give chloroform safely on the Hyderabad principle, while no one can on the heart theory.

16936. You think that the giving of chloroform to dogs would be a reliable method of instruction in the use of anæsthetics for medical students?—Most valuable.

16937. You consider, then, that anæsthetics now are not given as skilfully as they might be by the medical profession?—I do not say anything about the skill. It is not a question of skill; it is a question entirely of principle.

16938. Then you go on to say that it would be a great boon to our sailors and soldiers in war time if all nurses and hospital orderlies were taught to give chloroform—at any rate, up to the stage of unconsciousness?—Yes.

16939. Then you would advocate the use of chloroform by others than those who hold a medical diploma?—I would advocate the use of chloroform by nurses,

and by hospital orderlies, and by medical students under proper supervision.

16940. Then you go on to say that the specialist in anæsthesia was called into existence by the belief, originated by physiologists in 1864, that the fall of the blood pressure under chloroform is synonymous with heart failure. Surely that is rather a bold statement, is it not?—I think that is really what led to the employment of anæsthesia specialists.

16941. Were there not men specially skilled in anæsthetics before 1864?—There were, but a special class of specialist was called into being by the experiments of 1864, and those of 1879, which followed.

16942. You finish your *précis* by saying that chloroform was given by students in Edinburgh for more than twenty years in the middle of the last century in all surgical operations without a death. Is that quite the case?—It is absolutely true so far as the clinical wards were concerned.

16943-4. There have been deaths from chloroform in Edinburgh; I cannot say whether in the clinical wards or not?—At that time, I believe that in that period there may have been a death, there was a death, but it was not in Syme's wards. I am talking now of the wards of the professor of clinical surgery during that period.

16945. But there were accidents from chloroform, were there not?—There were no accidents in his ward.

16946. But Simpson had a fatal case?—Yes, he had, I believe.

16947. That is one. Are you aware of the difference in the way inquests are held in Scotland and England?—There are no inquests that I know of in Scotland.

16948. So that a death from chloroform does not get into the papers?—There is an inquiry by the Procurator Fiscal.

16949. But you feel quite sure that during twenty years—I do not know what twenty years you take—there were no deaths?—No death in Syme's wards from 1847 to 1870—until he died.

16950. But you do not say in Mr. Syme's wards?—My remarks apply to Syme's wards.

16951. But it makes a considerable difference?—It really does not make much difference, because I know of no death in the whole of the infirmary during that time except one rather doubtful one. That applies especially to Syme's wards. The chloroform in those days was invariably given by students. During the whole time that I was in India in charge of many different hospitals the chloroform was given by students also in precisely the same way, and there was one death.

16952. The whole bearing of your evidence before us to-day is that chloroform can be given with perfect safety, so long as the respiration is attended to?—So long as the regular respiration is maintained.

16953. And you would always be able to see whether respiration is embarrassed and you are getting into danger?—You have no business to allow the respiration to become embarrassed at all under chloroform. You must maintain regular respiration. If the respiration is embarrassed you must take away the chloroform.

16954. (*Sir Mackenzie Chalmers.*) And stop the operation?—No, on no account, stop the administration until the respiration is regular again. That is a matter of simple management.

16955. Is there any danger of pain supervening when you stop giving the chloroform?—No, not a bit, you only stop it for the temporary embarrassment.

16956. You can go on with the operation, the patient being perfectly free from pain?—You just stop the chloroform, you take away the cap while the embarrassment lasts, and then go on with it again. You do not allow the respiration to be embarrassed while chloroform is given.

16957. (*Sir William Collins.*) How long have you been engaged in studying anæsthetics?—I began to give them as clinical clerk in Edinburgh in 1865.

16958. How long have you made them a special study?—Ever since then.

16959. A special study?—Yes, a very special study.

16960. About how many cases have you administered chloroform to?—I published some statistics in 1901 for 10 years. That included 17,000 cases, and I have not got the record for previous years, because I was moved about in India. I cannot quite tell how many, but the number was enormous.

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16961. We have been often told that chloroform was discovered by experiments on man, and not on animals?—Yes, but its present position depends on laboratory experiments.

16962. Was it discovered by experiments on man?—I believe so.

16963. I understand that you think Syme's method of administration satisfactory?—It was satisfactory in his day.

16964. Very satisfactory?—Yes, perfectly.

16965. Was it based upon clinical experience?—It was based entirely on clinical experience.

16966. Did Syme rather deprecate experiments on animals?—I cannot say anything about that. I do not know what his opinion was.

16967. I think before the last Royal Commission of 1876 it was stated that he rather opposed, at any rate, in his later years, experiments on animals?—From my recollection of him I should think he did, but I cannot say positively.

16968. You are a strong advocate for watching the respiration, and you think the suggestion that chloroform kills by action on the heart is erroneous, I understand?—We have proved it; the Hyderabad Commission have proved it.

16969. Did the Cambridge experiments to which you have alluded tend to show that it acted upon the heart?—The Cambridge experiments were a failure; they were allowed to be wrongly performed.

16970. Were they vitiated?—In the experiments that were done at Cambridge the chloroform had access to the brain when it was supposed to be having access to the heart alone.

16971. Was it thought as the result of those experiments that chloroform acted upon the heart?—Yes, it was allowed.

16972. Is that what you consider a wrong principle?—Yes, it is a wrong principle, from our point of view.

16973. Was the vitiation of those Cambridge experiments admitted?—It was admitted by Dr. Gaskell himself.

(*Dr. Gaskell.*) Oh, no.

(*Witness.*) I have your letter.

(*Dr. Gaskell.*) The vitiation of the experiments was not admitted by me.

16974. (*Sir William Collins.*) Are you referring to the letter on page 17 of your book on chloroform?—Yes.

(*Sir William Collins.*) I do not know whether Dr. Gaskell wishes it read.

(*Dr. Gaskell.*) You may read it if you like.

16975. (*Sir William Collins*) What is the clinical practice at the present time of those who administer anæsthetics; is it to watch the heart or watch the respiration?—All the teaching at present in the text-books is that chloroform affects the heart, but I have seen many of the best anæsthetists in London give chloroform, and I have never seen any of them watch the pulse.

16976. On page 20 of your work on chloroform I see you quote Sir Lauder Brunton's statement that "the fear of chloroform paralysing the heart is based on laboratory experiments rather than a clinical experience." Is that the point that you wish to bring out?—Yes, that is a point that I wish to bring out.

19677. That clinical experience rather than laboratory experiment has been the truer and safer guide in the administration of chloroform?—The point is this, that the laboratory experiments were supposed to afford scientific proof that chloroform affects the heart.

16978. I understand you to suggest that they do not?—Our experiments proved where those scientific experiments were mistaken. The Hyderabad Commission experiments traversed those experiments, and we showed where the mistake arose in the physiologists' experiments.

16979. So that the laboratory experiments I understand you give contradictory results?—Yes.

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16980. Whereas the clinical experience you regard as safe?—And there are no contradictory results in this way. All our own experiments were done by chloroform administered in the ordinary way by inhalation. All the laboratory experiments on which this theory is based, that chloroform affects the heart, were performed, so far as I know, with chloroform given through a tracheal tube.

16981. But there were results which were opposed to yours?—They are opposed to observations we made when chloroform is given by the mouth.

16982. You quote with approval the statement of Sir Lauder Brunton: "The fear of chloroform paralysing the heart is based on laboratory experiments"?—Yes.

16983. So that apparently there were laboratory experiments which suggested that chloroform paralyses the heart?—Yes, those are the experiments of the 1864 Commission.

16984. Was that conclusion in your opinion right or wrong?—That conclusion was really wrong, because it was inferred that the fall of blood pressure was synonymous with heart failure.

16985. Then should I not be right in saying that the conclusions arrived at from the laboratory experiments were contradictory?—Yes, they were in that way, but we brought them all into harmony.

16986. Was the harmony supported from the first by the clinical experience?—It was.

16986a. (*Sir Mackenzie Chalmers.*) I do not want to go into technical matters, but the Hyderabad Commission experimented with animals, did they not?—Yes.

16987. Do you agree that animal experimentation is necessary for the purpose of determining the action of chloroform and other substances?—It was necessary then to refute other experiments by laboratory experiments. There was no other way of refuting them.

16988. Do you agree that animal experimentation generally is necessary for advance in medical science?—Yes.

16989. And in physiology?—Yes.

16990. And in pharmacology?—Yes.

16991. Has medical science received great benefit from it in the past?—Yes, enormous benefit, but it would receive the greatest benefit of all if the Hyderabad experiments were not ignored.

16992. Apart from these tracheotomies that you saw in Professor Rutherford's laboratory, have you any knowledge yourself of any painful experiments being performed without anæsthetics in England?—I have seen two sets of experiments.

16993. In one of which I understand the animal had a dose of morphia?—They all had a dose of morphia.

16994. In one set of experiments nothing was done?—No.

16995. And in the other set of experiments, the cross circulation experiments, morphia was given?—Yes.

16996. Apart from that, do you know anything about the ordinary practice in laboratories in England?—No, except what I heard upon that trial, the Bayliss trial.

16997. The evidence in that trial?—Yes.

16998. It did not appear at that trial, did it, that tracheotomy was performed before the animal was anæsthetised?—I do not remember that anything was said about it.

16999. So far as we have had evidence of the Commission here, the evidence has been that an anæsthetic is administered first. Assuming that to be so, all that is done afterwards would be painless, would it not?—Quite.

17000. You have no doubt whatever that in whatever way chloroform is administered, it is an absolute anæsthetic, absolutely freeing the animal from pain?—Absolutely; from the time of unconsciousness there can be no pain.

17001. Your point is that it is improper and wrong to perform tracheotomy before the chloroform is administered?—Yes.

17002. Is tracheotomy a very painful operation?—It is a very painful operation, I should think.

17003. It is done continually on children, is it not?—It is done for croup and diphtheria.

17004. Pretty continually?—Yes.

17005. It is done on children, of course, without anæsthetics?—Not always.

17006. Could you give anæsthetics to a child with diphtheria?—Yes, you can give a small dose, but a child as a rule by the time it gets to the stage where tracheotomy is required is almost unconscious. It would practically not need an anæsthetic, or at any rate only just enough to divert its attention from the incision. You would not need more than that.

17007. In the case of a child it is not what you call a painful or severe operation?—It would be if the child was not suffocated.

17008. We have had evidence which does not always coincide on this point: That when chloroform is administered some witnesses have said that there is a sudden passage from feeling pain to insensibility, and others have said that there is a gradual diminution of pain. What is your view on that point?—Supposing that you are giving chloroform, you can keep on asking the patient questions, and suddenly the patient will stop answering, and has become unconscious.

17009. Do you think unconsciousness to pain goes before or after the general consciousness?—It goes after.

17010. For instance, if chloroform is administered not in a sufficient quantity to produce unconsciousness, is there any diminution of pain?—The patient would remember it.

17011. But do you think the pain would be equally acute?—No, not nearly so acute.

17012. You think, in fact, that as chloroform is administered, there is a progressive diminution of pain?—A progressive diminution of sensibility to pain.

17013. And that diminution of insensibility precedes unconsciousness?—Yes, and directly unconsciousness supervenes then there can be no pain; from that moment the patient will never remember anything you do afterwards.

17014. He will never remember it, and so far as you can tell will not feel it?—He will not feel it at all.

17015. You connect the feeling of pain entirely with the higher centres?—With consciousness. The patient must be conscious in order to have pain.

17016. That is to say, conscious to outward surroundings?—He must not be unconscious.

17017. But pain may be diminished before that stage is reached?—Yes.

17018. Have you performed other vivisection experiments besides those with reference to chloroform?—No, except with ether.

17019. Your whole study has been the action of anæsthetics?—Yes, and, of course, I taught students to operate by vivisection.

17020. In India?—Yes.

17021. You practised the student on an animal?—Yes.

17022. Which, of course, you anæsthetised?—Always.

17023. And then the student learnt what we may call manual dexterity?—Yes, he learnt how to become accustomed to do operations on living tissues, and also how to give chloroform at the same time.

17024. Do you attach great importance to students doing operations on living animals before they practice on living human beings?—It is of tremendous importance in India, because it is the only chance that a student ever has of doing an operation at all.

17025. But in England what opportunity has he?—In England a man goes through successive stages in hospitals of every kind. He may be house surgeon and hold all sorts of appointments, and becomes assistant surgeon and then surgeon to a hospital. But these men in India are frequently sent straight out from the schools into a district of which they have complete charge, hospital and all.

17026. They may have to do important operations which perhaps they may have only seen once in their student career?—Yes, they have to do everything from the time they get charge of a district.

17027. You are speaking of natives, of course?—Yes.

17028. Is there any difficulty with the native population in regard to teaching students on animals; is there any very strong feeling against it?—No, there is no feeling whatever as long as you do not give the animals pain.

17029. As long as the natives are satisfied that the animal is anæsthetised properly there is no feeling whatever?—No.

17030. You have no protests?—No, never.

17031. Is it since your day that what is called the Pasteur Institute has been established?—It has been established since I left India.

17032. You cannot tell us anything about it?—No.

17033. Can you tell us anything about the feeling of the natives with regard to that?—I think the natives would place the most implicit confidence in it, from what I know of them.

17034. Have you ever discussed with natives in India the question of animal experimentation?—Yes, often.

17035. Their objection is to the causing of anything like unnecessary pain?—Yes.

17036. As regards vivisection generally, do you agree to this extent, that it is not only lawful to experiment on animals, under anæsthesia of course, but that it is lawful to allow the animal to recover for the purpose of watching the effects of the operation?—Yes, I agree in that.

17037. And do you think it is necessary to inoculate animals for the purpose of watching the progress and spread of the disease?—I cannot give an opinion of any value about that, but I should think it is.

17038. You agree, I suppose, that for very slight operations more discomfort and pain would be caused by giving an anæsthetic than, say, the mere prick of a hypodermic needle?—I think it should be entirely left to the patient, however slight the operation is.

17039. Taking the case of an animal, supposing an animal is to have a hypodermic injection, it is more humane, is it not, to use the hypodermic needle without an anæsthetic than to give the animal an anæsthetic?—I do not see how it could be, although I should think it is unnecessary to give an anæsthetic. You could give an anæsthetic in the box without the animal being upset by it or terrorised.

17040. You do not think that the administration of anæsthetics to animals causes any great discomfort or apprehension?—None whatever, unless you fasten them down or hold them down.

17041. How do they do it in India?—They put them into a box with a glass cover, and directly the animal falls down on the floor of the box you take it out and put it on the table.

17042. You think that given in that way, the animal does not suffer?—It does not mind it or make any objection to it.

17043. What animals do you use in India?—Dogs, monkeys, rabbits and cats.

17044. But you would not yourself use an anæsthetic to an animal in case of an injection with a hypodermic needle or inoculation?—I should think it hardly necessary; it is so momentary.

17045. (*Mr. Ram.*) As I gather, your evidence comes merely to this, does it not? You hold a certain theory as to the action of chloroform, as to the way in which it acts?—Yes.

17046. Other people hold that chloroform acts in a way other than that which you believe?—Yes.

17047. It is admitted, is it not, on all hands that whichever theory is correct, chloroform rightly administered is a safe and sure anæsthetic, and absolutely frees the animal from pain?—Yes, but everybody does not agree that it is a safe anæsthetic at present.

17048. I leave out the word "safe." I meant by that that it is an anesthetic which absolutely frees the animal from pain?—Yes.

17049. I see in your *précis* you use this phrase, "but if it" (that is, chloroform) "is given on the theory that it may cause death by heart failure, sometimes suddenly it is the most dangerous of all anæsthetics"?—Yes, it is.

17050. Dangerous, I suppose, in that connection means likely to cause death?—Likely to cause a certain percentage of deaths.

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17051. So that it would not matter to the animal which is under operation, whether the chloroform caused death or whether it did not; in neither case would the animal feel?—That is so.

17052. So that whichever theory is right (yours or the other people's), so far as the animal is concerned the animal is equally safe if it is under a proper dose of chloroform?—So far as an anæsthesia is concerned.

17053. This Commission, as I daresay you know, has to report upon the practice of subjecting animals to experiment, whether by vivisection or otherwise, and then to consider whether the law wants amendment. Will you just tell me, please, how your evidence bears upon that, from the point we have to report upon?—My point is that all vivisection experiments can be performed painlessly if chloroform is given in the right way.

17054. Then it comes down to this, does it not, that your evidence is valuable to us according as it bears upon the method of administering chloroform?—Yes, in two ways, as regards the animal and as regards the student.

17055. You have seen it administered years ago *vid* the trachea?—Yes, but it is on those very experiments performed in 1894 that the present teaching of anæsthetic is based.

17056. And in the instance which you gave you said that there was no previous anæsthetisation of the animal?—That is so.

17057. If an animal to which chloroform is given through the trachea is previously properly anæsthetised, your theory may be invalidated, but the animal does not suffer any more?—No, if the tracheotomy is done under anæsthesia

17058. Then so far as regards the point that we have to inquire into here, it comes down to this, does it not, that you in 1890 and 1894 saw what according to your belief was, I will take it if you like that it was an actual fact that you saw the administration of chloroform *viâ* the trachea, the animal not having been previously anæsthetised?—Yes.

17059. That was an illegal operation?—That I do not know anything about.

17060. But perhaps you will take it from us as being really contrary to the very words of the Act?—Certainly.

17061. If that be so the Act wants no amendment in that respect?—No.

17062. Because the Act already makes it illegal?—Yes.

17063. Were you aware when you saw it that it was contrary to the Act?—I was told so afterwards.

17064. You were not aware of it at the time?—I did not know it at the time.

17065. How soon afterwards did you know?—Immediately afterwards.

17066. Did you report the case to anyone as being an illegal act?—No.

17067. (*Dr. Gaskell.*) I am just going to ask you one question. Can you give me the name of any person who has been in the habit of administering anæsthetics or has studied the action of chloroform who believes that chloroform has no effect upon the heart except yourself?—Surgeon-General Bomford.

17068. You mean in India; he was with you in India, was he not?—Yes.

17069. Is there anyone in this country?—I understand that all Syme's pupils have the same view as I have, but still I cannot speak for anybody else.

17070. I meant people who have been in the habit of giving anæsthetics. I mean professional anæsthetisers and that kind of person or people engaged in the investigation of anæsthetics in physiological laboratories?—In physiological laboratories, no, I cannot give you any name, but I can give you names of many surgeons and practitioners.

17071. They have hardly studied the question, have they?—They have been accustomed to doing operations, and have studied it, I should think, very closely.

17072. (*Mr. Tomkinson.*) You give a description, I see, in your *précis* of a case in which tracheotomy was performed without anæsthetics upon a rabbit, and in

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view of the questions which you have been asked as to the painfulness of it, I want to ask you one question. I see that you describe it as having been necessary or convenient to cut away the skin—to flay the skin with scissors?—Yes.

17073. That, of course, would be exceedingly painful?—I thought so, frightfully painful.

17074. There would be no comparison between the pain caused by a mere incision in the skin of a child's flesh with that inflicted upon that rabbit?—No comparison whatever.

17075. In which the skin has to undergo the most painful operation that can be imagined almost?—Yes, a very painful operation.

17076. The skin, of course, is excessively sensitive?—Yes.

17077. The cutting away of that skin, the flaying of it back would be an exceedingly cruel thing without an anæsthetic?—Yes.

17078. And that was done in that case?—Yes, in several cases.

17079. And in no case was an anæsthetic administered before the tracheotomy?—No, in no case.

17080. What is the supposed advantage of introducing the chloroform in that manner instead of through the mouth, as I see you say? That means the mouth and nostrils, I take it? The inhalation is through the nostrils?—Through the mouth and nose.

17081. The mouth has to be covered as well?—Yes.

17082. What is the supposed advantage of administering it direct into the trachea instead of through the nostrils and mouth?—I cannot imagine any advantage whatever.

17083. You hold that there is none?—Yes.

17084. And it is a wholly unnecessary operation?—Yes, and it teaches the students a wrong way of administration; they learn nothing from it.

17085. Of course it would never be so administered to a human patient except in case of obstruction?—Except in exceptional operations round the mouth or jaws which would interfere with natural breathing.

17086. You hold strongly that morphia and morphin are not anæsthetics?—I am certain that they are not.

17087. Until the point is approached of almost stupefaction approaching death?—Until their poisonous effect is produced.

17088. I asked the question of a previous witness, whether the condition of a subject under the strong influence of morphia was not akin to that of a man stupefied with alcohol. You, I suppose, would hold that in neither of those cases are they incapable of feeling pain, although apparently stupefied?—I think you might do a great deal more to a man stupefied by alcohol without his feeling it, than to a man under the influence of a medicinal dose of morphin. We did a great many experiments with morphin in the Hyderabad Commission, and our conclusion was that dogs are very indifferent to the action of morphin, so that if you want to produce any effect upon them at all you have to give an enormous dose.

17089. Have you any experience of the action of curare?—No.

17090. Or the use of it?—No.

17091. Have you an opinion about it?—I really have not, because I have no experience of it at all.

17092. But you know, I suppose, generally, what its effect is?—Yes.

17093. Do you hold any strong view as to the necessity or the advantage or the propriety of administering curare in company with other anæsthetics?—No, I cannot see what is to be gained by it myself.

17094. It is rather a leading question, but should you have any fear that its paralysing effect would prevent the animal from showing signs of pain, although it might feel it?—I should think it might, but I really do not know.

17095. (*Dr. Wilson.*) Were these morphia experiments carried out at Hyderabad on dogs to render them, or to attempt to render them, completely insensible to pain?—We carried them out really to see whether they deepened the anæsthesia or not.

17096. With chloroform?—Yes.

17097. Have you operated on dogs with morphia alone as an anæsthetic?—No, we never used it as an anæsthetic; but it had no effect in deepening anæsthesia.

17098. Can you give me an idea of the dose that would prove fatal to an ordinary sized dog?—I should think you would have to give it at least 5 or 10 grains.

17099. But it would vary very considerably, I suppose, according to the idiosyncracy of the animal, and possibly the breed of the animal?—Yes.

17100. But you could not be guided by the bodyweight?—No.

17101. Did you experiment with any other narcotics besides morphia?—We experimented with various drugs administered in order to ascertain whether they had any effect in modifying the action of chloroform; such as atropin, morphin, nicotin, and so on.

17102. But with your knowledge of morphia or morphin, would you say that it was an unreliable anæsthetic for experiments on animals?—It is not an anæsthetic at all.

17103. I mean an abolisher of pain, then?—It would not abolish the pain of a cutting unless you poisoned the animal with it.

17104. But supposing that an animal gets what is called a lethal dose, how long would it live do you think under an operation?—That I cannot tell you. It might live some hours.

17105. But it would be completely insensible to pain all the time?—Yes, quite, if it were comatose.

17106. Have you seen much of plague out in India? You do not come here to give evidence on it?—No; but I have seen a great deal of plague. I was Plague Commissioner for Hyderabad for five years.

17107. Do you believe in Haffkine's prophylactic?—At the time when I saw it used, it was always putrid.

17108. Have you studied the literature of the subject since?—I cannot say that I have followed it very closely since.

THIRTY-EIGHTH DAY.

Tuesday, 26th November 1907.

PRESENT:

The Right Hon. The Viscount Selby (*Chairman*).
Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir J. McFadyean, M.B.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.

Captain C. Bigham, C.M.G. (*Secretary*).

Mr. John Hughes, F.R.G.S., called in; and Examined.

Mr. J. Hughes F.R.G.S. 26 Nov. 1907.

17109. (*Chairman.*) You, I believe, are Secretary of the National Canine Defence League?—Yes.

17110. How long have you been so?—Three years.

17111. You have not, I understand, had any special education in surgery or physiology?—No.

17112. You speak principally to what we have been calling the ethical side of the question?—Yes. I wish to lay before the Commission the work that my society has done without entering into the scientific aspect of the question at all, that I propose to leave to the scientific witnesses whom we hope to produce.

17113. We should like shortly to hear what the number of members of your society is, and what their object is, but perhaps you will take it very shortly, because our object is not to inquire into the protection of dogs generally, but only with regard to surgical experiments?—I quite understand.

17114. Perhaps you will tell us, to begin with, what the number of the members of your society is?—Our membership at the present moment is 4,421.

17115. That is to say, I suppose, they subscribe?—They subscribe.

17116. One member of the Commission asks what is the subscription?—I am giving you a list of the members up to the present day. I can only give you a list of the subscriptions up to last year.

17117. (*Sir Mackenzie Chalmers.*) Is there any minimum subscription?—2s. 6d.; that is the minimum subscription.

17118. (*Sir John McFadyean.*) Annually?—Yes.

17119. (*Chairman.*) I suppose we may shortly take it that the object is that indicated by the title—namely, to defend dogs from anything that you think cruel or unjust, or wrong treatment in any direction?—Yes; to protect dogs from ill-treatment or ill-usage of any kind.

17120. How long has your society been in existence?—It was founded in 1891, but it was founded, not as a Canine Defence League, but as an Anti-Muzzling League. It was called into existence at the time of the rabies scare in 1891, when the Muzzling Orders were made by the Board of Agriculture.

17121. Do I rightly understand that the League was founded with the view of opposing what I may call the muzzling legislation of Mr. Long?—Yes, quite so.

17122. Have they continued always of that view?—They have retained that part of the work, of course.

17123. Of opposing muzzling?—Yes, that is a part of their work, but the work has developed in other directions since then. As the society has grown in strength they have seen other objects which they thought it advisable to take up, so that now the work of the League covers the whole of the question of the protection of dogs from ill-usage.

17124. Do I rightly gather that they always have been, and are opposed to, the principle of muzzling dogs, and of excluding dogs coming from abroad in order to prevent the disease of rabies being spread?—If you refer to the quarantine regulations of the Board of Agriculture, I cannot say that the society as such is opposed to quarantine regulations. We certainly think they are too stringent, and would like to have them altered in that respect.

17125. Does your society think they are useful in preventing the spread of rabies?—Yes, as long as they are not made too stringent.

17126. Do they object to muzzling on the ground that it is a cruel, though effective measure, or that it is a cruel and ineffective measure?—Cruel and ineffective.

17127. Quarantine they think is effective, but muzzling they think is not?—Quite so. We believe that quarantine ought to be in existence because it is a measure of prevention, to prevent the possibility of the spread of rabies. We believe, on the other hand, that muzzling really has been the means of causing rabies in dogs, rather than of preventing it.

17128. I daresay some other members of the Commission who deal with these matters will ask you some questions about that; I will not pursue it myself. As regards vivisection, that, I understand, you are altogether opposed to as regards dogs?—Quite, so far as dogs are concerned.

17129. Not merely to vivisection, but to all experiments on dogs either by way of inoculation or testing for drugs or for the causes of diseases?—Yes, we oppose every kind of experiments on dogs.

17130. And that is whether the dogs are anæsthetised or not?—Yes.

17131. Do you draw any distinction between the doing of it under anæsthetics and without anæsthetics?—No, we do not believe that any experiments either with or without anæsthetics should be permitted.

17132. You think that they are equally unjustifiable?—Yes.

17133. When I say you, of course I am speaking of the views that you represent?—Yes.

17134. Do you consider that experiments on animals generally are unjustifiable?—I can, of course, only give my own personal opinion on that question. I have every reason to believe that a large number of the members of my society are opposed to the vivisection of any animal, but I know on the other hand

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that there are some who, whilst they believe that the vivisection of some animals may do good, are equally strong against vivisection of dogs.

17135. That is not that they think that useful results might not be obtained from experiments on dogs, but that they think dogs ought to be exempt?—They think dogs ought to be exempt on moral grounds, and also so far as I can understand their views, because they believe that other animals could be experimented upon with greater effect for purposes of investigation.

17136. In all cases?—In all cases.

17137. However, that is a matter as to the distinction between the formation of the dog and the formation of man or a monkey, which is not a matter that you profess to speak about yourself?—No, not on my own authority. I have, of course, the testimonies of others; but I hope to bring forward a witness who will speak specially on that point.

17138. I think we had better, perhaps, keep clear of those questions in examining you, because we have had many gentlemen on both sides who speak from personal experience and physiological knowledge?—Yes.

17139. I understand that your main reason for claiming exemption for the dog is on moral grounds?—That is the first reason. I have put down the reasons as they occurred to me in my *précis* of evidence.

17140. That, I think, is a matter that does not require much elaboration, because while people may differ as to the weight of the reasons, the nature of them is quite obvious. Then you speak of their docility, faithfulness and companionship?—Yes.

17141. And evolution. What do you mean by that?—What we mean is that generations of training and being brought into contact with human beings have rendered the dog so much more highly developed than other animals that it is not a fit subject for experiment.

17142. But the word evolution is often used in another sense. Do you mean by it, that they are so far on their way to becoming men?—That they are nearer human beings. I do not say that they will ever become men.

17143. Not that they are on their way to that as an end?—They are on the road, but I do not know that they will ever reach it.

17144. Then you also say that they were never intended for experimental purposes. I do not quite understand what you mean by their never being intended. Do you mean never intended by Nature or Providence?—Never intended by Nature or Providence. What we know of dogs is of such a character that we cannot conceive of the possibility of their ever having been intended for the purpose of being put to pain, or for experimentation for the purpose of finding out cures for disease.

17145. When you come to say that they were never intended by Providence for that purpose, is not that entering into a very large subject, perhaps a little outside our scope. Would you say that man was never intended to be eaten by a tiger?—Certainly.

17146. Or that a cat was never intended to torture a mouse?—I cannot say that I can enter into that, but I certainly agree that man was never intended to be eaten by a tiger.

17147. I am not so sure about it. What is the distinction between a tiger eating a man and a cat eating a mouse, from the tiger's and cat's point of view?—I cannot conceive of the possibility of a beneficent Ruler and Creator creating man for that purpose.

17148. (*Mr. Ram.*) Or a mouse?—Or a mouse.

17149. (*Chairman.*) Perhaps that is pursuing, as I say, questions that we cannot follow out to the end; but it is rather difficult to assume that Providence intended a cat to torture a mouse, but did not intend a mouse to be tortured by a cat?—I can only say that it is tortured by the cat. Dogs, I am afraid, are tortured by men, too, but I do not think they were intended for that purpose.

17150. (*Sir Mackenzie Chalmers.*) I am afraid that cats are sometimes tortured by dogs?—Yes, and dogs by cats.

17151. (*Chairman.*) But when you say that dogs were never intended for experimental purposes, it opens a question which is rather difficult to decide, if you follow it out thoughout the animal life?—I quite agree, but I only put that down as the view of my society; they certainly believe that, on the same ground that you put just now, that a man was never intended to be eaten by a tiger.

17152. That is very much the same question?—Quite so.

17153. Then you give as another reason the futility of experiments. As to that again, you are not speaking from your own personal experience?—No; I have simply put those down as the reasons held by my society.

17154. We may take it, shortly, from you that that is one of their reasons?—Yes.

17155. And another is the difficulty of anæsthetisation?—Yes.

17156. That stands on the same ground?—Yes, it does, but I might say that we have had special evidence on that point, some of which I hope to be able to lay before the Commission to the effect that a dog cannot be anæsthetised in such a way as not to suffer pain; that if you give it an anæsthetic strong enough to do that you kill it.

17157. We have had before us some very eminent anæsthetists as well as eminent physiologists, and we have also had some gentlemen with knowledge and experience who have spoken on the other side of the question, but having stated that as the ground you act on, we need not follow it further with you?—No.

17158. And in the same way, with regard to the difference between human and canine organs, we have had physiologists speaking about the differences. There is no dispute that there is a difference; whether it absolves physiologists from the necessity of using dogs is another question?—Our point is that the differences are so great that all experiments on dogs are useless for the purpose of understanding the human organism.

17159. You have endeavoured to obtain legislation in favour of your views?—Yes, we have had a Bill before the House of Commons during the last three Sessions, but, unfortunately, we have not been able to get a good place in the ballot, with the result that the Bill has had to be put down under the 11 o'clock Rule, and as it was blocked every time, it has had no opportunity of being discussed in the House. I enclosed with my *précis* of evidence a copy of this Bill for each member of the Commission. I do not know whether you wish me to go into it.

17160. I think we have all read it—I have. It is a very short Bill?—Yes.

17161-2. You need not read it through. Practically it is to say that a dog shall not be operated upon; that whatever permission may be given by Parliament for operating on other animals, dogs shall be excepted?—Yes, that it shall be unlawful to perform any experiment of a nature causing, or likely to cause, pain or disease to any dog for any purpose whatsoever, either with or without anæsthetics. That is really the purport of the Bill.

17163. And that nobody, of course, shall be licensed for the purpose?—Yes.

17164. And then the rest is penalties?—Yes.

17165. That Bill, I think, has never been debated in the House of Commons?—No, it has never been debated in the House. I might say, in this connection—although I put it down in another connection—that we canvassed all the candidates at the last election on the question, and we have written pledges from considerably more than half the Members of the present House of Commons that if an opportunity occurs they are quite prepared—and they are pledged, in fact—to support the Bill.

17166. You issued a petition, I think, to the effect that the use of dogs in vivisection experiments is in no way necessary for the advancement of medical science or for instruction?—That is the medical petition. The general petition comes first, which is in these words: "That your Petitioners are entirely opposed to the vivisection of dogs, who, on account of the generations of training to which they have been subjected, and their domestication as household companions and pets, have been rendered exceptionally intelligent, peculiarly sensitive to suffering and to terror, and are docile and obedient even under torture. Your Petitioners are furthermore of opinion that, on account of this docility and obedience to the word of command, the dog has of late years been specially selected by vivisectors for

extensive and peculiarly revolting and painful experimentation, and also for demonstrations of a prolonged and agonising nature before classes of students. The Home Secretary stated on May 10th, 1903, that at one laboratory alone—that of University College, London—no fewer than 232 dogs were used for experimental purposes during the year 1902. Your Petitioners therefore pray that your Honourable House will be pleased at an early date to pass the Bill now before Parliament to exempt dogs of every kind and breed from being made the subject of vivisectional or other experiments of any kind whatsoever." That is the general petition.

17167. I see that you do not refer to anæsthetics. Is that because you think it is immaterial whether anæsthetics are administered or not?—Yes, we oppose experimentation of every kind on dogs, with or without anæsthetics.

17168. But it is very material, is it not, on the question whether the operations on them are of an agonising nature?—Yes, we do not allege that all the operations are of an agonising nature.

17169. But when you allege that all of them were of a prolonged and agonising nature, and do not say anything about anæsthetics, is that not a statement that no anæsthetics are used?—I do not think we say that they all are. We say "the dog has of late years been specially selected by vivisectors for extensive and peculiarly revolting and painful experimentation." It does not say every one of those experiments.

17170. It does not say "some of which are," but it says were "specially selected by vivisectors for extensive and peculiarly revolting and painful experimentation." However, you say that what you mean by that is "some of which were"?—Yes, we are simply giving that as a reason for doing away with it. The possibility of that being done in any case is a reason why it should be done away with altogether.

17171. Still the people who sign this petition are not people who have any apportunity of examining statistics, at least not people likely, most of them, to examine statistics, to check statements of this kind. I am only pointing out that what we have learnt from a great deal of the evidence before us is that these operations are in almost all cases done under anæsthetics—in all cases where they are not privileged by the Act?—Quite so, but a great number are privileged by the Act.

17172. Not a great number.

(*Mr. Ram.*) A very small number, indeed.

17173. (*Chairman.*) What do you say?—I think it is a large proportion—too large a proportion, I say.

17174. How large a proportion do you say? Perhaps you have some information which we have not got?—I am sure you have the information that I have, but I think the proportion is fairly large. I find that last year, according to the Home Secretary's Returns, there were no less than 281 certificates issued permitting experiments on dogs and cats and recovery from the anæsthesia; those must necessarily be of a painful character.

17175. (*Mr. Ram.*) What are you reading from?—The Home Secretary's Return of last year.

17176. Does that distinguish between cases allowed to recover and cases not allowed to recover?—Yes.

17177. (*Sir Mackenzie Chalmers.*) Certificate B you mean?—Certificate B plus E E.

17178. (*Mr. Ram.*) Would you kindly read it again?—"Permitting experiments on cats and dogs, and recovery from anæsthesia, 281."

17179. (*Chairman.*) But how many of those 281 were permitting operations on cats and dogs, and how many recovery from anæsthesia?—The whole 281 were recovery from anæsthesia.

17180. For all animals?—No, cats and dogs only. Under Certificate B, allowing for recovery of any animal, 917. I am only going into the question of dogs.

17181. There are gentlemen here more familiar with these statistics than I am; I will not ask you any more about it. You say that 281 is the number?—Yes. I have only the Home Secretary's word for it.

17182. (*Mr. Ram.*) What are you reading from?—This is an extract from the Return of the Home Secretary

17183. May I see it?—Certainly. (*Handing in the same.*)

17184. (*Chairman.*) How many signatures were there to your petition?—We have received up to date 754,584. Of that number we have already presented to Parliament 673,294.

17185. You mean that you have gone on receiving signatures since the House rose?—Yes, we are receiving them continually.

17186. Then you also have a medical petition?—Our medical petition is a short petition. It is as follows: "That we, the undersigned medical practitioners of the United Kingdom, are of opinion that the use of dogs for vivisectional experiment is in no way necessary for the advancement of medical science, or for the adequate instruction of medical students. We therefore most earnestly and respectfully pray that your Honourable House will be pleased at an early date to facilitate the passing of a measure prohibiting the vivisection of dogs." We have already received 1,261 signatures to this petition, and a large number of those have been sent in voluntarily. We sent a number of letters out, asking medical men to sign, but we made no attempt whatever to canvass the whole of the medical profession. I may say that fully two-thirds of the medical profession have not been approached on this matter.

17187. Of the one-third who have been approached, how many have signed?—1,261.

17188. I see that you wish to give an explanation of your reasons for withholding the names of the signatories from the public?—Yes.

17189. I did not know that they were withheld, but in point of fact you mean that you do not make the names public?—We do not make the names public, except with the express consent of the medical men who sign. We find that some of them object to it, and the reasons they give are such that we must respect them; they say that it places them at a disadvantage with the profession, and that since the heads of the profession are very much in favour of vivisection, they find they are both boycotted and persecuted if they are known to be anti-vivisectionists.

17190. What percentage of the 1,261 write in that strain?—A very small percentage. I could not tell you the exact number, but it is a small number comparatively. A large number have no objection whatever to their names being published.

17191. Do you mean that you have had 12, 20, or 30 letters of that kind?—I should say under 100. We put in our covering letter, a copy of which I enclosed for the Commission: "I may add that no signatures received will be published in any way, or made use of for any purpose other than for presentation to Parliament." I thought it was only fair that the Commission should know exactly how we get these signatures

17192. You have letters, you say, from medical men?—Yes, I have a very large number of letters from medical men, all of which were unsolicited; we have never asked for them.

17193. It is rather difficult to take the opinions in letters of medical men with regard to questions of medical knowledge. We might dispense with witnesses altogether in the Commission if we did that?—Yes; I quite understand that; only I thought the Commission would like to know on what we, as a society, base our views. We get these letters from all over the country from scientific men, and these come to us quite unsolicited. Our view, is, of course, that there is a very strong consensus of medical opinion—I mean a very large minority of medical men—who are opposed to the vivisection of dogs.

17194. I think we have given an opportunity to you, and I believe you are going to avail yourself of it, to call a couple of medical men?—Yes, only two, my Lord, out of a very large number.

17195. There is no objection to your stating that you have received a number of letters from medical men and the general effect of them; but if we were to have a number of letters read from different medical men who give their names, and stating what their views were, it seems to me that we should get into considerable difficulty because any member of the Commission might say that he desired to ask them questions and we should be led into a number of cross-examinations of witnesses?—As you wish about

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it, of course, my Lord. I have a selection of the letters here. I have not got them all.

17196. I think we may take it generally from you?—I should very much like the Commission to allow me to read these letters; it will not detain you a long time.

17197. I do not think the Commission object to your reading two or three for samples, but you produce a very formidable bundle there?—I do not intend reading all those, but I hope to be allowed to read about a hundred of them.

17198. I think you are a little *exigeant*?—I do not want to withhold anything from the Commission.

17199. You might give us two or three of the strongest.

(*Mr. Ram.*) And the most eminent leaders.

17200 (*Sir Mackenzie Chalmers.*) What we should like is really a man with scientific qualifications entitled to speak in the name of science?—May I point out before I begin that I have already printed some expressions of medical opinions, a copy of which I enclosed for members of the Commission; it is a leaflet that we have issued. I may say that those expressions of opinion were taken from letters which were sent to us in reply to our letters asking for signatures to our petition; but since then I have received in connection with my attempts to get witnesses before this Commission, 700 or 800 letters, quite unsolicited, from people, in a great number of which very strong expressions of opinion are given on this question of the exemption of dogs from vivisection.

17201. (*Chairman.*) Do you say 700 or 800 from people or from medical men?—From medical men. In addition to that I ought to say that I advertised offering a prize for an essay, during the summer, on the moral and scientific reasons for exemption of dogs from vivisection, and a large number of medical men, seeing this advertisement, wrote to me immediately for a copy of our petition, as they wished to sign it. Those, of course, came quite unsolicited, simply by seeing the advertisement.

17202. Your pamphlet was addressed, I understand, to the question of the exemption of dogs?—Quite so.

17203. And that is what their letters are on?—Yes.

17204. They are not letters on vivisection generally?—There are several of them that go into the question generally.

17205. But when you said about 100 of them, you were not referring to anything more than the exemption of dogs from vivisection?—Yes, that is our position. We do not profess at all that 1,260 doctors are in favour of total exemption from vivisection of all animals. I have a letter here, for instance, from Professor Woodroffe Hill, a very well known veterinary surgeon. This happens to be the first that comes to my hand now, if you will allow me to read an extract from it. I had hoped to have Professor Woodroffe Hill before you; but, unfortunately, his health is such that he cannot attend just now.

17206. (*Sir John McFadyean.*) Where is he Professor now?—I cannot say; but he is known as Professor Woodroffe Hill. He is a Fellow of the Royal College of Veterinary Surgeons. He says:—"As an earnest student of canine pathology for over 40 years, and having closely observed the various traits of canine character, I consider the vivisectors' evidence before the Commission concerning dogs is perfectly monstrous, notably with regard to anæsthetics." That is an extract from his letter.

17207. (*Dr. Gaskell.*) Does he say why he considers it monstrous?—That is all he says. Of course, I have not asked him any questions.

17208. I was wondering whether he gave any definite statement as to the meaning of the word "monstrous"?—No. Then I have a letter here from Dr. Frederick Page, of Newcastle-on-Tyne:—"Dear Sir,—My views about vivisection are that no vivisection for purposes of demonstrating to students facts should be allowed. I do not think we should interfere with injections."

17209. (*Chairman.*) He does not say of dogs there?—No; but he signed our petition for the exemption of dogs. And he goes on later:—"I would absolutely forbid dogs to be employed for vivisection purposes." This is one of the men, of course, who do not believe in total anti-vivisection for all animals; but he is very strongly in favour of the non-vivisection of dogs. Then Dr. MacCarthy says:—"I am quite prepared to give evidence in support of my position—that is, total abolition of vivisection on dogs, abolition of vivisection on all other animals, except by expert original researchers." That is Dr. MacCarthy, of Abbeyfeale, County Limerick. I have also a letter from Dr. Hogg, the coroner for Middlesex, in which he says (I cannot put my hand upon it at the moment) that he is prepared to give evidence in favour of the total exemption of dogs from vivisection, in the same way as the others do. I find it rather a difficult matter at a moment's notice to select the strongest letters, as your Lordship asked. Dr. Kellard, of Southsea, writes:—"I may further say that I have the greatest horror of vivisection. I cannot understand how anyone can tolerate it. There is enough misery in the world without going out of our way to make animals suffer in this cold-blooded, fiendish way." I would like to point out that these are the doctor's words, not mine:—"This earth is almost a hell for many poor animals. It is an excuse for a morbid curiosity in many selfish wretches."

17210. Do these letters which you have read to us give us a general idea of the views which have been expressed to your society?—Yes, I think they do. Some, of course, are stronger than others.

17211. It would be impossible to take the evidence, you know, of 100 doctors in this sort of way. I think we cannot go on reading any more. We may take your word for it that those are fair samples of a number of letters that you have received?—Very well.

17212. You have told us about the Members of the House of Commons who are pledged to support your Bill?—Yes. Of course, as public opinion is shown in elections, I should like to say a word about it. As I said before, we approached every candidate during the last General Election with a view of obtaining their views on this subject, and we received a reply of some kind or another from most of them; and from the replies that we received we found that 380 Members of the present House of Commons are pledged to support the Bill. These candidates were approached not officially from the head office in every case; in some cases they were, but in most cases they were approached by some of the members of the League who happened to live in their constituency. In one or two cases we made a special effort either to return men whom we were anxious to return, or to oppose and upset the election of a man where we considered it was necessary to do so and in two cases I may say that two men who had opposed our Bill in the House in the previous year were, mainly through our instrumentality, defeated.

17213. Can you tell us who they were?—One was Dr. Hutchinson, the Member for Rye, and the other was Sir Mark Stewart, Member for Kirkcudbright in Scotland. They both opposed our Bill; I mean they blocked it in the previous Session, and our members felt that a determined effort ought to be made in those cases. A very determined effort indeed was made in Rye, and in spite of the fact that the party to which Dr. Hutchinson belonged was in the ascendancy at the moment, he was very badly beaten. That, I think, is only one example of the public opinion that we have behind us on this question.

17214. I am afraid that most Members of the House would think that it is rather a strong thing to say that Sir Mark Stewart was thrown out on the question of exemption or dogs from vivisection?—It may be so, but I am quite sure that we exerted great influence at the election.

17215. It may have had its effect, no doubt?—Perhaps I ought not to say that we threw him out. All I know is that he was thrown out.

(*Mr. Ram.*) It may have been *post hoc*, not *propter hoc*.

17216. (*Sir Mackenzie Chalmers.*) You opposed him and he fell?—Quite. It may have been a coincidence, but it is a fact for all that.

17217. (*Chairman.*) Then you wish to refer to Professor Schäfer's experiments?—I put that down in my *précis* simply because I think he has appeared before the Commission.

17218. It is not a subject that you have personal knowledge about?—No, only so far as regards the society opposing the experiments at the time. They

got up a petition, it was before my time, to the Home Secretary, and also a petition to Her Majesty the Queen, on this subject, to which they received a reply expressing her disapprobation of the experiments. They also obtained the opinion of 109 medical men, and among them were very strong letters in opposition to these experiments. This is the petition that we sent to the Home Secretary: "We, the undersigned medical practitioners of the United Kingdom, beg respectfully and earnestly to protest against the licence which has been recently granted by the Home Office to Professor Schäfer to submit unanæsthetised dogs to the process of drowning with a view to studying various means of resuscitation therefrom for the benefit of human beings. We would point out that previous well-known experiments of a similar nature have proved futile of result, and we are of opinion that the present experiments must be equally unsatisfactory and disappointing, by reason of the difficulties and fallacies associated with the anatomical and physiological differences between the animals operated upon and the higher creatures whom these procedures are supposed to benefit. But even if some slight gain in knowledge above and beyond that with which we are already acquainted were possible by such means (which we do not admit), we should, nevertheless, emphatically protest against such inhuman cruelty, perpetrated in the name of science, upon the helpless, sentient, and sensitive creatures. We trust, Sir, that you will yet use your great power and influence to put a stop to any further experiments of this nature, which can but bring discredit upon a Government Department, which possesses the power of control in questions where the exercise of justice and humanity are concerned."

17219. You tell us that that came into the hands of Her present Majesty the Queen?—Yes.

17220. And she sent you a letter?—Yes, a letter from the Honourable Charlotte Knollys to Lady Paget, who sent the petition to the Queen. I find that it is marked "private."

17221. Then perhaps we had better not have it. I should not like to admit a letter from the Queen marked "private" without Her Majesty's permission. I will take your word for it that Her Majesty disapproved of the experiments?—If you please.

17222. (*Sir John McFadyean.*) Has the letter been made public?—No, of course not; it is marked "private"——

17223. (*Chairman.*) What is the next point you wish to mention?—There is a reference in my *précis* to the "British Medical Journal," and to a letter by Dr. Silvester, which appeared in that journal at that time. I am sorry that in my hurry to come away I forgot to bring it with me; I daresay it will be known to the Commission. If the Commission will permit me to send it on again, I shall be glad to do so.

17224. We have had Professor Schäfer here, and I think that some of the witnesses who were objecting to his experiments read from the "British Medical Journal"?—Anyhow, may I send it on again.

17225. Is Dr. Silvester alive?—I really do not know. I think so. I am not sure.

17226. You have told us about your membership?—I may say that in addition to our membership, we have now a large number of centres all over the country. We have over 200 centres, and there is an honorary secretary in each place who is responsible for carrying on the work of the League, so that the influence of the League is really very far reaching. I should like to emphasise one fact, if I might. We believe, of course, feeling that we have a majority of the House of Commons in favour of our Bill, that the Bill has been already passed by the nation, and that it only awaits an opportunity to be passed by the House of Commons.

17227. I understand that one of the objects of your League is the total suppression of the support of vivisecting institutions by public money?—Yes.

17228. You mean hospitals that have laboratories?—We mean that; but what I had particularly in view in that was the support of institutions from the rates and taxes which are licensed for vivisection. We do not believe that Government Departments, such as the Board of Agriculture, the Local Government Board, and others, should have any laboratories licensed for this purpose. We do not believe either that any local institutions supported by county councils or local authorities should be used for vivisection.

17229. Would it not be more accurate then, to say the total suppression of the support of laboratories for vivisectional purposes by public bodies?—Yes.

17230. That is what you mean to convey?—That is what I mean exactly. We very strongly feel that the using of public money for this purpose is not only unfair and injust, but immoral.

17231. And you object to the proceedings of the Local Government Board and the Colonial Office for the purpose of ascertaining the causes of disease and the remedies?—Yes, quite so. We object to vivisectional laboratories for those purposes because we do not believe that they are needed.

17232. Then you desire, what I hope we all wish to do, to raise the standard of humane feeling in the country?—Yes, that is really one of the great objects that we have in view.

17233. And the view of your League, I understand, is that vivisection does not do any good, which is a matter on which you do not profess to give expert evidence, but also that even if it did, dogs ought not to be experimented upon?—Yes.

17234. That that would not be sufficient cause?—Quite so.

17235-6. Then you have some notes about the Buisson bath propaganda. Would you explain what that means?—That arose at the very commencement of the League's operations in connection with the rabies scare. The views of the originators of the League were that the rabies scares were really got up largely in the interests of Monsieur Pasteur's Institutions, and they adopted the Buisson bath as an antidote to hydrophobia and found that in all cases where it was tried it was a very great success. I may say that this also was before my time, and I am only speaking from hearsay, not actual experience. These baths are, I understand, designed to bring about a free action of the skin.

17237. Have you not some witnesses coming who understand this from the medical point of view?—I am afraid I cannot do that, because I am only allowed to bring two witnesses, and I cannot very well bring witnesses who are experts on all points.

17238. We were obliged to select from each society a limited number. We have a very long list?—I quite understand that, but we rather feel that you have limited us rather more than other societies.

17239. I do not think so?—Well that is my view.

17240. Would you tell us without professing to argue for the medical merits of this system, what it is?—It is a vapour bath, the object of it is to take out of the system any virus that may have been injected into it by the bite of a rabid dog, and my society believes that this is really a more commonsense method of dealing with the question than by injecting a virus of the same character as that which has been already injected.

17241. Your society, I suppose, is governed by a committee?—Yes.

17242. Who express the views of the Society?—Yes.

17243. Are there any medical men upon it?—Yes, there are three.

17244. Will you tell us who they are?—Sir James Thornton, Dr. Hogg, and Dr. Bayfield. In addition to that we have a number of medical men who are vice-presidents, who also, of course, are of the same opinion as the committee. I could give you the names of those if you wish it.

17245. I daresay that some of your papers have their names on them?—Our letter paper has them.

17246. That will be quite sufficient?—Dr. Charles Bell Taylor is one of our vice-presidents, Dr. Carpenter is another, Sir Francis Cruise, Honorary Physician in Ordinary to His Majesty the King in Ireland, and Sir Robert Jackson.

17247. (*Sir John McFadyean.*) Might I ask whether the personal opinion of each of these gentlemen has been obtained on this particular point, that is to say, as to the failure of the Pasteurian method and the success of these baths in every case?—No, we have not canvassed them on those points.

17248. Are you sure that they approve of what you

are telling us?—I am sure that they do not disapprove, or else they would not associate themselves with the society.

17249. It is not in the constitution of the society is it, that it must believe that the Pasteurian treatment is inefficacious, and not in keeping with common sense?—Well, they have a copy of our Report every year, in which we refer most pointedly to this question, and the gentlemen I named last have only recently become vice-presidents, and they have not done so without knowing exactly what they were doing. Of course, they must generally agree with everything the society does or else they would not associate themselves with the Society.

17250. (*Chairman.*) I presume that all your members are in favour of the exemption of dogs from vivisection experiments?—Yes, and all the other objects that we have in view. We have a large number of objects besides those.

17251. Yes, I mean for the protection of dogs generally from cruelty?—Yes.

17252. But I was coming to the point about the Buisson bath?—That is certainly one of the objects of the society, and one of the forms of the work that we have been prosecuting.

17253. (*Mr. Ram.*) Is that stated publicly as being one of the objects of the society?—Yes.

17254. In what publication?—In the Annual Report.

17255. Can you give me one?—I have only one that is marked. I can send you one.

17256. Can you read me the words that would show to any intending vice-president that he was committing himself to the refutation of the Pasteurian theory and the adoption of the bath?—We have here in one Appendix particulars of how this thing is to be done.

17257. How what is to be done?—How we are prepared to assist people who want to use the Buisson bath.

17258. But I want to see whether a vice-president would necessarily know that on becoming a vice-president he was committing himself to disbelieving in the Pasteurian theory and adopting the Buisson bath. If you will show me that I shall be obliged?—There is a marked reference in the Report itself, both of last year and this year, in fact every year, to this work, which cannot fail to be brought to their notice.

17259. (*Chairman.*) I think that one of your medical witnesses who is coming might very well qualify himself to give us a really medical opinion upon this subject?—Yes.

17260. That would be better than taking it from one who does not profess to be a medical expert?—I will do my best to get a witness to do it. I cannot undertake definitely to do so, but I will do my best.

17261. You see there are gentlemen here who are familiar with medical questions?—Yes, I quite understand that.

17262. (*Sir William Collins.*) Do you state anywhere what are the precise objects of your association?—Yes, in our Report.

17263. Will you read what are the objects for which the association is formed?—"Organised opposition to the muzzle. This includes the repression of dog scares, opposition to Pasteurism, and a Buisson bath propaganda." That is what I was asked for just now. That is the first object that we put down.

17264. (*Chairman.*) Which Report are you reading from?—This is in every one. This is our last published Report.

17265. Is that passage that you have just read in your first Report of 1891, because you said it began with muzzling?—I cannot say; I do not know what the Report of 1891 was; but this is really an object of the society at the present moment. The second object we put down is "quarantine business of every kind"; that is to say, we assist people who want to bring their dogs over from the Continent by giving all information and assistance in the way of quarantining dogs.

17266. (*Sir William Collins.*) Is not the object defined more definitely than stating "quarantine business of every kind"?—That includes everything connected with quarantine.

17267. That might mean either that you were in favour of it or opposed to it. Is your society incorporated?—No, it is not.

17268. Is it registered?—No.

17269. Amongst the objects of the society is there anything stating a general opposition to the practice of vivisection?—No, not general opposition to the practice of vivisection, opposition to the practice of vivisection of dogs.

17270. Then so far as vivisection is concerned, is the sole object of your association to free dogs from the practice?—Yes, so far as the object of our society is concerned.

17271. Then your society, as such, would not oppose the vivisection of guinea-pigs or frogs?—I am not prepared to go into that at the present moment. We are a dog society.

17272. But if the society does not go into that, should I be wrong in concluding that it is not an object of your society to oppose vivisection of guinea-pigs or frogs?—It is not an object of the society to do that.

17273. Or higher mammals, such as the ape?—We cannot go beyond the dog. We are a dog society. I have every reason to believe that practically the whole of the members are against vivisection, as such; but, as a society, we cannot do that.

17274. Then your society, as such, does not oppose the vivisection of the ape?—Not as such. We have taken no measures to do that.

17275. Does your society oppose the vivisection of dogs, if the dogs are rendered anæsthetic throughout the operation?—Yes.

17276. Then if the dog were excluded from the operation of vivisection your society would, so far as vivisection is concerned, have exhausted its object?—Well, I suppose so. I cannot say what action they would take if they succeeded in excluding dogs from vivisection—whether they would go on to some other animal. But at present we are certainly confined to dogs.

17277. But your society, as such, *qua* vivisection, limits its operation to dogs alone?—At present.

17278. You make no claim to speak, I understand, on the scientific aspect?—No, I do not.

17279. (*Sir John McFadyean.*) Your society at present numbers 4,421 members?—Yes.

17280. Can you tell me how the sexes are distributed?—I could not.

17281. Are the majority of the male or female sex?—I should say the majority are ladies.

17282. What is the income of the society?—Last year our income was £1,698.

17283. Can you tell us how it is mainly expended?—Yes.

17284. (*Sir Mackenzie Chalmers.*) You publish a balance-sheet, I suppose?—Yes. Our printing last year cost us £664; elections, lectures, dog shows, memorials, travelling expenses, £133; advertisements, £116; office rent, £80; salaries and wages, and extra clerical assistance, £487.

17285. (*Sir John McFadyean.*) I think that is enough, thank you. Can you tell us what your clerical expenses would be at the last election?—The amount I have given, £133, covers that.

17286. It is mixed up with these items?—Yes.

17287. In what sense did you oppose Sir Mark Stewart when he was a candidate?—We sent people up there to speak, and we also issued leaflets calling special attention to what he had done on this question.

17288. You sent people who addressed public meetings?—Yes.

17289. Were these largely attended?—Yes.

17290. Can you tell me where any one was held?—I could not at the moment; I can give you the particulars later on.

17291. Do you know whether the successful candidate thought that you had lent him any assistance or not?—I really could not say. I fancy he did.

17292. Have you had any number of letters or statements from voters to say that your views had influenced them?—In that constituency, yes.

17293. How many?—I could not say off-hand.

17294. Five, twenty, one hundred?—I really could not say.

17295. You are sure that you had five letters?—Oh! yes, a good many more than that.

17296. What do you mean when you use the expression "the rabies scare"?—What we mean is that orders were given by the Board of Agriculture to muzzle dogs, on the ground of rabies being in certain districts. We deny that rabies was there.

17297. Is this a point on which you are prepared to offer direct evidence, and not hearsay evidence?—No, it is not.

17298. Then who is going to come? Who are "we," when you say: "We maintain that these were not cases of rabies"?—The society.

17299. Do you mean to say that the whole 4,421 members are entitled to hold an opinion on that point?—I think I am quite entitled to say that, since they are members of the society and connected with it.

17300. Then, on the average, they are better instructed on that point than you are? They are better entitled to form an opinion on that question that you are? I put it to you. It may appear an affirmation, but it is a question?—No, I do not for a moment profess that every one of the 4,421 members would be prepared to come here and give evidence on the subject; but they have their opinions on the subject. So have I. You asked me was I prepared to give expert evidence.

17301-2. I did not use the word "expert"?—I beg your pardon; I understood you to say so.

17303. No; I said "direct"—direct evidence as opposed to mere opinion?—Yes.

17304. You say that you do not desire to offer evidence on that, but that it is the opinion of your society?—Yes; I am quite prepared to bring you cases.

17305. Cases which were not cases of rabies?—Yes.

17306. You are prepared to give evidence on that?—Yes, I am quite prepared to bring forward cases which were not cases of rabies, but were returned as cases of rabies at that time.

17307. Do you know anybody who ever held that the statistics with regard to rabies were accurate in every case?—No; everybody, I think, held that they were absolutely inaccurate.

17308. Everybody did?—Everybody who knew anything about it, because the data from which the statistics were obtained were inaccurate.

17309. Just a moment, please. Everybody, you say, who knew anything about it. Then the expert officers who were employed by the Board of Agriculture were among those who knew nothing about it?—No, I do not think that they believed in the Rabies Returns.

17310. Will you give us some evidence to show that they disbelieved them?—It is very evident, from the fact that they changed the data upon which this evidence was taken so frequently, that they had no confidence whatever in the statistics given.

17311. Please give us particulars as to the way in which they changed the data?—They first of all based it on autopsy. In the next year they decided on another form—I can give it to you in a moment—the inoculation tests were brought forward. I have reasons for believing that in the first year, when the test method was adopted, the number of rabies cases returned was very much greater, simply because it was an improper way of doing it.

17312 What was there improper about it?—In this way, every man, almost, even the policeman, in many cases, I understand, was allowed to declare a dog rabid, without any evidence at all, except his own opinion.

17313. Where do you find it laid down that the policeman might make the diagnosis?—I am told so.

17314. Who told you?—I have been told so several times. I could not quote any authority, but that is the impression on my mind.

17315-6. That is not evidence before this Commission. But did you approve or did you disapprove of the attempt of the Board of Agriculture to make the statistics more accurate by introducing the inoculation test?—We approve, of course, not of the inoculation test as such, but of every effort that is made by the Board to be accurate.

17317. Then I will put it the other way: Did your society disapprove of the attempt of the Board to make the statistics more accurate by resorting to the inoculation test?—I do not quite understand your question.

17318. I will spend a little time in making it plainer, if I can. You complain that the statistics were very inaccurate and misleading when the diagnosis was based on something else than the inoculation test?—Yes.

17319. The inoculation test was introduced in order to make the statistics more accurate, and I understood from you just now that you are prepared to admit that they were more accurate afterwards?—They were more accurate afterwards, but I do not admit that they were accurate.

17320. Did your society approve of this procedure in order to make the statistics more accurate?—They certainly approve of every method being adopted, if it is a proper method.

17321. Then they approved of this method?—I cannot say whether they did, or did not, approve of the inoculation test as such. They certainly are very desirous that the figures should be accurate, but whether the mode adopted for that purpose received the approval of my society, I am not in a position to say.

17322. This is not a scientific question; it is a moral question and an ethical question, and I will put it direct to yourself. Do you approve of that method of endeavouring to obtain accurate information regarding the occurrence of rabies in this country?—You say that it is not a scientific question. I must reserve to myself the right to an opinion on that question; inoculation is certainly a scientific question, but I say that I approve of every proper method which may be used for the purpose of accuracy.

17323. That, of course, is not answering my question, because you introduced the word "proper"?—But you say that it is not a scientific question; I say that inoculation certainly is a scientific question, and if you ask me whether I approve of inoculation you enter on a province of science.

17324. I am afraid that you have misunderstood me. I did not say that this was a scientific question?—I understood you to say that it is not a scientific question.

17325. The question I put to you was not a scientific question, it was a moral question. You admit that these scientific questions have a moral and ethical side, because you have come here specially to speak upon it?—Quite so.

17326. Very well, then; has this method of inoculation, which is a scientific method, got an ethical side?—I disbelieve in inoculations entirely.

17327. Then you disapprove of this particular method of endeavouring to make the statistics accurate?—That is a scientific question on which I am not prepared to give an opinion.

17328. Do you mean that the question, whether you disapprove or not, is a scientific question?—The question whether inoculation is the right way of finding out the correct figures is, I say, a scientific question.

17329. But I have not put that question. I will ask you, please, to assume that it is a good method, and that it actually does enable you to obtain accurate information?—If that is so on those grounds, I have already said that I approve of every proper method of finding out correct figures.

17330. But do you approve of this method?—I cannot say whether it is a proper method.

17330A. But I ask you to assume that it is what it professes to be, an accurate method. Do you approve of it?—If it is what it professes to be (I cannot have an opinion upon that), I certainly approve of any proper method for the purpose of finding out correct figures.

17331. That may be held to mean that you approve of this particular method if it is accurate?—If it is accurate and proper.

17332. What do you mean by the word "proper"?—Right, morally right.

17333. And what I am asking you is whether you

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think it is morally right?—I did not understand you to ask me that. You asked me whether I approved of it.

17334. But, of course, I tried to make it plain that that was whether you approved of it from the moral or ethical standpoint?—I have already said that I totally disapprove of every process of inoculation, because I do not think it is a proper one.

17335. May I take that as meaning that you disapprove of this particular method?—From that point of view I do.

17336. So that from the point of view of your society they would rather have had the statistics inaccurate than have had them made accurate in this way?—We do not for one moment say that this is the most accurate way of finding out statistics. What we do say is that they certainly were more accurate when they took this method of finding them out, but we say that even then they were inaccurate, because we have cases of dogs having been put to death for rabies during that period which on *post-mortem* examination were proved not to be suffering from rabies at all.

17337. (*Chairman.*) Put to death by whom?—By the authorities.

17338. (*Sir John McFadyean.*) Did these animals which were killed, as being the subject of rabies, and which were afterwards found to be not suffering from rabies, figure in the statistics as rabies?—So I understand. For example, the chairman of my society, Mr. Pirkis, had a dog which was condemned as being a rabid dog, and was put to death. They insisted on a *post mortem* examination, and when the *post-mortem* took place it was found to be that what the dog was suffering from was that it had swallowed a piece of leather, which stuck in its throat, and, of course, made it very ill, and caused the people to think it had rabies.

17339. Then it was proved that it was not rabid simply by the *post-mortem* examination?—Yes.

17340. Do you think that a reliable method of telling whether a dog is rabid or not?—I am told so. I am not in a position to give an opinion myself.

17341. But I thought that a minute ago you endeavoured to discredit the statistics of the Board, by pointing out that they were based on *post-mortem* examinations?—No, I did not; inoculations, I think I said.

17342. You think that *post-mortem* examination is more valuable than inoculation?—I should think so. Of course, as I said before, I am not giving a scientific opinion; I cannot do that, but I should imagine so.

17343. You are aware of the extraordinary reduction in the cases of rabies in this country which followed the introduction of the muzzling order?—I am aware of the reduction in the figures, but, as I said before, my society did not believe in the existence of rabies to the extent that they were returned by the Board of Agriculture.

17344. Does your society believe that there is such a thing as rabies at all?—Yes, but they believe that it is a very rare thing.

17345. How many of these cases, say in the year 1895, when there were 672 cases, do you think were cases of rabies?—Very few.

17346. How many is very few?—I really could not say.

17347. One, two, or three?—It is quite impossible to say.

17348. Do you think it is possible that there might be none of them cases of rabies?—Quite possible.

17349. So that all those who have given special attention to the subject, including veterinary surgeons and medical men, are entirely mistaken?—Probably.

17350. Why probably? Does it not seem probable that they would be right rather than wrong?—Perhaps I ought to have said possibly instead of probably.

17351. So that you do not attach even any importance to the fact that no cases of rabies have been returned during the last few years at all?—We do not attach the same importance as the Board of Agriculture do; because we do not believe that rabies existed to the extent that they said it did in previous years.

17352. Do you think it exists at all now?—Not in this country.

17353. Do you think it existed at all before the Muzzling Order was introduced?—I am not in a position to say that.

17354. So that it is actually debatable whether there has been any rabies in this country?—Yes.

17355. Do you wish to give evidence before the Commission based on your own reading and observation to bear out that view?—I am not in a position to do so.

17356. Whose opinions are you quoting?—I am quoting the opinions of my society, so far as I can gather them, previously to my coming into connection with it. Personally, I have no experience of that during the last three years; fortunately, there has been no occasion for it.

17357. But do you think that these 4,421 members are entitled to a first-hand opinion on that subject?—I think they are entitled to an opinion on anything they like. We are all entitled to an opinion on anything we like.

17358. I am afraid we use the word "entitled" in a different sense. I meant were they in a position to form an opinion that this Commission ought to attach any weight to, on a question like that?—No, I do not say they are. They are not experts, of course.

17359. Then you said in your evidence that the difference between the canine and the human organs was so great as to make investigations conducted on dogs useless as a means of extending knowledge with regard to human diseases?—So I am given to understand.

17360. But, again, you do not wish to give scientific evidence as to that?—No, I have already said that I intend to bring a witness forward to speak specially on that subject.

17361. How does the question stand with regard to experimentation on dogs for the purpose of extending knowledge with regard to dog diseases. That argument would not apply, would it?—This particular argument would not apply.

17362. Do you think that knowledge with regard to dog diseases might be extended by experiment?—It might be, but it is very questionable whether it would be right to use that method.

17363. Supposing that it were a question of investigating the skin parasites of the dog, the flea and the louse, for instance—would it, in your opinion, be a moral proceeding to transfer those parasites from a dog in which you found them occurring naturally to another one, that they might breed upon it, and afford you an opportunity to investigate their life history?—No, we certainly think it would be quite wrong to do that. We see no reason in the world why that could not be studied on the animal which suffers naturally from it.

17364. But, assuming that it would be convenient to the investigator, and provide him with a larger quantity of material, you think it would be morally wrong?—Yes, I do, certainly.

17365. Then do you take up the view that it is morally wrong to cause pain in any circumstances to a dog?—Yes, quite.

17366. Is it wrong to chastise a dog with a whip?—Chastisement comes, of course, under a different category. I do not believe myself, personally, in it.

17367. It comes under the same category?—I do not believe it is at all necessary to chastise a dog. I never chastise my dog. I find that it does all I want without it, and does it better by kindness. I do not think it is necessary ever to do that.

17368. That was not my question. My question was: Do you think it is morally right or wrong to chastise a dog?—I think it is morally wrong to do it unnecessarily.

17369. Does your statement that it is morally wrong to cause pain to any animal in any circumstances apply to other animals than the dog?—You are asking for my personal opinion, I presume; my society does not go beyond the dog.

17370. You could not possibly give me the opinion of your society on that question?—I certainly think it is wrong to give unnecessary pain to any animal.

17371. But you have introduced a most important

qualifying word, "unnecessary." That we would all assent to. But what you said before was that it was morally wrong to cause pain to an animal under any circumstances?—To a dog, I said. I see no necessity for it. That is why I say it is morally wrong.

17372. But that is not the question, because you introduce the word unnecessary, which begs the question?—I think I explained to you why I did not chastise my dog—because it is not necessary. I find I can do better without it.

17373. I did not ask you whether you chastise your dog or not. But you admitted that in your opinion, to inflict pain on a dog in any circumstances is morally wrong. Have you answered the question as to whether it is ever morally defensible in the case of other domesticated animals?—I still say that it is absolutely wrong to give unnecessary pain to any animal.

17374. But my question, please observe, was not whether it was right to cause unnecessary pain, but whether it was ever justifiable to cause pain?—I cannot say that there might not be cases in which it is justifiable. I cannot at the moment think of any, but I would not like to at once make a sweeping assertion that every possible case of that kind is wrong. But I certainly again say that every unnecessary pain inflicted on an animal is an evil, and morally wrong.

17375. I have not asked that question. But you say you feel unable to think of any circumstances in which the infliction of pain on an animal would be justified?—I cannot think of any at the moment.

17376. Then it was hardly necessary to introduce the word "unnecessary." Is it justifiable? Have you read the evidence given by previous witnesses on that subject?—I have read some of it; I have not read it all.

17377. Do you think it is morally wrong to kill an animal for food purposes?—No, I do not.

17378. Do you think that the pain inflicted in that case is necessary?—I certainly think they ought to be killed by the most humane methods possible.

17379. But supposing it were not possible to kill them without pain, is it still morally defensible to kill them?—But I think it is possible, from what I understand of the question; so that I do not see why we need discuss it.

(*Chairman.*) Shooting pheasants, for example?

17380. (*Sir John McFadyean.*) Is that painless?—I cannot say really. I do not know whether it is or not, but I say that if a pheasant is shot it ought to be shot in the most painless way possible, and no man has a right to shoot it unles he learns how to do that unerringly.

17381. (*Chairman.*) Supposing it gets shot in its body and not in its head, it suffers a good deal of pain?—I do not think that any man has a right to kill it unless he does it in the most humane manner possible.

17382. (*Sir John McFadyean.*) The most humane consistent with what—the pleasure of the man shooting?—I think the man's pleasure ought to be secondary altogether. The first thing ought to be the feelings of the animal that is being shot.

17383. I suppose you must be aware that shooting pheasants and other game, as it is ordinarily conducted, causes pain to the majority of the animals that are killed?—I suppose it is so.

17384. Is that morally right?—Well, of course, that raises the whole question of sport. I do not think it is.

17385. Is it morally more justifiable than the painless experimentation on animals with a view to extending human knowledge?—An animal that is killed for the purpose of providing food is, in my estimation, in quite a different category from animals killed for purposes of sport, although they may be used for food afterwards.

17386. Does that apply to pheasants bred on purpose that they may furnish sport?—Yes, I do not believe in that.

17387. Do you mean that you do not believe there is such a thing?—I do not believe it is right to breed pheasants or any other animal for the purpose of sport, though they may be used for food afterwards. They are not killed for the purpose of food; they are killed for the purpose of sport, and I disapprove of that.

17388. Is it morally less defensible than painless experimentation on animals?—I consider that they are both indefensible.

17389. Would you have the preservation of game for purposes of sport put down by law?—I would. I would like to say that I am not speaking for my society now; I am giving my own personal opinion.

17390. But supposing that your society is successful in its immediate objects, you would be in favour of their perhaps trying to influence public opinion so as to have sport put down?—There are societies for that purpose, and they have my warmest support personally, but I do not think my own society would enter into that at all, as it would be outside its province.

17391. Would it be justifiable to ride a horse to death in order to save the life of a human being?—That is a very difficult question. I do not think it would.

17392. It ought not to be a difficult question; I should not find the question at all difficult, and I expected you would not either; but it seemed to me to be a good case to define what is the attitude of your society with regard to these things?—I do not think it would be justifiable.

17393. Did you draft this petition to Parliament?—No, I did not.

17394. Do you approve of it?—Yes, I do.

17395. The second paragraph reads: "Your Petitioners are furthermore of opinion that, on account of this docility and obedience to the word of command, the dog has of late years been specially selected by vivisectors for extensive and peculiarly revolting and painful experimentation." Can you tell me what is the evidence that the dog was selected for experiments on account of its docility and obedience?—It is a well-known fact that the dog is very much more easily managed than any other animal.

17396. Than a sheep?—Than a sheep.

17397. Is it much more docile than a sheep?—I would not say more docile.

17398. As docile as a sheep?—Yes, quite as docile. It is more intelligent than the sheep, of course.

17399. But it was its docility that was specially mentioned?—That comes in "Docility and obedience to the word of command"—come in together. It is more intelligent than a sheep, and therefore more easily managed.

17400. Do you suggest that in experimentation a dog is managed by merely ordering it to do this and that—that the question of obedience is of real importance when an animal is to be operated upon by a physiologist?—I think so; it gives him less trouble, I should think, judging from what I gather.

17401. At what stage of the operation does it give less trouble?—At all stages.

17402. Do you mean that it is easier to catch a dog and secure it to a table than to catch a sheep?—Yes.

17403. You mean that the experimenter is in more danger of being bitten by a sheep, perhaps?—I do not mean that.

17404. In what respect is it more convenient to use a dog?—Because a dog can be persuaded to do almost anything.

17405. Do you suggest that when a physiologist proceeds to prepare an animal for experiment, he uses his power of persuasion, in the ordinary sense of the word?—I might refer to what Professor Starling said before your Commission. He said that he had known a dog which had undergone an operation and had been brought in after it had been kept for observation a certain time, and when it was brought in it licked the hands of the experimenter, and wagged its tail, and showed every sign of joy at seeing him again. I certainly repudiate most strongly the conclusion that Professor Starling came to from that. He wanted to make out that the dog was glad to see the experimenter, because he wanted to be experimented on again.

17406. (*Chairman.*) Do you say that Professor Starling said that?—That is the conclusion that he came to.

17407. I suppose from what he said?—I take it so, from what he said.

17408. Did he say that he believed the dog licked

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his hands because it wanted to be experimented on again?—I believe he brought it forward to prove that dogs are not averse to being experimented upon.

17409. You say that he meant the Commission to understand that?—Yes, I thought so.

17410. (*Sir John McFadyean.*) But that is really quite beside the point that I wished to receive information about?—I think that illustrates the question. You asked me why I think that a dog is more easily managed than a sheep. I say that Professor Starling himself has proved it. At least, he believes that, I should imagine from his evidence.

17411. Are you aware that, with the rarest possible exceptions, dogs when they are subjected to vivisection operations, are secured to a table?—Yes, I suppose they are—that is, for physiological experiments.

17412. That is for vivisectional experiments in the literal sense of the word?—Yes, quite.

17413. The dog has to be forcibly tied to the table?—Yes.

17414. In what sense are its docility and its obedience of advantage there, as compared with a sheep?—It is very much easier to persuade a dog. A sheep is stupid.

17415. But you do not persuade. You take the animal forcibly and lay it on the table, and tie its legs down. In what respect is it of advantage to the experimenter to select a dog for this purpose, and as a rule a strange dog?—I think that if I were going to experiment upon animals I would very much prefer to select a dog, because I know that I could manage it better than any other animal.

17416. Have you any knowledge of sheep?—No, I have not.

17417. So that it is possible that your opinion as to the advantage of a dog as compared with a sheep is entirely mistaken?—It might be, of course.

17418. Have you any knowledge of the pig?—I have no expert knowledge. I have seen one. I cannot say very much about it.

17419. Do you think it would be easier to secure an ordinary strange dog than an ordinary small pig?—I should think so.

17420 Why?—Pigs are so stupid, for one thing.

17421. But I have already pointed out that you do not ask for any assistance from their intelligence. You lift the animal forcibly off the ground, and tie it to the table?—That is where we must disagree, I think. It appears to me quite unnatural to think that an experimenter would not take advantage of the natural characteristics of a dog as compared with a sheep or a pig for the purpose of helping him in his experiment.

17422. Do you think that the experimenter relies on assistance from the dog?—I should think so. I should think it would be to his advantage to do so.

17423. (*Chairman.*) Are you not assuming that this dog is a dog that has known the owner for many years?—No, I am not afraid of any dog.

17424. You think that a strange dog would do all it could to assist the man in his experiment?—Yes.

17425. (*Sir John McFadyean.*) Supposing that the operation is to open the dog's abdomen to study some point of physiology in connection with the liver, would you please tell us the assistance that you would expect a strange dog to give you?—I cannot; that goes beyond my province. I cannot give you any view at all as to anything of that kind, upon the scientific side of the question. I do not know the nature of the experiment.

17426. You are aware that in most cases the first step, or a step at any rate before any cutting takes place, is the administration of anæsthetics?—I suppose so.

17427. Tell us what assistance you expect a dog to give you in that stage of the operation?—After the anæsthetic?

17428. Before; the assistance that you would not get from a pig or a sheep?—But it would not resist to the same extent.

17429. (*Sir Mackenzie Chalmers.*) But when it is under the anæsthesia what is the difference?

17430. (*Sir John McFadyean.*) Is the advantage lost as soon as the animal becomes unconscious?—I cannot say. I am told that it cannot be anæsthetised, of course.

17431. But you do not wish to give evidence on that point, do you?—No, I do not wish to give evidence on the question, because, as I said before, it goes beyond my province entirely.

17432. But still you approve of that statement in this paragraph?—So far as I know dogs, I think it is quite true, and I know them fairly well now. I have found dogs both docile and obedient to the word of command, and it stands to reason that it is easier to experiment on an animal of that kind than upon one devoid of those characteristics.

17433. (*Mr. Ram.*) Under anæsthetics?—No, before that.

17434. If it is under anæsthetics, where is the difference?—I am told that it is not anæsthetised. That you cannnot anæsthetise a dog.

17435. That is what you base yourself upon?—Yes.

17436. (*Sir John McFadyean.*) I did not doubt that you believed that; but I have spent about ten minutes in trying to get you to give reasons, which would satisfy the Commission as to the soundness of your conclusions; and you have not been able to give any that satisfied me?—I can give no better reason than my knowledge of dogs.

17437. But you admitted that you had no knowledge of sheep?—I have the same knowledge of sheep.

17438. Then why institute a comparison between a dog and other domestic animals?—I do not think it is necessary to have an intimate knowledge of any animal to institute a comparison.

17439. Do you know the statistics regarding the operations performed under a licence and certificates?—I cannot say. I have seen them.

17440. Can you tell me what particular operations are referred to in this petition as "peculiarly revolting and painful"?—I imagine the reference was, I do not know, to Professor Crile's experiments.

17441. What makes you think so?—Because I think this petition was drafted about that time, or shortly after that. I think they had that in view.

17442. So that if Dr. Crile's experiments were of a painless character, you would be hard put to it perhaps to justify that expression?—I do not think so. I think they would be able to find others.

17443. Will you tell me some others?—We consider that Professor Schäfer's experiments come under that description.

17444. They were "peculiarly revolting and painful," were they?—I think so. We considered them so.

17445. And others?—I do not know that I could tell you off-hand now. No doubt I could find a large number upon which you could base this. I have not the slightest doubt I could.

17446. You think you could find a large number that would be correctly described as revolting and painful?—I think so. But even if we could not I think the two cases that I have already cited are quite sufficient to justify this expression.

17447. But still you say you would have no trouble in getting a large number?—I think not.

17448. Will you undertake to look up a large number and add them to your evidence?—Yes, I will.

17449. Giving particulars as to time and place and so on?—Yes, I shall be delighted.*

17450. Then you do not think it was too strong language to use when they spoke of "Demonstrations of a prolonged and agonising nature before classes of students"?—No, I do not think it is too strong.

17451. Can you tell me any particular experimenter who has within recent years in demonstrating to a class of students caused prolonged and agonising suffering to an animal?—I cannot at the moment.

17452. Do you think you can find examples if you have time?—Yes, I think so.

17453. You think you could find a large number of cases?—A number of cases; I cannot say a large number of cases. I do not know how many, but I will find as many as I can for you.

17454. You will find as many as you can?—Yes.*

Further particulars were not supplied by the witness.

17455. Thank you very much. You quoted Mr. Woodroffe Hill's opinion. Do you know whether he is entitled to be considered an expert on the anæsthetising of dogs?—I think so. He is a dog specialist. He has published several books on dogs, and has made a very careful study of dogs in every sense of the word.

17456. Is it within your knowledge that he has anæsthetised twenty dogs in his life?—I have only his word for it. He has told me that he has done so—at least tried to do so.

17457. He has tried to do twenty?—I do not know about twenty. I cannot say how many. He has told me he has tried to anæsthetise dogs.

17458. (*Sir Mackenzie Chalmers.*) And failed?—Yes.

17459. (*Sir John McFadyean.*) Would you admit that one might measure the value of his opinion by getting to know how many dogs he had tried it on?—I should say so.

17460. You would personally yourself attach most weight to the opinion of those who had most experience, other things being equal?—Naturally.

17461. At the present time your own experience and observation do not entitle you to form an opinion on this question?—Not at all.

17462. You are adopting the opinion of Mr. Woodroffe Hill, I think?—Yes, and others who have had experience in the matter.

17463. You would be prepared to abandon that opinion if you had laid before you the views of others of a contrary character based on a very much larger experience?—Quite so, if they convinced me that their experience was of such a character as to be reliable.

17464. I want to tell you that last week we had evidence given here from a gentleman, who had administered anæsthetics successfully to many thousands of dogs, who had actually administered it, and who had tabulated the results of the administration of anæsthetics to 1,200 consecutive cases, with only, I think, five deaths; and that many of these animals were under the influence of anæsthetics for a long period, up to five hours. Would you advise the Commission to attach greater weight to that evidence or to Mr. Woodroffe Hill's?—Of course the Commission is in a better position to judge than I am. I have not heard this gentleman's evidence. I take your word that it is so. The Commission are in a much better position to judge which of the two opinions to take. I would not presume to advise the Commission as to anything.

17465. Has the fact, which I have just told you, that we had this evidence given to us raised any doubt in your mind as to the soundness of the conclusions you had previously arrived at?—I am always prepared to accept the evidence of people who know more about a subject than I do myself. And I shall very carefully read the evidence to which you have referred, in order to be able either to revise or to justify my opinion.

17466. The witness is Mr. Hobday?—Thank you.

17467. (*Sir Mackenzie Chalmers.*) May I take it generally that your society hold the opinion that dogs cannot be anæsthetised?—Yes, that is the opinion of my committee.

17468. And even if they could be anæsthetised, that would not remove their objection?—Oh, dear me, no.

17469. In your petition you mention that dogs are used for "demonstrations of a prolonged and agonising nature before classes of students"?—Yes.

17470. You are not prepared with any evidence on that subject to-day?—No.

17471. Are you aware that under the law as it stands a dog in a demonstration must be absolutely under anæsthetics, and must be killed before it comes out?—So I understand.

17472. Do you know at all on what evidence that assertion was based?—No, I do not.

17473. It was before your time?—This petition was drafted before I became connected with the League.

17474. How long have you been secretary?—Just under three years.

17475. Do you devote the whole of your time to it?—Yes, practically.

17476. Then there is a paid secretary in London and honorary secretaries in the country?—Yes.

17477. The present committee are responsible for this statement, of course?—Yes.

17478. This petition is still being used?—Yes.

17479. But you do not know on what evidence it was founded?—No, I can ascertain for the Commission.

17480. I suppose you will agree, at any rate, that if the statements are correct, the operations were illegal?—Yes.

17481. You do not know whether the attention of the Home Secretary was called to any of these illegal operations?—No, I am not aware that it was.

17482. I am afraid you cannot help us on that. I should have liked some information with regard to it. You know dogs suffer from distemper a good deal?—Yes.

17483. It is a painful disease, is it not?—Yes.

17484. Would your society hold that any experiments performed on healthy dogs for the purpose of finding out the cure or better treatment of distemper are justifiable?—They would, of course, highly approve the study of distemper in dogs which were already suffering from it; but they would totally disapprove of any inoculations or any experiments performed on healthy dogs for the purpose of giving them distemper, even with the view of finding out a cure or better treatment of the disease.

17485. Even for the purpose of saving future generations of dogs from distemper?—Yes.

17486. And that would apply to all canine diseases, of course?—Yes.

17487. Are you aware that veterinary surgeons perform very severe operations on dogs for curative purposes?—Yes, exactly the same as medical men do on human beings.

17488. Does your society hold that those operations are justifiable, if a dog cannot be properly anæsthetised?—Yes, I think so, if it is for the good of the dog itself.

17489. Whether it can be anæsthetised or not?—Yes, for that particular dog.

17490. If the operation is severe would it not be much more merciful to kill the poor beast?—It depends. If there were any hope of its recovery I should not say it would be more merciful to kill. If there is no hope of recovery, certainly it would be more merciful to kill it.

17491. Does your society agree that superfluous dogs ought to be destroyed—No, we do not believe in destroying dogs.

17492. For instance, take the Dogs' Home, where about 30,000 dogs are killed every year?—We do not approve of that.

17493. What do you suggest as an alternative?—We take measures to protect all stray dogs that come into our hands. We send them to our own home and find homes for them. We find people who take charge of them.

17494. But you are not prepared to provide for 30,000 canine orphans every year, are you?—I do not think we should find much difficulty in finding homes for them. We have no difficulty now in finding homes for hundreds which pass through our hands annually.

17495. Am I right that about 30,000 are now destroyed in the Dogs' Home in London alone?—I do not know the figures, but I know there are a large number.

17495A. However, your society disapproves of that?—We do not believe in it. If a dog is suffering from an incurable disease, then it is a greater piece of mercy to put it to death, and that we do not disapprove of; but we do not believe in the wanton destruction of dogs.

17496. I imagine that your society disapprove of the practice, for instance, of spaying bitches to prevent their breeding, which is very common?—As a society I do not know that they have any opinion as to that. I have not heard it expressed. I have heard individual opinions, of course.

17497. I was asking the views of your society?—I do not think they have any.

17498. I want just to ask you one question about Professor Schäfer's experiments. You know that most

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of his experiments were on dogs, which he had previously chloroformed and anæsthetised?—Some of them were, I understand; I do not know the proportion.

17499. Do you know that only three were performed on dogs that were not anæsthetised?—I know that some were.

17500. According to his evidence before us, three dogs were experimented on without anæsthetics. You know that in each of those cases the dog was not allowed to recover, the dog was drowned?—I do not know the particulars.

17501. It was killed by drowning, but at any rate that was an experiment that your society strongly objected to?—Yes.

17502. Are you aware that superfluous dogs in Edinburgh, where these experiments were performed, were destroyed by drowning?—We do not approve of that method of destruction. If they have to be destroyed we believe they ought to be destroyed mercifully in a properly constituted lethal chamber.

17503. I suppose everybody would agree that that is the best way, but would your society draw any distinction between drowning a dog as a superfluous dog, and drowning a dog in the hope of discovering a means of saving human life?—I do not think we draw any distinction whatever, because we do not believe in superfluous dogs.

17504. At any rate your society protested against Professor Schäfer's experiments, but did not protest against the destruction of superfluous dogs in Edinburgh?—We protest against the destruction of dogs by drowning everywhere, where it has come to our knowledge. We have discussed the matter with the Leeds Town Council recently, and persuaded them to discontinue it.

17505. (*Chairman.*) Does that include puppies; do you extend your care down to new-born puppies?—Yes.

17506. (*Sir Mackenzie Chalmers.*) Would you not allow a puppy, before it has opened its eyes, to be drowned?—That is a matter of individual opinion, I suppose. I cannot commit my society to an opinion upon that point.

17507. There are only two other points I want to ask you about. You mentioned certain Government laboratories; what laboratories do you refer to. I am not aware of any Government laboratories?—The Board of Agriculture have licensed laboratories for the purpose of vivisection, so I understand, and also the Local Government Board and, I believe, the India Office and the Home Office.

17508. I am not aware at any rate of the Home Office having any. I should have heard of it, I think?—I may be wrong about the Home Office.

17509. (*Chairman.*) You probably mean the Colonial Office?—Yes.

17510. (*Sir Mackenzie Chalmers.*) You may perhaps speak of the Lister Institute and the School of Tropical Medicine?—Yes.

17511. That is to say, the Government ask for experiments to be performed with reference to work in subjects dealt with in the departments?—Yes; the Return of the Home Secretary for last year makes special reference to these Government laboratories and laboratories which are supported by public money under the auspices of local authorities in different parts of the country. Those are what we mean.

17512. The only other point is this. You mentioned that some of the medical men who side with you found a difficulty in expressing their opinions, because of the strong opinion held by the heads of the medical profession. How do you account for it that men of the greatest eminence in the medical profession are all on one side, that the heads of the profession are so strong?—I cannot account for it. I do not profess to be able to account for it. I am very sorry that it is so.

17513. You admit the fact?—I am afraid that I must. Of course on the other side I say there are many eminent men who do not believe in vivisection.

17514. Many eminent medical men?—Yes, I believe so.

17515. (*Mr. Ram.*) Can you tell me their names?—Yes.

17516. (*Sir Mackenzie Chalmers.*) You have given some names. We should like to know the names of any existing men of acknowledged scientific attainments in the medical profession who are on your side?—We have often heard Sir Frederick Treves quoted; I am not at all prepared to say what his views are on the general subject, but he certainly expressed himself very strongly.

17517. On one particular point?—Yes.

17518. But speaking generally it is not from the heads of the profession, but from the other end that you have to draw your witnesses?—I do not know. I think you have had some fairly eminent men from our side before your Commission.

17519. (*Mr. Ram.*) Just a few questions as to facts, please. You have given us on several occasions the views of your society. How are they arrived at?—I do not quite understand. How do you expect me to answer?

17520. I leave you to find an answer. I put the question: How do your society arrive at the views which you have represented as being theirs?—As I told you, the society was started as an anti-muzzling society, and it has developed little by little until it became a General National Canine Defence Society. They have not arrived at these views suddenly, but it has been really a development, as with most societies.

17521. To take one example, for instance, which has been already referred to. You say that your society object to all experiments, because of the difficulty of anæsthetisation?—Yes.

17522. On what do they base themselves in saying that it is difficult to anæsthetise animals?—I think I explained before that we have received letters from medical men and veterinary surgeons, who give it as their opinion, and give it strongly as their opinion, that dogs cannot be anæsthetised.

17523. You gave us the name of one gentleman who said so, and who said that he had tried and failed on a few dogs. Can you give us the name of any other gentleman besides him?—I read to you an extract from Professor Woodroffe Hill's letter.

17524. That is the one to whom I am alluding. Can you give us any other names?—Dr. Lucas Hughes, of Liverpool, wrote us a very strong letter on the subject. We issued that letter from Dr. Hughes as a leaflet, and we also published it in our report.

17525. That is what made me anxious to get at it?—I am afraid that I cannot put my hand upon it at the moment, but it was a very strong letter.

17526. What was the date of it; do you know?—I should think it was about two years ago.

17527. Did he state how many animals he had attempted to anæsthetise?—No, he does not.

17528. You will get that information for me and send it to the Commission?—I will, with pleasure.*

17529. You have already been referred to the evidence of Professor Hobday as to anæsthetising animals. I want to read to you two sentences from the evidence of Dr. Dudley Wilmot Buxton, a Doctor of Medicine and past President of the Society of Anæsthetists, Consulting Anæsthetist to the National Hospital for Paralysis and Epilepsy, Anæsthetist and Lecturer on Anæsthetics in University College Hospital, who has had a very large experience in the study and administration of anæsthetics. He was asked this at Question 12439:—"Is there any difference, and if so will you explain it to us, in the method of operation of anæsthetics upon man and upon the lower animals?" And his answer is "None." Then he was asked, "Do you note their condition of insensibility, and the degree of it, in the same way with animals as you would with men?" And he said, "Always"?—That refers to all animals.

17530. That refers to all animals. Your society, as I understand, have been informed that it is specially difficult to anæsthetise dogs?—Yes.

17530*a*. Dr. Buxton was asked that question at Question 12489:—"We have been told by several witnesses that dogs are particularly difficult to keep under anæsthetics. Is that your experience?—(A) Certainly not. (Q) There is no difficulty in keeping a dog for several hours under anæsthetics?—(A) Not if you understand your business. (Q) We have also been told that it requires a very skilled anæsthetist to anæsthetise a

Further particulars were not supplied by the witness.

dog. Is that so?—(A) That would depend entirely upon the apparatus employed." Then, at Question 12496:—"And, in your opinion, is any special skill or knowledge required for anæsthetising a dog in order to produce anæsthesia?—(A.) No, I think not. Of course, an inexpert person may produce an overdose by giving too much. I do not think he is likely to err on the other side." And again:—"If the dose of anæsthetic was insufficient, the dose of air would be insufficient, and the animal would die?—(A.) That is so. (Q.) That is your opinion?—(A.) That is my opinion." That is the evidence of a very distinguished man who has made it the study of his life to ascertain the effects of anæsthetics upon human creatures and upon animals. What is the evidence that you propose to offer to us on which you would ask us to set aside that evidence?—I am only prepared to give you the evidence that we have received, to some of which I have already referred, of such men as Professor Woodroffe Hill and Dr. Lucas Hughes of Liverpool.

17531. Those are two?—Those are two that I can remember at the moment.

17532. Our object is to see what evidence there is on either side?—I have also had conversations with several medical men, who have all said the same thing.

17533. In those conversations did you ever ask those medical men how many animals they had operated upon?—No.

17534. That is rather material, is it not?—Yes, it may be. I have simply asked them what their views were, and did they believe that this was correct, and they have said yes—that they thought the views expressed by those two gentlemen were correct.

17535. The views of your society, of course, are valuable entirely according as to whether they are or are not founded upon accurate knowledge?—Quite so.

17536. Can you tell me who drafted the petition which you presented to Parliament?—I really cannot tell you exactly who drafted it. It was drafted with the approval of the committee, but who the actual person was who drafted it I do not know. I daresay I could ascertain it.

17537. You have been good enough to hand to me a pamphlet just now, and I want to ask you about one or two expressions in it?—We do not issue that pamphlet, of course; it is a pamphlet which is issued by another society, which I simply put in my bag for the purpose of reference to the figures. I take no responsibility whatever for the pamphlet.

17538. Do your society adopt the statements that are made in it?—We simply adopt the facts as they are taken out of the Home Secretary's Returns. I could not put my hand on the Home Secretary's Official Reutrns, and therefore I put this in my bag instead.

17539. Do I take it that you have read this pamphlet yourself?—Yes.

17540. Do the statements herein made represent the views of your society also?—I do not know that they do exactly. I would not like to take the responsibility for them without going more deeply into them.

17541. Does your society think that the Act is "useless as a protection for animals, while protecting their torturers"?—That sounds rather strong, does it not?

17542. That is for you to say. Does that represent the views of your society?—I cannot say that we would put it quite in that form.

17543. I will just read you one other passage. Does your society think that "an actual painless vivisection is an impossibility"?—We do not take any responsibility for that statement.

17544. Then I will pass from that as it is not issued by your society. In the covering letter which you issued with the petition you say there has been a "terrible increase of experiments on dogs during the past three years"?—Yes.

17545. That is written in the year 1906?—Yes.

17546. "An increase proved beyond controversy"?—This letter is dated 1906; I believe it was drafted before that, only it was reissued in 1906.

17547. Will you tell me to which three years you refer in this letter when you say that there has been "a terrible increase of experiments on dogs during the past three years"?—I should think it refers to the years 1902-3-4.

17548. How can that be when it goes on to say that it is proved by a statement made by the Home Secretary in 1903. That cannot prove anything about 1904?—No.

17549. Nor can it prove anything about 1903, can it?—But this is only a statement made with regard to the year 1902, which I take to be the first of the three years referred to in the letter.

17550. (*Chairman.*) What is the date of the petition or letter?—The date of the issue of this particular letter which I have before me is April, 1906, but it was drafted, I am sure, before then, and was issued in the first instance earlier than that. This is a reissue sent out later.

17551. (*Mr. Ram.*) But it is on the faith of this letter that you are asking persons to subscribe and to take up the petition to Parliament?—Yes.

17552. Therefore, I want to test the accuracy of the statements in this letter. The letter is signed by your chairman and issued in April, 1906?—Yes.

17553. It says that in the past three years there has been a terrible increase in the number of experiments on dogs, as proved by the statement made in Parliament by the Home Secretary on May 11th, 1903?—Yes.

17554. I ask you, in this letter which is sent to me and many other people, which are the three years to which you refer?—I cannot tell you the exact three years; 1902 may have been the last of the three years for anything that I know. At all events, I do not think that the facts stated in the letter can be impugned at all, because there is a very great increase in experiments performed on dogs and other animals for all the years since 1897.

17555. (*Chairman.*) But this is all about dogs; there is nothing about other animals?—This is including dogs.

17556. (*Mr. Ram.*) Since 1897?—Yes.

17557. And other animals?—Yes.

17558. Please narrow yourself to dogs. How can you prove that there was a terrible increase in experiments on dogs during the three years preceding the issue of this letter?—I have not the figures with me with reference to dogs alone, but I feel pretty certain that it is so.

17559. Do you know how many dogs were employed by Professor Starling at his laboratory?—He says 155.

17560. Well, do you doubt that?—I do not remember exactly what year he refers to. If he refers to 1902 I cerainly do not doubt it.

17561. I will give you the reference. You will find it in Professor Starling's evidence on page 116 of the first volume, in an answer to Colonel Lockwood, at Question 3470: "I will take the year 1902, since at the request of the Inspector I separated the experiments made in that year on dogs from those made on other animals. In this year 155 experiments were performed on dogs in the physiological laboratory at University College. Of these, 151 were performed under licence alone. What does this mean? In experiments performed under licence alone the animal must, during the whole of the experiment"—(then quoting the Act)—"'be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain, and the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered.' That is a quotation from the Act. In all these experiments, therefore, the dog was first chloroformed. The experiment was then performed while it was fully anæsthetised, and at the conclusion of the experiment the animal was killed without ever recovering consciousness." That was true of 151 experiments out of 155?—Yes.

17562. You accept that statement of his?—I have no reason to disbelieve it. You see that we are saying that the numbers in 1902 were 232, and I find that that is quite consistent with what Professor Starling says.

17563. I am not disputing now whether the number is 232 or 155?—But I should like to explain that, because there seems to be a discrepancy.

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17564. If you please?—Professor Starling said that he used 155 dogs, which I find is absolutely correct. We say that there were 232, which is also absolutely correct, the difference being that in the Physiological Department of University College 155 dogs were used, and 77 were used in the Department of Pathological Chemistry; that is the answer of the Home Secretary to which we refer in our petition, and also in the covering letter, 155 in the Physiological Department and 77 in the other, so that Professor Starling is absolutely correct and we are correct, too.

17565. I am willing to accept the numbers. What I want to point out to you is that out of those numbers it is only possible that four dogs can have suffered anything at all?—I do not say that.

17566. That is what Professor Starling states?—Yes, exactly, he says so, but I am afraid that we cannot accept that statement that only four out of 155 could have suffered any pain.

17567. Then will you tell me why you ask this Commission not to accept that statement?—Because neither Professor Starling nor anyone else can prove that the animals do not suffer. We have only to use our own common sense in the matter.

17568. But using common sense, if an animal is put under anæsthetics by which it is rendered wholly unconscious, and dies before it come from under the influence of the anæsthetic, how can that animal suffer?—I am not convinced that a dog can be anæsthetised.

17569. If you choose not to accept Mr. Buxton's and Professor Hobday's evidence, that is another matter; but hypothetically, if an animal is anæsthetised so as to be beyond all possible sensation, will you agree that it cannot suffer?—I suppose I must if it is beyond all possible sensation.

17570. And if the animal dies before recovery of consciousness, do you agree that there can be no kind of suffering in that animal?—I suppose I must. I should like, at the same time, to guard myself by saying that, even granting all that, we do not believe that vivisection of dogs is justifiable, on other grounds, of course.

17571. Following up that last remark of yours, granted that a dog is wholly anæsthetised and can never recover consciousness at all, on what grounds do you object to its being experimented upon?—We do not consider that man has a moral right to experiment upon any live animal for his own purposes. We think it is absolutely wrong to do so.

17572. That goes against any vivisection at all?—Yes.

17573. Do you draw no distinction then between dogs and other animals?—If you ask my own opinion, I do not personally.

17574. Does your society?—I have already said that my society does not go into the question of other animals. I believe that the great majority, if not the whole of the members, of my society do not believe in any vivisection at all, but *qua* the society we only advocate the non-vivisection of dogs.

17575. I will read you one other passage from Professor Starling's evidence, which you will find at page 115, in answer to Question 3451: "Though I have been engaged in the experimental pursuit of physiology for the last seventeen years, on no occasion have I ever seen pain inflicted in any experiment on a dog or cat, or, I might add, a rabbit, in a physiological laboratory in this country, and my testimony would be borne out by that of anyone engaged in experimental work in this country." Can you show the Commission any reason why they should not accept that statement?—All I can say is that I would not accept it.

17576. Why not?—Simply because it seems to me absolutely unreasonable to think that a man like Professor Starling, who has been engaged in this work for 17 years, never, as he says, saw pain inflicted on an animal. I am sorry to say that I cannot accept it. Professor Starling may think so, but my common sense will not allow me to accept it, and revolts against its unreasonableness.

17577. That is a direct statement of fact by Professor Starling?—No, of opinion, I take it.

17578. No, of fact?—So far as his opinion goes he has not seen any pain inflicted. I cannot accept that. That is all.

17579. You say that your common sense is the ground on which we ought to decline to accept that statement?—I am not dictating to the Commission; I should be very sorry to do that. I am simply giving my own views. The Commission are in a better position to judge as to what they should accept than I am.

17580. You quoted Professor Starling just now as to a dog being glad to be introduced into a laboratory. Let me read his exact words, and you will see what he said?—Thank you very much. I shall be glad.

17581. I thought you took it as implying that a dog shows pleasure at being about to be experimented upon again?—That is the impression that I had.

17582. Let me see if I can remove it. Will you turn to page 139, Question 4024—it is a question put by myself: "Such an animal, I presume, if it was an intelligent animal—a monkey or a dog—say, might have more apprehension than an animal which had never been so treated before, might it not?—(A.) I do not think so. I cannot, of course, judge of these inoculation experiments, but the experiments which I have in my experience are experiments in which the animal—the dog—has had an intestinal fistula established—that is to say, a little opening into a loop of its intestines. These animals are quite happy. When we want to observe the flow of juice from these fistulous openings, the dogs are brought into the room, placed in front of a fire in a little stand, and slung up in a duster to take the weight off their legs. Either I myself, or the laboratory boy, stays with them and talks to them, and meanwhile they are given some meat, and the juice drops from the opening into a flask placed below. These animals are happy to be brought in. They are taken notice of, and talked to, and given a good meal of meat. They are very happy to be brought into the laboratory, to be subject to observation under those circumstances, and they do not seem to be incommoded in any way by the fact that they have an opening into a loop of their intestines. (Q.) And during such time as they are not under observation in their cages, are they then in a state of suffering?—(A.) No, none whatever. (Q.) Even a well-established fistulous opening does not produce suffering while it is going on?—(A.) But what I wanted specially to insist on was that they were not even in a state of apprehension. They were perfectly happy. They liked to be talked to, and they behaved like a normal dog, in fact." That is the passage, I think, to which you alluded?—Yes.

17583. You observe there that the dogs were not brought in to be experimented upon again at all?—Yes, for the purpose of observation, I take it.

17584. They were not to be subjected to anything which was to cause them any pain?—That is the only respect in which I can modify my opinion. I am sorry that I said the other. I understood that it was for the purpose of experiment. At the same time that does not alter my views of Professor Starling's evidence, and it does not affect the point made when I adduced Professor Starling's evidence as a proof of the docility of the dog as compared with that of other animals.

17585. There is only one more question that I think I need trouble you with, and that is as to rabies. Your society, you say, doubted the existence of rabies to anything like a considerable extent?—To the extent to which it was returned.

17586. Did they doubt the existence of rabies at all?—No, I do not think that they doubted its existence at all. I think they believed that it did exist to some extent.

17587. Do they believe that it is now extinct in this country?—Yes.

17588. If rabies was not widespread, how do you account for the large number of deaths of persons who died of rabies—not of dogs, but of human creatures?—I do not think there were a large number.

17589. Do you know what the numbers were?—Not at the moment.

17590. Have you never had the numbers given you?—Yes, I have, but I cannot at the moment go through them, and my impression is that there was not such a large number.

17591. Your society, I understand, object to muzzling?—Yes.

17592. Have you ever put a muzzle on a dog?—Yes, I have.

17593. Have you ever had a dog which had a muzzle on for three or four days?—Yes.

17594. Did the dog mind it?—Yes, it objected very much.

17595. After it had worn it three days?—Yes.

17596. What sort of dog was it?—A Yorkshire terrier.

17597. Did it never get reconciled to its muzzle?—No, I do not think it ever did.

17598. I think it must have had rabies to start with; it is very different from my experience.

17599. (*Chairman.*) The muzzling was objected to on the ground of cruelty, I understood you to say?—Yes.

17600. (*Dr. Gaskell.*) There is just one question I want to be clear about. I understood you to say that in your opinion, and as representing your society, an actual painless vivisection in dogs was not an impossibility?—Oh, no.

17601. You said that just now; those were your words?—No, what I said was this. I was asked, assuming that a dog could be anæsthetised, assuming that what Professor Starling said was true, if a dog could be anæsthetised, then the experiment must have been painless, but I say that we deny, so far as we know, that a dog can be anæsthetised, and that if a dog cannot be anæsthetised, then it cannot be painlessly experimented upon.

17602. Then you want to withdraw that remark that you made just now?—I am not aware that I made it. I had no intention of making it.

17603. (*Sir John McFadyean.*) It may have been a misapprehension on your part?—Absolutely, because I have all along said that we believe that a dog cannot be anæsthetised, and therefore it cannot be painlessly vivisected. I understood the question to be whether if a dog could be anæsthetised and destroyed before it recovered consciousness, I believed that it suffered pain. I certainly say that if it could be anæsthetised it could not suffer pain.

17604. (*Dr. Gaskell.*) Did you not also just now say that in your opinion, and in that of your Society, a properly constituted lethal chamber was the best means of destroying dogs?—Yes.

17605. What do you mean by a lethal chamber?—A box designed for the purpose of putting the dog painlessly to death.

17606. In what way; how would it be put to death; what is the ordinary method in the lethal chamber; what is used?—Principally chloroform and carbonic acid gas, I think.

17607. But as dogs cannot be anæsthetised that is a painful method of putting a dog to death?—I do not say that a dog cannot be killed, I say that it cannot be anæsthetised. I said at the outset that in our opinion what we had been given to understand was that a sufficient dose cannot be given to a dog for the purpose of anæsthetisation without killing it, and that it cannot be made unconscious with a dose of chloroform or anything else without ending in death. We believe that dogs can be killed, of course.

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17608. Then that must necessarily be a painful death?—Not at all.

17609. Because they cannot be anæsthetised?—I do not see that, if the chloroform or other anæsthetic kills them.

17610. If the action of chloroform is first to anæsthetise an animal, and, in a lethal dose, to cause death, the period between when it is first given and the time of the animal's death, must either be a painful period or a non-painful period must it not?—I am not an expert on poisons, but I have seen a dog destroyed in the lethal chamber, and the whole process does not take a minute and a half.

17611. But during that minute and a half it is painful?—But it does not seem to have any pain at all.

17612. Of course it does not?—But, then, that is a very different thing from giving a dog a dose of chloroform when it is intended to be experimented upon by, say, a cutting process, when that dose is not sufficient to enable it not to suffer pain. I think there is a very marked distinction between the two.

17613. I was only on the question of the morality of putting it to death in the lethal chamber by means of a drug which does not anæsthetise, which seemed to me to involve a painful proceeding, and I wanted to know your opinion?—I do not think it is.

17614. (*Sir John McFadyean.*) Are you quite certain that none of these unconscious dogs in the lethal chamber could be revived?—No, they cannot.

17615. On what ground do you base that statement?—I have never known any to be revived.

17616. Is that clear evidence that they cannot be revived?—I do not think that they could always; and we have had veterinary surgeons present when they were lethalised.

17617. Have you had veterinary surgeons trying to revive them?—Oh, dear me, no; we do not experiment upon dogs.

17618. So that really you have no evidence of it?—Our object is to put them to death, when we find it absolutely necessary. We never try to revive them.

17619. Am I right, therefore, in saying that you have no evidence to show that they cannot be revived?—Because we have not experimented.

17620. So that you have no ground for asserting that they cannot be revived?—We have only the ground that they never have been revived.

Mr. JOHN ROSE BRADFORD, M.D., D.SC., F.R.C.P., F.R.S., called in; and Examined.

17621. (*Chairman.*) You are a doctor of Medicine, and a Doctor of Science, and Fellow of the Royal College of Physicians, and a Fellow of the Royal Society?—Yes.

17622. And you are Professor of Medicine at University College Hospital Medical School, Physician to University College Hospital and to the Seamen's Hospital?—I am.

17623. You are a member of the Advisory Board of the Army Medical Service, and a member of the Advisory Committee for the Tropical Diseases Research Fund of the Colonial Office, and a member of the Tropical Diseases Committee of the Royal Society?—I am.

17624. And you are Chairman of a Sub-Committee of the Royal Society for the Investigation and the Treatment of Sleeping Sickness and other Diseases?—That is so.

17625. I believe you desire to give evidence as to the value of experiments on animals principally in assisting the physician in actual practice?—Yes.

17626. We have had a great body of evidence as to the value of experiments on animals, and I will not take you into the whole history of the reasons why they are important; but perhaps you will just give us your views about the importance of it as a physician?—I have sent in a short statement already, and I am prepared to answer any questions to the best of my ability that any Commissioner may wish to ask me. I do not know that I have any reasoned detailed statement to make. If there are any points in this short summary on which any Commissioner would like to ask further questions to bring out the points I shall be very pleased to answer them, but, as I say, I have not got any reasoned detailed statement to put before you.

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17627. So far as I have looked through your statement, you go through the principal services which have been done, in your view, by experiments on animals, by discovering causes of disease or remedies, very much on the lines that a great number of witnesses have gone through them?—Yes.

17628. I do not want to take anything in unnecessary detail?—I have divided this summary, roughly speaking, into two groups—two paragraphs—the one dealing, I may say, with the results which have been obtained in the various committees on which I happened to serve, especially those in connection with the Royal Society; but I believe you have had a very considerable amount of evidence bearing on that already brought before you, so that my position as regards that is that I am here to answer any questions that I am able to answer which anybody may wish to ask me with reference to that point. Then, with reference to the use of experiments on animals in the practical work of the physician, I have divided

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that into two short headings. One is as regards the necessity of every fully-trained physician having a sound knowledge of the healthy functions of the body, which you may certainly say could not have been obtained without the results of experimental science; and then I have mentioned two or three instances in which I thought that the physician in his every-day practice was directly dependent upon the methods he had to employ, involving experiments on animals to obtain the knowledge that was necessary for the treatment of the sick.

17629. Would you tell us what those are?—The instances I quoted bear, for example, on the necessity for accurate diagnosis in the early stages of disease. There are a great number of diseases which in the early stages do not give rise to what physicians talk of as marked signs; they cannot be detected by the handling and looking at the patient, but they can be detected by methods of investigation necessitating animal experimentation. I do not know whether you want me to give specific instances?

17630. Perhaps you would give us one or two?—A specific instance which would occur to one would be, for example, the case of a patient who had suspicions of tuberculous disease of the lung, at the top of the lung. What we talk of as the physical signs of that disease may be produced by other maladies than tuberculous maladies, and there are classes of cases in which the only satisfactory method of determining whether the patient is or is not suffering from tuberculosis is the inoculation of a guinea-pig with the material expectorated by the patient. That is an illustration of what I mean. If you wait until there are unequivocal signs of the presence of one or more of these two diseases, very much valuable time is lost.

17631. Are you speaking there of a disease that is of comparatively slow progress?—Yes.

17632. You could not use that sort of experiment to discover whether it was scarlet fever?—No.

17633. That would discover itself before the experiment was worked out?—Yes.

17634. But if it is like tuberculosis or suspected tuberculosis, is that experiment practically done now by physicians?—Yes, I have done it—or, rather, have had it done on several occasions on which there has been some doubt as to whether the patient was suffering from tuberculosis or from a malady that is known to physicians as bronchi-ectasis.

17635. (*Mr. Ram.*) Is the result on the guinea-pigs certain and final?—If it is positive, of course.

17635A. (*Sir William Church.*) But the same method is also of use in what you may call acute diseases, is it not?—Yes, certainly; but I thought the Chairman asked with regard to that simple experiment.

17636. But it is also of use for diagnosing purposes in acute diseases?—Yes, and that I have alluded to in the second paragraph, where I quote the instance of diphtheria and typhoid as illustrations of acute diseases.

17637. (*Chairman.*) How would it help you in a case of suspected diphtheria?—In a case of suspected diphtheria, the modern diagnosis is made by recognising the organism as such, the cultural peculiarities of the organism. But the foundations of our knowledge as regards the organisms are derived from experiments which in the past involved experiments on animals, as, for example, to determine whether an organism is virulent or not. That is no longer necessary now in the practical question of an individual patient at the present day; but our knowledge of the virulence of the diphtheria bacillus has been obtained by experiments on animals.

17638. I quite understood that distinction. I only put the question that I did in order to distinguish the two cases—the one in which you can actually test, by an experiment on an animal, whether your particular patient in a particular case has got a particular disease, and the other the general knowledge that you have derived from discovery?—Yes.

17639. I was not aware that it was ever done in practice—that physicians in a case like tuberculosis had a special experiment made on an animal by which to ascertain whether that particular patient had that particular disease?—Yes, that is done.

17640. I think you are the first witness who has referred to that?—Yes.

17641. (*Sir William Church.*) It is also frequently done, more frequently for tuberculosis in other organs of the body, is it not?—Yes.

17642. For instance, in the bladder?—Yes; I mention that in my summary. But I took the first illustration when the Chairman asked me.

17643. What is the next head upon which you desire to give evidence?—The next head is the one Sir William Church has alluded to with reference to the same method being applied in diseases of the urinary organs. They are facts of the same order, of course.

17644. Is the use of special experiments in particular cases common now amongst physicians, or is it only some very advanced ones who take it up?—I do not think it is a question of their being advanced. I think the number of cases where that is required is a very limited number, but they do occur. I have not been quoting hypothetical cases, I mean; I have been quoting actual cases that have occurred. Certainly, in cases of diseases of the bladder and diseases of the kidney it is often extremely difficult to be absolutely certain that tuberculosis is present without an inoculation experiment.

17645. (*Sir William Collins.*) Would the finding of bacillus of tuberculosis alone be insufficient?—You mean by other than inoculation methods?

17646. Yes?—I am not a practical bacteriologist, you must clearly understand that. I do not give evidence as an expert in bacteriology. I give evidence simply as a physician from this point of view. But I have always understood that there are difficulties in the recognition of the bacillus of tuberculosis in certain fluids—as, for example, the urine. And certainly putting that on one side, an inoculation experiment is a very much more satisfactory method, because you learn not only with certainty that the bacillus of tuberculosis is present, but you also learn the degree of virulence of the organism which (here I am only perhaps expressing my own personal opinion) I think sometimes is a fact of considerable importance.

17647. Is that true also of the sputum in the case of tuberculosis? Would the finding of the bacillus be insufficient without inoculation of an animal to verify the diagnosis?—I think that the recognition of the organisms in the sputum is more easy by other methods. There are not the same sources of fallacy as there are in the case of the urine.

17648. Then would it be sufficient without inoculation?—It might be. I do not know whether the Commission wish me to quote individual cases—whether it is worth while.

17649. (*Chairman.*) Certainly, where it illustrates your views?—In a case that I have very much in my mind of a man with what is spoken of as apical bronchiecktasis, the illness was extraordinarily suggestive of tuberculosis. The examination of the sputum was negative, but yet the clinical picture was such that one had grave doubts about the accuracy of repeated examination of the sputum; and the inoculation experiment was also negative. And the subsequent history of that case proved the correctness of absence of tuberculosis, to the best of my judgment.

17650. (*Sir William Collins.*) In that case did not the bacteriological test and the inoculation test point to the same conclusion?—Yes, in that case, certainly.

17651. Can you give us an example in which the bacteriological test and the inoculation test pointed in opposite directions?—I could not recall a case actually at the moment; but I think I am correct in saying that in cases of pleural effusion one has often failed to find the organism in the fluid when the inoculation of the fluid has led to the development of the lesion in animals. I have not got an actual concrete case before my mind.

17652. (*Sir John McFadyean.*) It is a matter of common knowledge, is it not, among pathologists and bacteriologists that the inoculation test is a much more delicate one than the microscopic test?—That is so; that is my point, of course.

17653. You can use for one inoculation operation an amount of fluid that ten men could not examine in the course of several days?—That is so.

17654. (*Chairman.*) Do you attach importance to the freedom of the physiologist to experiment upon different kinds of animals?—Yes, I attach great importance to that.

17655. Will you tell us why?—First and foremost because I think that, in order to obtain a really sound knowledge of physiological processes, one wants to have the opportunity of investigating them under different conditions. Similar functions are performed

in different animals by different agencies, and with slight differences in their physiology; and one in that way, by the use of different animals, gets a wider perception of the physiological processes in any given organ. I think that is one reason.

17656. We have been told that different animals resemble man in different parts of their bodies?—Yes; some animals resemble man more closely in certain parts of their structure and functions than other animals do.

17657. The monkey, with regard to the brain, I understand?—Yes.

17658. And I think we have been told by several witnesses that there are some parts of a dog—the digestive organs and the bowels—that closely resemble those of a man?—Yes; the general nutritive processes in the dog have a closer analogy to those in man than in the case of many other animals.

17659. I suppose there are other instances in which certain parts of an animal would not afford much assistance with relation to the knowledge of man?—Yes.

17660. The last witness that we had was one who belonged to those who protest very strongly against dogs being used for these purposes. Are there any purposes for which you consider the dog is essential?—I think that it is very difficult to carry out experiments on the circulation in other animals than the dog. When I say that it is very difficult, that is perhaps not quite the correct word to use. But the dog is the animal in which the circulatory apparatus has much more resemblance to that of man than in the case of many other animals. That is one instance, I think. Then, of course, the digestive processes and the nutritive processes generally, to which you have already alluded, is another illustration.

17661. (*Dr. Gaskell.*) Would you include in the circulation the lymph system, the lymphatics?—Yes, I think so. I have not yet myself worked at the lymph system, but I certainly think I would include it.

17662. (*Chairman.*) If dogs were excluded from experimentation, what would be the result to physiology. Many people think that they should be excluded, and I wish to know what, in your opinion, the effect would be?—I do not wish to use exaggerated language, but I am bound to say that I think it would cripple physiology in this country.

17663. (*Sir Mackenzie Chalmers.*) To a great extent or to a small extent?—To a great extent.

17664. (*Chairman.*) By not giving the same opportunities to physiologists of comparing results, in the dog and in man, in particular parts of the body, do you mean?—Yes, and in preventing physiologists making useful additions to knowledge in physiology.

17665. As a matter of research?—As a matter of research, and amongst other things, as a matter of direct practical value to man. The two things go together, I mean; they cannot be separated.

17666. I understand that there are a good many experiments that could not be satisfactorily performed and with satisfactory results if physiologists were confined to such small animals as guinea-pigs, rabbits and rats?—I think that is unquestionable.

17667. As to demonstration by experiments before students, what have you to say to that?—I think that it is essential for the proper education of students that there should be demonstrations. I understand by demonstrations, of course, demonstrations on anæsthetised animals.

17668. (*Mr. Ram.*) And animals that cannot recover from the anæsthesia?—An animal that is killed before it recovers from the anæsthesia.

17669. (*Chairman.*) Have you ever known, I ask you this because suggestions have been made before us to the contrary, of animals being vivisected for purposes of demonstration (I mean cut into with the knife for purposes of demonstration before students) without anæsthetics?—No.

17670. Never?—Never.

17671. You have had long experience?—Yes, I began working in a physiological laboratory in the year 1883. I cannot trust my memory as to exactly when I gave up my licence; it was three or four years ago. Since that I have not held a licence. Roughly speaking, I have been acquainted with physiological laboratories for twenty years.

17672. What is your view as to the value of demonstrations?—My view about that is, that it is quite impossible for a student to get really a correct knowledge of any fundamental phenomenon in physiology, such, for example, as the beat of the heart, without really having seen the heart beating. I do not think that that kind of real knowledge can be acquired from reading. If you take two students, one of whom has been told the stimulation of a nerve in the neck stops the heart, and the other has seen the heart stop with the nerve being stimulated, I do not believe that those two students have got an equal knowledge. I know that I never had what one might call a true knowledge of the thing until I had seen it.

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17673. (*Sir Mackenzie Chalmers.*) That, of course, is done under anæsthetics?—Yes.

17674. (*Chairman.*) Were demonstrations of that kind common when you were learning your profession?—No, when I learnt my profession, I think I am correct in saying that there were no demonstrations to the general class, and that the only demonstrations that took place were to students who were taking the higher course of instruction, the so-called advanced course. That is my recollection. I have no recollection of having seen demonstrations in the ordinary large lecture class. I am talking of 25 years ago, of course, and more than 25 years ago.

17675. Do all students now have an opportunity of seeing demonstrations?—I believe so. I cannot speak of that of my own knowledge, because, of course, I am out of touch with the teaching of physiology now, even at my own medical school, and I do not know for certain whether all the students see demonstrations or whether it is only those who are taking advanced courses. But you have had many witnesses before you who could answer that of their own knowledge. I am not prepared to say from my own knowledge.

17676. You think it is advantageous, at any rate, that they should have the opportunity?—I think it is more than advantageous; I think it is necessary.

17677. (*Sir William Church.*) You have seen the printed evidence before this Commission?—Yes. I cannot say I have read it all.

17678. I want to ask you a question about it. You will find on page 66, first of all, Question 5843, at the bottom of the first column, that a witness, Mr. Graham, alluded to some experiments of yours in the following words: "I only propose to give just one or two cases so that my argument may not be in the air. Dr. Rose Bradford cut into the ears of dogs, destroyed the tympanic plexus, scraped out the middle ear and poured in pure carbolic." Mr. Graham, of course, had no idea what was the object of that experiment there narrated. Is he quite wrong in his description of it? I should like you to explain to the Commission what was done, and what was your object in doing it?—I daresay it is within your knowledge that I was written to by the Secretary of the Commission with reference to this matter, and I have sent in a statement already with reference to it. These experiments were experiments on the physiology of secretion from the salivary glands—experiments on nervous regulation of secretion which involved the cutting of nerves supplying certain glands, the submaxillary gland and the parotid gland. The nerve going to the sub-maxillary gland is, of course, easily divided, the nerve going to the parotid gland cannot be readily divided, and so, in order to destroy that nerve, the ear was opened, under anæsthetics, of course, in the usual way in which a surgical operation on the ear is performed, and the interior of a certain portion of the ear was rubbed with a spoon and then one drop, one minim, of pure carbolic acid was allowed to run in, and was then wiped up with a piece of cotton wool and the wound was closed. That is a description of the operation. The object of the operation was to divide the nerves, or to destroy them, it comes to the same thing, supplying this particular gland.

17679. These particular experiments were done, I suppose, under Certificate B?—Yes.

17680. Then the witness went on to say that the operation was done under chloroform, but the dog lived for weeks afterwards in what must have been a condition of great suffering. I want you to tell the Commission whether, in your opinion, those dogs did suffer greatly?—It is quite untrue, of course, to say that they lived in a state of acute suffering. There was no evidence of suffering at all, and I may say that, of course, it is a common surgical procedure to scrub wounds with pure carbolic. It is, if anything, rather painless than painful.

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17681. It was done, first of all, whilst the dog was still under anæsthetics?—Yes.

17682. What is the action of pure carbolic acid when used in that way on the immediate parts?—It has a very successful but very complete destructive action; it just destroys what it is in contact with and does not, therefore, lead to any subsequent sloughing or inflammation when it is used in a small quantity like that, and is applied just in the form of one drop locally.

17683. And it also benumbs the part?—Yes.

17684. (*Sir Mackenzie Chalmers.*) How far is it a local anæsthetic?—It is a very powerful local anæsthetic.

17685. For instance, when applied to the painful nerve of a tooth?—It is only used for that purpose.

17686. As an anæsthetic?—It is applied, of course, to destroy the nerve as an anæsthetic, but to destroy the nerve as well.

17687. (*Sir William Church.*) Therefore what I want to get at is, that in your opinion, these particular experiments did not entail much after suffering in a dog?—I should have said they entailed no more than obtains in the healing of any healthy wound.

17688. In the first volume of evidence that was published, I do not know whether you have seen certain statements made about you. You will find on page 65, at the bottom of the first column, Question 1798, a statement made by Mrs. Cook that you performed various operations upon the kidneys of dogs at the Brown Institute?—Yes.

17689. She goes on to say: "Chloroform and morphia were used for the actual operation, and the animals were then placed in a glass case, with a glazed floor, for observation. Pieces were cut out of the kidneys, and they were mutilated in different ways. In the case of one dog, the operator cut a piece out of a kidney, and then tried to graft the piece and make it grow on another part of the inside of the animal. The animal died in four days." She, of course, had no idea of the object with which these experiments were done. I did not know whether you would like to mention that to the Commission?—First of all, these experiments were not done at the Brown Institution; I do not know that that is very material; they were done before I was at the Brown Institution. There was no operation of that kind done at the Brown Institution.

At this point Sir William Church took the chair.

17690. (*Sir Mackenzie Chalmers.*) How many years ago is it?—These experiments were begun in 1889, but they went on for many years. If you want the facts, I should have to look them up. I cannot pin myself exactly to a date, but they went on for six or seven years. But these particular experiments were not done at the Brown Institution. The object of these experiments was to see whether it was possible to produce in animals, by diminishing the amount of kidney substance, a condition at all analogous to what is found in human Bright's disease—that is to say, whether it was possible to produce a slow poisoning at all akin to that seen in Bright's disease—for the purpose, I need not say, of investigating Bright's disease. A number of facts were observed in the course of these experiments, perhaps the most important being that nothing at all akin to Bright's disease was produced by the diminution of the kidney substance. I mean that no proceeding like ordinary uremia was seen as the result of these experiments. A considerable amount of information was obtained, which was of value in these observation.

17691. Could you kindly tell us in what way it was valuable, because that is important?—One of the things which I think was of value is that one got accurate information as to how much kidney was necessary for life to be maintained.

17692. That affects prognosis?—Yes, prognosis. It might also conceivably affect operative interference. Then another thing that I think of some value, only this is rather a complex subject, is that it gave one a different idea of the significance of the quantity of urine that was passed. There were certain conclusions in human medicine that were drawn from a person passing much or little urine, and I think that those experiments showed that at any rate other factors had to be taken into consideration. That is an illustration of the kind of value of the experiments. But the most important thing of all was, I think, that no such condition as that known as human uremia was produced.

17693. (*Sir William Church.*) Would you tell the Commission, if you remember at this length of time, exactly what did become of these dogs. You see, in that paragraph, Mrs. Cook accounts for nine out of the forty-nine; she does not tell us what became of the other forty?—I should be very pleased to put the facts before you. You have had a copy of the published paper, giving the facts as regards each one, of course. To summarise the thing shortly, I would say that a great number of these dogs lived, of course, for very prolonged periods, in perfect health.

17694. Without apparently suffering?—Yes.

17695. They were in the condition of a man who has had an operation on his kidney?—Yes, they were perfectly well. Some lived, for example, for two years, and they were killed, not because they were ill, but because they served no useful purpose to keep any longer. Others wasted; they passed large quantities of urine, and got thin, but they suffered no more than that. They did not suffer at all, of course, except that they were thin. Others, where the quantity removed was greater, got so thin that they became extremely weak, and no doubt ill; they vomited occasionally; and those animals were killed immediately—there was no object in keeping them, of course. Then some of them, as stated by the witness, died within a few days of the operation. I remember this one alluded to here as having lingered thirty-six days, because it was a very remarkable one. That animal lived for thirty-six days, and died suddenly. I do not know from what cause, but it is quite easy to give you the full facts as regards each individual one if they are wanted. They are fully published.

17696. So that only two died from blood poisoning as the result of wounds, and one in four days. That is what she says?—Yes, I have no doubt that is true, but I should have to look it up to verify it.

17697. And the others were much in the condition of a man who has had a successful operation performed upon his kidney after he recovers?—Yes.

17698. That is to say, it is not likely that they were suffering tortures or pain?—There is no truth in the statement that they were suffering tortures or pain. And the only ones where there was evidence of the wound going wrong in the first few days after the operation were killed, of course.

17699. I should just like to ask you one question with regard to the use of animals in diagnosis beyond what Lord Selby asked you. Is not experimentation by means of animals of the greatest use in diagnosing diseases in the interest of public health. Is it not the case that an officer of health at a port would use animals, for instance, for the diagnosis of plague or cholera?—Yes, that is so.

17700. Those, of course, are acute diseases?—Yes, and, of course, at a time in the evolution of the disease when it might be impossible to diagnose it by other means.

17701. (*Sir John McFadyean.*) Do I correctly understand that you justify and advocate experiments on animals for teaching purposes, first on the ground that such experiments can be made absolutely painless, and, secondly, that they are of great value for fixing facts on the minds of students, and giving them a better understanding of physiological processes?—Yes, only I go a little further. I say that without such demonstrations it is impossible for them to get an adequate knowledge of physiological processes.

17702. But you would not seek to justify, and you would not advocate, such experiments were you not satisfied that they can be made painless?—No.

17703. Then, with regard to the question of declaring it illegal to use dogs for experiments, I suppose that for certain experiments an animal of about the size of a dog is necessary?—Yes.

17704. That is to say, there are certain experiments which are frequently done, and you hold that it is advantageous to do, which could not be done on a guinea-pig or a rabbit?—That is so.

17705. Other animals of about the same size might be the pig, or the sheep, or the goat, but do you think that a sheep or goat, or even a pig, could well be substituted for a dog in all the experiments for which a dog is now generally employed?—I think that is quite impracticable.

17706. For example, would it be possible to throw much light on the physiological processes of digestion and assimilation in men by experimenting on ruminants?—No.

17707. At any rate, you do not think that information of value would be as likely to be obtained in that way as by using dogs?—Certainly not.

17708. And the same would apply also to the pig?—Certainly.

17709. (*Sir Mackenzie Chalmers.*) I only want to ask one or two questions from a purely lay point of view. You told us that it was necessary sometimes to have a diagnostic experiment in case of suspected tuberculous disease of the kidney?—Yes, and the urinary tract, yes.

17710. That is important for this reason, that according to the result the treatment would differ?—Yes.

17711. That is to say that if your diagnosis was wrong you would pursue a different treatment?—The treatment would be perfectly different, because the classes of affection that are likely to be confounded with tuberculosis of the renal tract are diseases associated with gravel, a totally different class of affection.

17712. Therefore the consequences to the patient of a wrong diagnosis might be very serious?—Yes.

17713. Would the same apply to what you told us about the tuberculous disease of the apex of the lung?—You would adopt a different form of treatment in the two conditions.

17714. Now as regards diphtheria, is it ever essential there to perform experiments on animals for diagnostic purposes?—No, I do not think it is, because the organism can be recognised by other methods, by cultural methods.

17715. But you have to diagnose the organism in diphtheria?—Certainly.

17716. You get the false membrane equally in laryngitis as in diphtheria?—Yes, there are other causes of false membrane besides diphtheria.

17717. Would the treatment differ according to whether you find the specific bacillus?—Yes. It is well known, of course, that in diphtheria it is of the utmost urgency to recognise it at the earliest possible moment; the life of the patient may depend upon it.

17718. As a practical physician I suppose you have seen much diphtheria?—Yes.

17719. Do you use the antitoxin?—Invariably.

17720. Have you read the evidence we have had about antitoxin here, that its efficacy mainly depends upon its early administration?—Yes, that is so, it very largely depends upon that.

17721. Have you any doubt yourself as a practical physician, that the antitoxin does save life?—I have not the smallest doubt.

17722. And not only saves life, but does it also cut short the disease?—I have no doubt of it whatever, having seen diphtheria before the antitoxin treatment, of course.

17723. Taking your opinion generally, you think that medicine owes a great deal in the past to animal experimentation, and that it must in the future, if it is to continue to make progress, depend on animal experimentation?—I have no doubt about it.

17724. As regards those experiments of yours on the ear, the whole experiment was performed under anæsthetics?—Yes

17725. Would the wound be closed in any way before the anæsthesia disappeared?—The wound would be completely closed and the dressing put on.

17726. (*Mr. Ram.*) An aseptic dressing?—Yes.

17727. (*Sir Mackenzie Chalmers.*) The ear is very sensitive, is it not?—Yes.

17728. Would there not be a danger of subsequent pain from this operation?—If the wound went wrong, if it became infected, but not otherwise.

17729. Not as long as it was aseptic?—No.

17730. Were not very sensitive nerves involved in the operation, and would not the ends of those nerves give rise to pain?—I do not think so. The nerves in the first place were destroyed by the carbolic acid, and the operation, I should like to point out, is not precisely similar, because the structure of a dog's ear is a little different from that of the human ear; but putting that aside the operation is practically identical with the very common operation in human surgery of opening that portion at the back of the ear.

17731. For the purpose of getting at an abscess in the ear?—Yes.

17732. (*Mr. Ram.*) Is that followed by pain in the human subject?—Not so far as I know. There may be pain where the thing is due to inflammation, for which the operation is done.

17733. (*Sir Mackenzie Chalmers.*) The actual operation does not give rise to great after pain?—Certainly not

17734. In the human subject at any rate?—No.

17735. Therefore there is no reason to suppose that it does in the dog?—I have no hesitation in answering that question, because I know that these dogs ran about just like normal healthy dogs, and they showed no signs of pain. Every physiologist would tell you that if he performs an operation, and the wound becomes infected the thing is to a great extent spoilt for his purpose.

17736. Then the dog is destroyed?—Then the dog is destroyed.

17737. I just want to clear up this further point. In the case of kidney operations you extirpated a portion of the kidney in certain dogs?—Yes.

17738. That was done, of course, under anæsthetics?—Yes.

17739. The wound was healed up and dressed under anæsthetics?—Yes.

17740. Was it ever necessary again to open that wound, or not?—I rather fancy that I sometimes had to take out a stitch; there may have been a stitch abscess. I never had to open the deeper parts.

17741. In fact your object was that the wound should heal?—Certainly, but some of these wounds become infected, as was stated.

17742. Then you killed the dog?—Yes.

17743. After a time?—I am speaking from memory; I have not looked up these paricular series of experiments, but my impression is that they were killed at once.

17744. What I wanted rather to get with reference to some other evidence that we have had was this: The wound was not reopened for the purpose of inspecting it, or anything of the kind?—No. When you asked me the question whether I ever reopened a wound my impression is that I did sometimes take out a stitch.

17745. It has been suggested to us that under Certificate B the operation is performed under anæsthetics, but that subsequently exploratory operations are performed without any anæsthetics—is that so?—That is not so.

17746. You never heard of such a case in your long experience?—No.

17747. You have read the evidence given to us as to various tropical investigations?—Yes.

17748. We have had evidence as to sleeping sickness, yellow fever, Malta fever, and plague?—Yes.

17749. Have you anything to add to the evidence that we have already had?—I do not think so. Sleeping sickness and plague are the only diseases of the kind that I have been directly concerned with, because I have been on the Advisory Committee controlling both.

17750. You were a member of the same Committee as Dr. Charles Martin, who gave us evidence?—Yes, the Committee at the India Office.

17751. And you agree with his evidence?—Yes.

17752. (*Mr. Ram.*) With regard to these experiments on dogs, when you extracted part of a dog's kidneys, as I understand, it was, of course, necessary that the dogs should survive after the anæsthesia passed away in order that you might see the effect?—Yes.

17753. In the majority of cases I understand that the dogs throve and showed no signs of illness?—In a large number of cases.

17754. In other cases the dogs, I suppose, were ill in consequence of the effect of the abstraction of more or less of the kidneys?—Yes, they wasted. Wasting was the prominent symptom of illness.

17755. In respect of some of those dogs, I suppose, there was pain, or at all events discomfort, in consequence of the illness?—I should say that there was discomfort. I should not say that there was pain; but there was in those animals in which there was the smallest amount of kidney left a very considerable physical weakness and inability to take food, and therefore I have no doubt that there was discomfort. There was never any evidence of local pain, and there was no reason for supposing that there should be.

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17756. Were the dogs in a condition in which they were lying about in discomfort moaning?—No.

17757. It did not amount to that?—No, I never heard one of these dogs moan.

17758. Or give any actual signs of acute suffering?—No.

17759. There was an expression used by Mrs. Cook, I think, which was put to you just now—I do not know whether you adopted it—that one dog lingered for thirty-six days, and died suddenly?—Yes, my impression is that it died suddenly, but I will look that point up if you wish it.

17760. Only to this extent, the phrase she used, I do not know whether it was adopted from you or invented, that it lingered for thirty-six days, gives an idea of great discomfort?—I do not adhere to the word lingering. I do not think I used it. I should prefer to look it up, and send you an account of it.

17761. It was put to us as a case in which it was proved that the dog suffered pain for a considerable time—thirty-six days. Anything that you could give us as to that particular case I should be glad of?—I have no hesitation in answering at once that the dog did not suffer pain. I do not know in what particular form of words I described its life for thirty-six days.

17762. One other matter, please. In your statement you speak about the advantages obtained by inoculation to enable you to form a right diagnosis with regard to, for instance, tuberculosis. Does it come to this, that the instances so far as you have found them yourself tend to correct the diagnosis, to prove whether the diagnosis is right or wrong?—Yes.

17763. It tends, therefore, I suppose to indicate the proper treatment?—Yes.

17764. And may be the means of saving life if the wrong treatment were negatived, and the right treatment indicated?—Yes, that is so.

17765. Is the good which has been obtained by the discovery of these antitoxins, and so forth, confined, do you think, to correct diagnosis, or is it in any cases used in a curative way for the patients themselves?—It is certainly so used in a curative way in the case of diphtheria, for example.

17766. We have heard a great deal of Sir Almroth Wright's experiments lately, and you have been speaking of such instances as acute forms of pleurisy, lung diseases, and tuberculosis of the urinary tract. Tuberculosis of the urinary tract, on which some witnesses have given us evidence, is a matter, I suppose, that can only be treated through the system?—It can be treated surgically, of course.

17767. But apart from surgically, it must be treated through the system?—Yes.

17768. So far as you know, is the serum used by Sir Almroth Wright used curatively?—On his patients, I believe so.

17769. Have you known any cases in which, for instance, in tuberculosis of the urinary tract, this serum has been used?—I have no personal knowledge.

17770. Do the books disclose any cases?—There have been cases recorded in literature where it has been successfully employed.

17771. But you have seen cases in books?—Not in text books.

17772. I use the legal phrase, in books?—I thought you meant text books.

17773. No, I mean in literature. You have seen instances there recorded, in which it has been useful as effecting a complete and permanent cure of such a matter as tuberculosis of the urinary tract?—Yes.

17774. (*Dr. Gaskell.*) You have had experience of experimentation upon various animals, have you not—dogs and others?—Yes.

17775. Have you ever found the slightest difficulty in anæsthetising dogs?—No, none.

17776. You would not put them in a different category from other animals in that respect?—No, I should not put them in a different category. I should have said that I have had personally more facilities in anæsthetising dogs, and that I have found them very susceptible to certain anæsthetics, chloroform particularly.

17777. (*Mr. Ram.*) Meaning, that they are more liable to die under the anæsthetics?—Yes.

17778. (*Dr. Gaskell.*) But when you have succeeded in keeping them alive, you can keep them well under anæsthetics?—Yes, you can get the most perfect anæsthesia in dogs.

17779. You have had a licence and various certificates for some time?—Yes, I have held, I think, all the certificates. I never held a demonstration certificate. I think I have held all the others.

17780. Do you consider that the present Act puts trouble in the way?—I think there is a little trouble. Personally, I cannot say that I have had any serious amount of trouble. I have never been delayed in the course of any experimental inquiry that I have been engaged in, but I think I have known of instances where delay has occurred.

17781. Do you think it would be better if the present Act were abolished altogether?—No, I do not think that. I think that experiments on animals should be regulated by an Act.

17782. Have you any suggestions to make to this Commission as to any alterations in the Act that would be beneficial, or have you not thought about that?—I have not thought about it of recent years, I must confess; but I think I can answer that question, at any rate, to a certain extent. I think that certain individuals, at any rate, should perhaps have more general powers given them; that instead of having to apply for certificates for different categories of animals, they might be all covered by one certificate. Some years ago, when I was conducting some observations on a disease allied to sleeping sickness (nagana) in animals, I had to do those experiments on a very large number of different kinds of animals—horses, cattle, and a number of different animals—and I had some little inconvenience in getting those certificates; but it never amounted to anything very serious.

17783. (*Sir Mackenzie Chalmers.*) Were those experiments in nagana done in England?—Yes, they were done at the Brown Institution.

17784. (*Dr. Gaskell.*) That inconvenience, I presume, also affected the people who had to sign the certificates?—Yes.

17785. It was a bit of a worry for them, was it not?—Yes. I think there is a grave misconception in the mind of the public with reference to the wording of some of these certificates—Certificate B especially. There is one certificate which on the face of it rather implies that you are doing the experiment without anæsthetics—it is Certificate A—and in many cases you are really using an anæsthetic.

17786. (*Sir Mackenzie Chalmers.*) Certificate A only authorises feeding experiments or experiments which do not involve more operative procedure than the prick of a hypodermic needle?—I think I am under a misapprehension; I think the certificate was altered a few years ago; but there was a time when I held the certificate, and the particular certificate was the certificate dispensing with anæsthetics.

17787. That refers to the old E before EE?—Yes; there was one certificate which you had to apply for, which dispensed with anæsthetics, and you had to apply for that certificate when you were going to do an experiment on a particular animal under anæsthetics, and therefore the public who could see an intimation that So-and-so held a particular certificate, acquired the idea that they were doing experiments without anæsthetics, when really they were not.

17788. We had that in the evidence of Sir Victor Horsley. It was the old Certificate E, afterwards abolished by Mr. Asquith, and EE substituted for it?—Yes. I think there might be some modification with regard to the certificates.

17789. (*Dr. Gaskell.*) I may take it then that you would like to have some modification with regard to certificates?—Yes.

17790. And larger powers given to certain individuals?—Yes.

17791. Would you specify those individuals?—No, I would not specify them. I think that ought to be determined by the people who sign the certificates; they might act as referees with reference to that. They might be defined perhaps as teachers and professors of physiology in universities and teachers in medical schools.

17792. It has been suggested to us that they might be defined as heads of laboratories?—I think that would be quite a suitable thing.

17793. (*Sir William Church*). Would you be in favour of doing away with all certificates?—No, I do

not think I should. I think there might still be certificates for certain animals.

17794. But you would be in favour of doing away with all certificates for persons who were in the position of head of a laboratory?—Yes, I think so.

17795. But for men who held less responsible and less well-known positions, you would still require Certificates A, B, C, and EE?—I think the certificates might be simplified.

17796. (*Dr. Gaskell.*) Have you read Sir Victor Horsley's evidence on that point?—No. It is not a thing that I have given much thought to, but I certainly quite agree that heads of laboratories ought to have further facilities.

17797. (*Sir Mackenzie Chalmers.*) You told us that you thought the Act ought to be kept up?—An Act.

17798. Is that for the reason that having an Act tends to keep up a high standard of humanity, or for the reason that it tends to allay public apprehension?—That it tends to allay public apprehension. I do not think there is any necessity to set up a standard of humanity for the gentlemen engaged in work of this kind, because their object is to relieve humanity. *Mr. J. R. Bradford, M.D., D.Sc., F.R.C.P., F.R.S.* 26 Nov. 1907

17799. Would there not be danger without the Act, of experiments being performed by quite a different class of people, who might not have the same qualifications or the same standard of conduct?—I imagine that those individuals come under the Prevention of Cruelty to Animals Act—Martin's Act.

17800. They would now; but if you abolished this Act, would there not be a danger of unscientific people in the name of science performing reckless experiments?—I am not in favour of doing away with an Act.

THIRTY-NINTH DAY.

Wednesday, 27th November, 1907.

PRESENT:

The Right Hon. The Viscount Selby (*Chairman*).
Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. J. Tomkinson, M.P.

Captain C. Bigham, C.M.G. (*Secretary*).

Mr. Henry Head, M.A., M.D., F.R.S., F.R.C.P., called in; and Examined.

17801. (*Chairman.*) You are a Doctor of Medicine, a Fellow of the Royal Society, and a Fellow of the Royal College of Physicians, I believe?—Yes.

17802 And Physician to the London Hospital?—Yes.

17803. You desire to offer some evidence to us, I believe, with reference more particularly to the relation of diseases of the nervous system to experiments on animals?—Yes.

17804. First, I think, you desire to point out how far experiments on animals have led to the elucidation of difficulties in the structure and functions of the nervous system?—If I may, I want to point out that all rational diagnosis and treatment must be based upon knowledge of the structure and functions of the nervous system. Sixty years ago little was known of its structure, and still less of its functions. Charles Bell and Majendie had proved that the anterior roots were motor and the posterior roots sensory in function. Marshall Hall was describing reflex action in the spinal cord. It was then an entirely new idea. A few diseased states were known to be accompanied by gross changes, but none of the paths by which impulses pass to and from the brain were then known. I thought it might interest you to hear what Sir Thomas Watson, in his text book (Lectures), said in 1845. He explains the peculiar difficulties to which the physician was exposed when dealing with nervous disease by saying, "One source of difficulty lies in the circumstance that the structure of the nervous system has no perceptible or understood subservience to its functions. We do not discover in the mechanism of this system that adaptation of means to an end which is so conspicuous in many other parts of the body; and consequently, though such adaptation doubtless exists, we are not able to trace the reason or the manner of its interruption"; and again, "It is a further source of difficulty that physiologists have not yet been able to determine with anything like precision or certainty what share the several parts of the brain and spinal cord have in regulating, respectively, the functions which all physiologists acknowledge to belong to the nervous system in the aggregate. There are many and convincing reasons for believing that the brain is a complex organ; but we can seldom put our finger upon this or that portion of the nervous matter which composes it, and say here resides the influence that governs this or that particular function." That was 60 years ago.

17805. Sir Thomas Watson was then, I suppose, a gentleman who might be relied on to give the up-to-date knowledge of his time?—I think so. Sir Thomas Watson was particularly interested, I believe, in matters of this kind. When the part of the book on the nervous system is read, it is very difficult to say how it reads, the ignorance is so profound. When you read the rest of the book on the other systems much of the knowledge given could be transferred to an ordinary text-book. *Mr. H. Head, M.A., M.D., F.R.S., F.R.C.P.* 27 Nov. 1907

17806. We may take that as showing the high-water-mark at that time, then, of the specialist's knowledge on this subject?—Yes, I think so; because he even alludes to the very recent knowledge of reflex action; he mentions Marshall Hall, and he puts in the recent knowledge that was only published shortly before; so that I think we may take Sir Thomas Watson as fairly representative of the professors of medicine of that day, and of the knowledge that was given to students at that time. The whole of the knowledge which we now possess of the structure and functions of the nervous system is due to the close co-ordination of the results of animal experiment and the consequences of localised disease. The anatomist (this is the point I want to make first about anatomy) of the dissecting-room can help us very little. He cannot even say whether the biggest tracts visible to the naked eye conduct upwards or downwards. The beginning of our present knowledge came with Waller's discovery that a nerve degenerated when separated from its nutritive centre. This law was the direct outcome of experiments on animals, and its application to the brain and spinal cord has been responsible for the greater part of that knowledge we now possess of the structure of the nervous system.

17807. When you speak of a nerve degenerating do you mean losing all its functions?—Not only losing all its functions, but undergoing retrograde destruction. When a nerve is separated from its nutritive centre it becomes gradually functionless, but also it becomes gradually destroyed entirely, so that no nervous matter is left behind. Let me explain by a simile how the anatomy of the nervous system depends upon experiment. I am not talking of the functions at all; I am talking simply of the anatomical structure. Let me just take a simile. Imagine a wall covered with creepers arising from several stems. If we wished to know from which of these any one branch takes its origin, we could cut one stem and every leaf arising from it would die, marking out among the healthy foliage the offshoots of the divided stem. This is the principle that has been used in tracing the paths in the nervous system. Gowers, by applying this method, discovered the ascending tracts in the lateral column of the spinal cord.

17808. When you say, by this method, do you mean by experimenting in that way upon living animals?—By applying this method to localised lesions in the spinal cord.

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17809. (*Sir Mackenzie Chalmers.*) Accidental lesions or lesions caused by disease?—Yes.

17810. (*Sir William Collins.*) Did Gowers himself employ both methods?—No, I am just taking Gowers' as an application of this method to an accidental one first.

17811. But did Gowers employ both methods?—Gowers, by this method, discovered the ascending tracks in the lateral column of the spinal cord which had degenerated in consequence of injury. This is an instance of the fruitful application of a law discovered by experimentation on animals to a case of an accidental lesion in man, because, but for Waller's law Gowers could never have used the injury in man to discover ascending tracts in the lateral column. But there was a great deal of dispute about them until Mott and others settled their anatomical connections once for all by animal experiment. Gowers' own observation was not able to give us the complete distribution of these tracts; that only arose where the experiment could be repeated several times under set conditions in animals. From that time the preliminary observations made by Gowers have been verified by several others, and made perfectly certain now by animal experimentation, and we now know the greater part of the distribution of this path. The lesions of disease are usually too diffuse, and the opportunities for *post-mortem* examinations too uncertain, to give clear answers to a direct question. By experiments on animals a tract or set of tracts can be divided precisely; the animal is kept alive until degeneration has taken place, and is then killed. By suitable means the dead parts can be coloured so as to stand out clearly in the microscopical picture. The method by which these dead structures are made to show up clearly against the healthy parts was discovered by Marchi from experiments on animals. Waller's law and Marchi's method applied to material obtained from experiments on animals and from disease or injury in man, are responsible for almost all our knowledge of the anatomical paths in the nervous system. But for experiments on animals we should not have had, so far as we can tell, Waller's law; and the method of colouring the diseased parts (which had been the greatest difficulty in applying Waller's law to the nervous system) was discovered entirely by experiments on animals by Marchi.

17812. (*Mr. Ram.*) The knowledge of one was made available by the method of the other?—Yes. If I may just be allowed to say so, our present knowledge of the anatomy of the nervous system depends to an enormous extent on methods discovered from experiments on animals, and in the utilisation of these methods upon material derived from the results of disease or accident. When we turn to the knowledge of the functions of the nervous system still more do we require controlled experiments to correct and amplify the uncontrolled results of disease. For a knowledge of ingoing sensory paths we are obliged to fall back mainly upon disease, because man alone can tell us of his sensations, but disease produces irregular and diffuse destruction, and the most instructive conditions rarely come to post-mortem examination. Even here, when observation of disease on man starts with a preponderating advantage, in that man can tell us of sensations, experiments on animals are necessary to supplement the rare opportunities offered by disease. On the motor side experiments on animals have the overwhelming advantage that the destructive lesion or the stimulus can be localised accurately, and observations can be made, under the direct conditions of experiment, time and place being convenient. Hughlings Jackson discovered by long and laborious observation that when certain parts of the surface of the brain were the subject of disease, certain movements were produced in the limbs. But these results remained a brilliant hypothesis until Hitzig in Germany, and Ferrier in England, showed by simple direct experiment that stimulation of the surface of the brain in animals produced movements of the limbs such as Jackson had described. In spite of a generation of clinical observation since that time, many important details of cortical localisation still remained undecided until Sherrington and Grünbaum's experiments on the anthropoid apes; that is to say, that although Dr. Jackson, by the most brilliant intuition and most laborious observation, said that stimulation of certain parts of the brain marked by disease were associated with these convulsive movements, few, if anybody, believed him, because everybody said that it is impossible to stimulate the brain. They held exactly the idea that is given in Watson—that the brain was a sort of independent structure, in which no function was placed in one part or in another—that the brain was the seat of a sort of generalised function. But it might be asked: Why not experiment on men who offer themselves voluntarily? I hold very strongly that no man has the right to be the subject of an experiment unless the whole conditions surrounding that experiment are previously known as well as they can possibly be at the time. In my own case the most careful observations were made upon patients suffering from accidental injuries to nerves. Then the experiment was designed so that the question which could not be answered by these accidental lesions was put directly, and the answer given clearly.

17813. (*Sir Mackenzie Chalmers.*) In the case of an animal?—In the case of myself. No man has the right—I want to insist upon that—to experiment upon himself unless the whole of the conditions of the experiment are known beforehand, so far as it is possible. That is to say, that vicarious experiments are useless, and that an experiment on man, if it occurs—self-experimentation—must be the culminating point in a complete series of thoroughly thought out data, which can only be obtained by a long course of observations of disease, together with animal experiment. Every now and then, for the relief of pain, man is operated upon under conditions that closely represent those of animal experiment. I refer particularly to such operations as the division of the posterior roots for pain and the operation of the removal of the Gasserian Ganglion at the base of the skull for persistent neuralgia. From such instances, when they occur, we can obtain the most valuable information, but such cases can only occur with excessive rarity, and, as in the case of the Gasserian Ganglion, it is always the same structure which is removed.

17814. (*Chairman.*) When you say such cases, do you mean cases in which a surgeon would venture on such an operation?—Yes; I obtained most valuable information from two of Sir Victor Horsley's patients, in whom he had divided the posterior roots in the back for persistent neuralgic pain; but such cases occur with excessive rarity.

17815. When you say information, do you mean that you questioned the patients about their sensation?—I examined them as if they were animals only, with a human consciousness. People may say that there occur in human beings opportunities in consequence of surgical operations which are equally good with those of animals, and give you the advantage of having a man to deal with. But those are excessively rare, and not only that, but when they are not excessively rare, as in this modern operation of removing the ganglion at the base of the brain for this persistent neuralgia, it is always the same structure that is removed. I do not want at the present stage to see any more patients in whom the Gasserian Ganglion has been removed.

17815A. Will you explain?—At the base of the brain the whole sensory nerves of the face and head come together, and form a sort of knot. This knot or junction point is the part from which all the ingoing lines pass into the brain, so that they carry the whole of the sensations from that half of the head into the brain. Now, for extremely severe neuralgia this knot is cut, and the junction is removed, so that the pain impulses no longer pass into the brain.

17816. Do you mean pain impulses coming from the rest of the body?—No, from the face and head; it is facial neuralgia. They no longer pass inwards, and the whole of that part of the face is made entirely devoid of sensation.

17817. Is that an operation that is successfully performed now?—It is a brilliant operation; it is brilliantly successful.

17818. Is it successful in its after results?—Yes, brilliantly successful, if it is done with care by the people who can do it.

17819. (*Sir Mackenzie Chalmers.*) Is it a dangerous operation?—Yes, but the mortality is going down enormously; now that people are getting skilled the mortality goes down every year.

17820. Does the destroying those sensory nerves produce any other ill-effects afterwards? Have you

watched cases for a number of years?—Yes. One particular case that I have under observation now was done about 1897—ten years ago.

17821. (*Chairman.*) Does it affect the vitality of the patients at all generally?—No, not generally. It enormously improves it, because they have been the subject of this terrible pain over long years, and the relief of that pain often turns them into entirely different beings from the patients you have seen before.

17822. Does it alter the appearance of the person's face?—Yes, because you must divide the motor nerve for certain parts of the face, too; you cannot get the one away without the other.

17823. Does it take away the power of movement?—Part of it, not altogether.

17824. (*Mr. Ram.*) Is the whole of that side of the face affected permanently?—Yes, permanently.

17825. Is it dead to all sensation?—Very nearly.

17826. Does it feel a touch or a prick?—To a varying extent. There are parts which are entirely insensitive to everything; and pain, of course, disappears with it.

17827. (*Sir William Collins.*) Is the nutrition of the parts supplied by the divided nerve affected?—Somewhat.

17828. Detrimentally?—Not with care; it does not heal so easily quite on that side if there is an injury, but not otherwise.

17829. Is the eye affected?—That depends entirely upon whether you have taken care or not. If you have taken care, in the present day, the eye is not affected, for you can be careful to shield it.

17830. No ulceration of the cornea follows?—Not necessarily. In the modern operations the eye escapes altogether. In the old cases the eye used to suffer a good deal.

17831. (*Sir Mackenzie Chalmers.*) Was that from accidental division of nerves affecting the eye?—The eye loses its sensation.

17832. (*Sir William Collins.*) You spoke of the mortality having fallen enormously. Was the mortality originally enormous?—Yes, it was high at first.

17833. About what was it?—I would not like to say exactly what the first mortality was. I should have to look it up, but it is going down rapidly.

17834. (*Chairman.*) You only performed this operation in cases where the pain was so severe that it made life intolerable?—Absolutely. That is the question you always ask; Is life intolerable?—That was not quite the point that I wanted to make. The point I wanted to make was that these occasions arise very rarely, and although you might say—Ah, yes, but you have got, let us say, statistics of hundreds of Gasserian Ganglion operations to deal with, yet it is always the Gasserian Ganglion.

17835. (*Mr. Ram.*) You want to learn about other nerves?—We want to learn about others; you cannot go on for ever learning one particular thing. I have not the slightest doubt that years hence, when our knowledge advances, we may go back to this and learn more. What I wanted to prove was that animal experiment has been necessary, not only for the study of the function, which is more easily, perhaps, understood, but also has been absolutely essential in the progress that we have made towards our knowledge of the anatomy of the nervous system. And, then, it is not animal experiment only, but it is animal experiment, closely interwoven with other forms of experiment; that is to say, I want to show how closely related animal experiment and the utilisation of material obtained from disease or injury are to one another; that but for animal experiment we could not have utilised in the way we have done material obtained from disease. And I may perhaps be allowed to pass on to tell the story of a small research, which happens to illustrate this close inter-relation. The problem of this particular research was given by disease, and it was—Why does pain caused by irritation of some internal organ radiate on to the surface of the body or into the limbs, and why do these parts become tender? It had long been known that when an internal organ (let us take the heart) was affected, the pains were not always situated over the heart; they seem to go into other parts of the body. Now, in consequence of the work of Dr. Gaskell, some of us suspected that this was a definite phenomenon, and was the way in which the internal organs expressed themselves when they were diseased. First of all, Dr. James Mackenzie and myself independently attacked this problem, and we examined for a number of years the actual facts as they occurred.

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17836. (*Chairman.*) In patients?—In patients in the hospital. We selected instances in which one organ, if possible, was affected. Take, for instance, such a disease as stone in the kidney, where we watched the pain produced by the stone, and then we saw the stone removed, and saw the pain disappear, and by a long and laborious series of observations, extending over a number of years, we determined that when an internal organ was affected in this sort of way it expressed itself by pain radiating round the body, associated with areas of tenderness on the skin. That is a good instance of the phenomena of referred pain. But, of course, I need scarcely say that if you attempted to describe phenomena like that people might say—Oh, yes, we know that it happens, but it is all hung in the air; it has no relation to anything whatever that we know. So little had it any relation to anything that was known that most people doubted that these areas occurred at all. But throughout the whole research we were much comforted and supported by Sherrington's experiments on the distribution of the posterior roots. Sherrington found by a most ingenious method of experimenting, which I do not think I need explain, that supposing you worked out the parts supplied by one posterior root you found that they spread over the body and limbs, and were distributed in a sort of zone, like a belt round the body; they went round like a half-belt; and it was the fact that I was in touch throughout the whole of my observations with Sherrington, who was at that time working out these zones which were previously entirely unknown in animals, that made me perfectly certain that my observations on referred pain were correct, and enabled me to propound a set of figures, what you may call a map of the body. This I should never have dared to put forward had I not known that in the nervous system there lay the possibility of a series of zones of this kind; that is to say, it was knowing by experiments on animals that the posterior roots followed a course somewhat of this kind, that made me much more confident that what I found in disease was really the fact.

17837. Were the experiments on animals for the purpose of verifying an idea which had presented itself, apart from experiment?—No, Sherrington was working entirely independently. He went purely for knowledge. What is the distribution of the posterior roots is what he asked himself; that is, how are they distributed, and he worked that out by animal experiment. That was undertaken entirely apart from any thought of disease or anything to do with it, only it so happened that we were both influenced by Dr. Gaskell's ideas, and so he took the experimental side of the work, quite independently, and I wanted to see how far this idea could be borne out by the examination of cases of disease of patients.

17838. By observation?—By observation. But what I wanted to impress upon the Commission was that taking this small piece of work I would never have dared to put forward a map of the body worked out in this way from observation only, had I not known that the structure and functions of the nervous system contained something which was similar.

17839. For that purpose how long did you carry on experiments on animals?—I did not do any then at all. I was observing human beings.

17840. How long did Sherrington carry on experiments on animals?—I think some four or five years.

17841. And in that time he operated on a great number, I suppose?—A considerable number.

17842. 200 or 300?—I could not tell you without looking it up.

17843. What kind of animals did he use?—Monkeys and cats.

17844. Those were the most suitable for that purpose?—The monkeys particularly.

17845. Then he would use a good many, I suppose?—I do not know exactly how many.

17846. Would it be necessary in that case to get a certificate for letting the animal come to?—Yes, because he divided the roots; he cut the lines below and cut the lines above, leaving the one in between untouched; then the animal had to come to, and the sensory ideas were marked out later on when the animal was quiet.

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17847. When the animal was under anæsthetics the only operation was a cutting one?—Yes.

17848. Was the wound then allowed to heal?—Yes, the wound was allowed to heal in many cases.

17849. Then after the operation had been performed there would not be artificial irritation of the nerves of the body?—No, there was none. The only way in which it was done was by stroking or pricking the skin.

17850. To see whether there was sensation—not pain?—No, not pain at all. Sherrington would take a pencil, for instance, and rub it along, and then when the animal moved he knew that the movements must have been supplied by the other root that he had left there which was sensitive.

17851. (*Mr. Ram.*) Would the animal be in any state of suffering on account of the operation?—Not the least, except for possible post-operation discomfort.

17852. The healing of the wound?—Yes. The whole point in the operation was that the animal should be as normal as possible, otherwise it was no good.

17853. (*Mr. Tomkinson.*) Then after the wound was healed, would that monkey be operated on again for further experiment, as a rule?—Not necessarily; there was no necessity.

17854. But it might be; it had not to be destroyed?—That depended upon what the nature of the experiment was.

17855. In certain experiments was the whole object to see how the healing took place?—No.

17856. How the animal recovered from it?—No, it was allowed to recover that its sensations might be tested.

17857. Tested for what?—Tested for touch. You see, Sherrington had taken away sensation above and taken away sensation below, and left a little oasis of sensation in between in a desert of loss of sensation. You passed through the desert of loss of sensation, and the animal made no response; you came into the little oasis of sensation, and it responded at once; put its hand out, or whatever it was, and then you knew that it must be supplied by the only root he had left uncut.

17858. (*Sir Mackenzie Chalmers.*) That, of course, was a painless proceeding?—Yes.

17859. Merely tactile?—Yes, or no more than the prick of a pin.

17860. (*Chairman.*) As regards further operations, I suppose that for physiological purposes the animal might be fit for other operations on different parts of the body?—Yes, just as a man who has had his Gasserian Ganglion cut out of his face.

17861. But whether that can be done is rather a question of law than for you?—Yes, that is what I meant.

17862. (*Mr. Tomkinson.*) Would it be a perfectly healthy animal after the operation?—It would be as healthy as a man who had had the nerves of his face divided.

17863. Nature would not restore the lost part, would it?—No; arrangements were made that it should not.

17864. Do I correctly understand that there was a certain severing and taking away of the nerves above and below?—Yes.

17865. Those nerves would not come together again?—Arrangements were made that they should not, just as in human neurectomy.

17866. That leads up to a question I wanted to ask. You know, no doubt, that there is sometimes performed what, I think, is a very cruel operation on a horse, unnerving a horse to make a horse which is lame in the feet insensible in the feet altogether, to practically give it dead feet?—Yes.

17867. I understand that a piece of nerve is taken out?—Yes.

17868. And for a time the horse's feet are absolutely insensible; it will go on hard ground that it could not go on before?—Yes.

17869. But in course of time that nerve grows together again?—Yes, because it is not done in such a way as to prevent the nerve coming together again. But it can easily be done in such a way as to prevent the nerve coming together.

17870. (*Chairman.*) Whether or not that is a cruel operation, so far as regards the operation itself on the horse, could it be done under anæsthetics if the veterinary surgeon performing it thought right?—Yes, perfectly well. And not only that, but if it was done so that the nerve did not come together, it would not be cruel in any sense of the word.

17871. It would be a painful operation, I suppose, if the horse was not under anæsthetics?—It is difficult to say how painful. I should not think it would be excessively painful.

17872. And not lasting?—If you did it extremely quickly it would be a momentary violent stab of pain and then the thing would be over; but the only pain that can come in is when the nerve regenerates, not when the nerve is divided. If the nerve remains divided, no pain can come in. It is when the nerve regenerates that the pain comes in.

17873. I introduced that in the course of what you were saying, because I was anxious to bring what you said to bear on the subject of our inquiry, namely, the law relating to operations on animals. Will you now proceed?—I wanted, in this account of the small research in which I was personally concerned, to bring forward how closely all these methods are united together, how they are all woven up together. The next stage was to find out what happened if these parts that Sherrington had been working upon were irritated in man. That, of course, could not be done by an ordinary experiment obviously; but it so happens that there is a disease which is known to everybody, shingles, which produces the most violent irritation of this particular part. That is well known now ever since 1860.

17874. Which particular part?—This posterior root, the ganglion. But by working on the shingles, by working on the material, the dead-house material, microscopically, and using the method for colouring that had been discovered on animals, we were able to show that the irritation of the ganglion was able to produce these zones on the body closely resembling, but differing on the other hand to a certain extent from, those obtained by Sherrington. That last step in the research was given by the working up of dead-house material, of the material on dead human beings, by means of a method which was discovered by experiments on animals. At the end of this stage we at once saw how little we knew about the distribution of the nerves on to the surface of the body. Generations of anatomists had dissected the nerves of the human body, and it was thought that we knew all that was to be known about their distribution. Yet if a workman cut his wrist and came to the hospital with one nerve divided, the actual anatomical facts could not explain what we saw in any single case. Anyone who was honest when a workman came to the hospital, any physician or surgeon who saw him and was honest with himself, had to say, our knowledge gained from dissection tells us nothing with regard to the sensory condition of that man's hand. But there did not seem to be any way of working it out at first, and we were so impressed with the absence of knowledge on these points, that we set to work first of all to observe what actually happened when by accident various nerves were divided. We began to suspect that there were certain laws underlying the whole thing which had never been formulated. But good as hospital patients are (and they are extraordinarily good from the point of view of many observations) it was obvious that no working man could give the time required or the attention for a long and elaborate series of observations. Therefore after everything had been thought out in the most careful manner, and after we had made several hundreds of observations on ordinary hospital patients, I determined to have three nerves divided in my own arm. This was done with the most successful results; the question that we put by our operation was answered immediately with a clearness that we could not obtain by any other means, and for five years we have been working out the results of that experiment.

17875. Do you mean that you ascertained exactly what parts of the hand lost their use or their sensation in consequence of the severing of those nerves?—We knew before from our observations what parts would do that; from previous observations we knew what parts would probably lose their sensation. But Nature, or rather accident, cuts the wrist or the arm, say, with a circular saw, and you cannot tell a circular saw that you only want sensory nerves cut—it

cuts everything it comes across. Therefore the man loses not only sensation but motion. The problem that we wanted settled was, if only the skin is made anæsthetic what is the result? That was the point.

17876. Only the skin?—Not the parts supplied by the nerves which go to muscles. The problem we wanted to solve was this: There go to the hand, nerves which go to the muscles, and nerves which go to the skin. Ordinary accidents divide both. We wanted to know what happens if you divide only the nerves that go to the skin and leave all the nerves which go to the muscles untouched. The day after the operation that question was solved, and we thereby obtained a knowledge of an entirely new form of sensibility; a form of sensibility which had never been described before. A direct answer was given to a direct question.

17877. Could you take your three nerves, for example, and could you say what was the result of the severance upon the sensation in your hand or in what part of your body?—We desensitized the skin over the whole of the part. (*Describing the same.*)

17878. Did you find that it destroyed sensation in the skin in that part only?—Yes, we took only the skin nerves and left all the nerves which go to muscles, that was part of the operation; and the result was that we discovered that under this sensation, which is what we all have when we touch ourselves like this (*describing*), there lay an entirely unappreciated sensory mechanism, which is responsible for an enormous amount of our sensation, but that it runs with muscular nerves, which was an entirely new fact, and a thing which has revolutionised the conception of sensation altogether. Because in old days we would have been told, if you cut all the sensory nerves going to the skin you desensitize the part. It is nothing of the kind. You must cut the sensory fibres in the muscular nerves before you do that.

17879. You did satisfy yourself by an experiment on yourself; but could you have satisfied yourself by experiments on animals?—No. I was going to point that out to you, because we could not test the sensation of animals. But the whole point is, that running alongside came a series of experiments on the way in which nerves united in animals, which were necessary to show how these reunited nerves conducted. That has been going on upon animals, and can only be done on animals. Therefore the points that I want to make so strongly are, that in every inquiry into the structure and functions of the nervous system, observation of disease and experiments on animals are intimately interwoven; the one amplifies the other, and neither can be fruitful alone, because, as I have shown before, the very methods by which we examine disease are discovered from experiments on animals. Experiments on animals cannot tell us about what a human being feels; for that we require disease; but on the other hand in many of the cases where we have to depend upon human beings, we only know what is the matter with them because we have made that lesion in animals. So that the whole thing is all woven together like a fabric, and you cannot pull out one coloured thread without altering the pattern. Lastly I did not know whether it would be of any value simply to put one's self forward to answer any questions; as for practical purposes I am an animal that has been operated upon under Certificate B.

(*Chairman.*) I will leave that to some of the gentlemen who understand the matter more than I do.

17880. (*Sir William Church.*) You have given us a very interesting account of experimentation on animals and on yourself with regard to the nervous system, but I suppose you would say that, *mutatis mutandis*, all that you have said applies in a very large measure to researches into other organs in man?—Yes. I have been particularly interested in the nervous system, and also the nervous system was within so comparatively short a time a *terra incognita*. Our knowledge of the nervous system as exhibited by Watson is like the old maps of Africa, undiscovered country. Now we know its paths, because within a comparatively recent time we have learnt such an enormous amount about them; that is why I chose it particularly. But, *mutatis mutandis*, it is absolutely as you say.

17881. But you would, I suppose, agree that the same methods of observation and experiment are required for researches into our knowledge of other groups of organs in man?—Absolutely. I think it is only a small example, taken from the nervous system, of great general methods.

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17882. You personally, of course, do not maintain that experimentation upon animals should take the place of observation upon man in what we call clinical observation?—Most certainly not; the one can only be fruitful with the other.

17883. The two should be worked together?—Yes. It is absolutely necessary that the two should be worked together. The great progress in our knowledge of the paths of the nervous system came from Waller's discovery, which was made on animals.

17884. So that the great value of experimentation on animals is that by that means you can put a definite question as it were to Nature, and get a definite answer?—That is it exactly.

17885. Therefore, as your research into any group of organs goes on, so it is necessary for you to put definite questions and obtain, if possible, absolutely certain answers?—Yes.

17886. So that in your view the great value of experimentation on animals is in addition to our knowledge either for scientific purposes or for curative purposes?—Yes. I should like also, just as a corollary to that, to point out the uselessness of experimenting for a practical end. Men have been experimenting for practical ends and observing for practical ends with regard to pain and disease with comparatively little progress.

17887. Because they have not sought for knowledge but for practical results.—Yes. It was only when we determined to see whether we could not apply the theoretical results obtained by Dr. Gaskell to the structure and the functions of the nervous system that we discovered these other laws, which are of daily practical importance.

17888. Consequently those who object to experimentation on animals upon the ground that immediate practical results are not obtained simply do so because they do not understand the way in which practical results can be obtained?—Yes; certainly. Practical results are the bye-products of a manufacture of which knowledge is the aim.

17889. (*Sir William Collins.*) May I ask you one or two questions with regard to the very interesting experiment that you carried out upon yourself? It would not have required a licence under the Vivisection Act, of course?—I am not a domestic animal.

17890. It would not, therefore, have required a licence under the Vivisection Act?—No.

17891. I understood you to say that that experiment which you made entirely revolutionised our conception of sensation?—If the results that we have recorded are right, it has done so because it has discovered for us something that we did not conceive before. It has discovered for us the fact that nerves which run with the muscular nerves which were supposed to have certain comparatively inferior functions, have very much higher functions and are of very much more importance in the field of sensation that was ever supposed.

17892. You say, if the results that you derive from your experiments are correct. Have you any doubt as to the results?—I have not, but other people may have. That I leave to time; only time can tell that.

17893. The difficulty that you apprehend is in convincing others?—No, I think it is like all new things. They have to undergo the test of time.

17894. (*Sir Mackenzie Chalmers.*) One experiment is not conclusive?—It is one in a series of advances.

17895. (*Sir William Collins.*) I am anxious to know why you qualified that previous answer by saying if the results you obtained were to be regarded as correct?—I think that is the attitude of every one who is a scientific worker. Personally I do not doubt it.

17896. You have no doubt that this experiment has revolutionised our conception of sensation?—Certainly it has revolutionised my conception.

17897. And that you discovered a form of sensation which was never described before, I think you said?—Not in full.

17898. Would you tell me what are the three nerves that you divided?—They are the radial, and the two branches are what is called the external cutaneous, one of which runs down the front of the forearm, and the other down the back of the forearm.

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17899. Was it not surmised previously to your experiment that the radial nerve was concerned in cutaneous sensation of the extensor surface of the forearm and hand?—It was supposed to be proved that it was not. If you look up Bowlby's book, you will find that he showed that when it is divided, there is no change of sensation.

17900. Do you mean that I shall not find in any physiological or anatomical work prior to your work any such suggestion?—I do not for a moment mean that. That was not the experiment.

17901. That is my question?—No. The anatomist has always said that the radial played a considerable part, but when the radial was divided in man we could not find out that anything of the kind happened. That was one of the most characteristic instances where anatomy and fact did not go together. The anatomist always described (as you know from your books) a large area supplied by the radial. He put it down with very marked local borders like the borders between Switzerland and France and other countries. But when the radial was divided, nothing apparently happened.

17902. You stated that anatomical knowledge told us absolutely nothing as to sensation of the hand?—Quite so.

17903. Do you think that anatomical knowledge has not told us anything as to the radial nerve being associated with sensation of the extensor surface of the forearm and hand?—But it did not explain why, when you divided the radial nerve, you could not get the result that anatomists said that you should get. That was the trouble.

17904. Will you tell us what was the result of dividing the radial nerve in your own case?—I did not divide the radial nerve only; I divided it together with two other branches.

17905. Will you tell me what the result of the division of the radial nerve in your own case was?—I did not divide the radial nerve only. I divided it together with two others, so as to make quite certain that I got a loss of sensation which I found that I could not get by dividing the radial nerve only.

17906. Have we any knowledge of the part played by the ulnar nerve in respect of sensation motor power?—The motor power was accurately known; the sensation was very imperfectly known.

17907. Was nothing known of sensation of the hand in reference to the ulnar nerve prior to your experiment?—I am afraid I do not quite understand you. My experiment had nothing to do with the division of the ulnar nerve.

17908. But I understood you to say that anatomical knowledge had told us absolutely nothing as to sensation of the hand, and you said that the distribution of the ulnar nerve suggested nothing as to sensation of the part of the hand that it supplies?—That has nothing to do with my experiment. That has to do with my observations on man.

17909. It has to do with your statement that anatomical knowledge has taught us nothing as to the sensation of the hand?—That is perfectly true, but that has nothing to do with my experiment; it has to do with the observations made by Sherrington on animals and myself upon human beings, where the ulnar nerve had been divided. It was found in those cases that if you gave the anatomist a patient in whom the ulnar nerve had been divided, he could not tell you whether it had been divided and partly re-united, or whether it had not been divided, except by looking at the motor supply. If by any chance the motor supply was uninjured, he could not tell you in what condition that nerve was; he could not tell you whether to cut down and suture that nerve, nor could he tell you if you did cut down upon it what you would find.

17910. Had it been observed by surgeons that when the ulnar nerve has been divided at the elbow joint, there has been loss of sensation in the little finger and in the adjacent portion of the next finger?—But that is not sufficient to tell you whether it has been divided.

17911. Did we know absolutely nothing of sensation in the hand from such observations as that?—Nothing that would be of practical importance to you; so much so that it was the habit to neglect sensation and to depend entirely on the motor condition.

17912. Will you be surprised to hear that a surgeon seeing a case of division of the ulnar nerve at the elbow joint, and finding loss of sensation in the little finger and the adjacent portion of the next finger, had cut down upon that ulnar nerve and re-sutured it and found sensation restored?—Not in the least, because he cut down without knowing, except on the motor side, what he was going to find. Time after time I have known the ulnar nerve cut down upon in ignorance that it had reunited and reunited. Recently a patient of my own, in whom the ulnar nerve was united, was operated upon and the nerve exposed, simply because it was not known whether that nerve was united or not. We knew that it was united, and it proved to be so. Anybody who has had experience in this side of practice knows how difficult it was to come to a correct conclusion.

17913. What is the form of sensation which you are able to describe now, which was unknown before your experiment?—You are passing on to my experiment now. What is called deep sensibility.

17914. Would you tell us a little more about it, please?—It was found that after the operation a touch with the finger could be appreciated and localised although the whole of the skin had been rendered insensitive.

17915. (*Mr. Tomkinson.*) By considerable pressure?—No, only a touch. Secondly, it was found that by means of pressure pain could be produced with the same amount of pressure as on a normal hand.

17916. (*Sir William Collins.*) You mean that the sensory fibres apparently travel along with the motor fibres?—They do. Are you surprised to hear that? The surprise in your question is the measure of the new idea.

17917. I express neither surprise nor the reverse. I am asking you a few questions on the result of your own experiment?—The surprise of that question shows the new idea.

17918. I am afraid that you must not argue surprise from the question. Does that experiment then necessitate a revision of our opinions as to the distinction usually drawn between motor and sensory nerves?—No, between muscular and cutaneous nerves, which is a different matter.

17919. Do you distinguish between touch and pain and sensation of temperature?—I do not quite understand the question.

17920. When one speaks of sensation, is it necessary to discriminate between sensation of pain, sensation of touch, and sensation of temperature?—Do you mean from the point of view of the skin or the deep structures, or from the point of view generally?

17921. Either?—Do you mean from the point of view of the spinal cord or the brain or the peripheral nerves, the nervous system you are concerned with? That is the point.

17922. Do you, or do you not, consider it necessary to make a distinction in speaking of sensation generally between sensation to touch, sensation to pain, and sensibility to temperature?—Yes, discriminating also heat and cold.

17923. Would you tell me whether those forms of sensibility travel along with the motor nerves or with the cutaneous nerves?—One form of touch travels along the cutaneous nerves, and one travels along the deep fibres; one form of pain travels along the cutaneous nerves, and one travels along the deep fibres. Heat and cold travel solely along the superficial, along the skin fibres. Localisation of a spot touched travels along the deep fibres. Localisation of a spot touched travels along the superficial fibres; but the localisation of a spot touched travelling along the deep fibres does not carry the power of distinguishing two points; sensory impulses travelling from the skin carry the power of discriminating the two points. You see why I had a difficulty in answering the question.

17924. I am very much obliged to you. May I take it that those views are generally accepted by physiologists now?—No; they are new.

17925. Since when?—Since the work has been going on. I suppose the first publication of it was in 1905.

17926. With regard to your experiments in association with Professor Sherrington?—I was not associated with him. Sherrington was working independently entirely.

17927. I thought that you urged the importance of associating the results that you obtained with those that Professor Sherrington obtained?—That is quite right. We met, but we were marching entirely inde-

pendently. Sherrington published his results in the usual way, and it was my knowledge of his publications that helped me so profoundly in my research.

17928. As the result of those experiments and observations of yours and Professor Sherrington's, whether in association or not, do I correctly understand that you are able to localise the position of stone in the kidney, in the ureter, or the bladder according as the pain is distributed in the thigh or the buttock?—Not by pain only. There is no touchstone in medicine. That is only one of a set of facts which have to be taken into account in making a diagnosis. No diagnosis is made on one method of examination or one set of facts. A diagnosis is made and acted upon only by co-ordinating a large series of facts brought together by different means, and this is only one of them.

17929. Then do I rightly understand that localisation of pain will not localise a stone by itself?—Certainly it will not. A better instance in some ways is the pain down the inner side of the arm, produced by heart disease, which is a thing that was not understood at all.

17930. You spoke of the degeneration of nerve. Have you worked at the regeneration of nerve?—Yes.

17931. Am I right in thinking that there are two schools as regards the regeneration of nerves: those who argue for the peripheral regeneration, and those who argue for the central regeneration?—Yes.

17932. May I ask whether you can tell us your own view on that subject?—No, it would be impossible, because we are beginning to see that the problem is not so simple as that. It would be quite impossible here to explain the position that I hope to adopt in the matter.

17933. We have been told that there are peripheralists and centralists, and one witness, at any rate, classed himself with the one or the other. You are not able to do that?—That is too dogmatic. The problem is a more complex one than that.

17934. Have experiments been made with a view to elucidate that problem?—Yes; those are the very ones that I have been making.

17935. Have both peripheralists and centralists claimed, as the result of experiments on animals, the accuracy of their view?—Yes, certainly, because it is like all those things; we have to work on both sides of the question before we ultimately reach the truth.

17936. You alluded to Sir Charles Bell's work?—Yes.

17937. It has been claimed, I think, that Sir Charles Bell's work was the greatest that had been done in physiology since Harvey. I do not know whether you estimate it as high as that?—I think the knowledge that the posterior root is sensory, and the anterior root motor has had an enormous effect upon our knowledge of the nervous system; that is the way in which I can put it.

17938. Was the knowledge of the nervous system obtained by Galen valuable?—I am afraid I do not know; because I am afraid that I know very little about it.

17939. We have been told by one previous witness, I think, that much of the knowledge of Galen was valuable to-day, and others, I think, have rather disputed that view. I was wondering whether as a neurologist you could give us your view?—The only way in which I should put it is that in 1845 it had no fruitful effect upon knowledge as judged by the ordinary standards of the time.

17940. The experiments of Hierophilus on human beings have also been alluded to; do you consider that they yielded any knowledge?—I do not know anything about them. The point is that 60 years ago our knowledge of the paths in the nervous system was exactly in the condition of a map of Africa, and now we know a number of paths, a number of tracts, and a number of territories. That is the point.

17941. We have had our attention called to the opinion of Sir Charles Bell as to the comparative uselessness of experiments on animals for the purpose of ascertaining the structure and functions of the nervous system. He said in one of his works "Experiments have never been the means of discovery, and a survey of what has been attempted of late years in physiology will prove that the opening of living animals has done more to perpetuate error than to confirm the just views taken from the study of anatomy and natural motion." I gather from what you have told us to-day that you do not agree with that view?—It is not a question really of whether I agree or not; it is a question of pure fact. Sir Charles Bell's statement there is not worth any more than the statement of any other person on that matter. The point is how much more do we know now in consequence of direct experiment. Bell did not know what we know. You cannot foretell. He only made that statement because it was his impression of his own time—that is a long while ago. We know a great deal more since then, and not only do we know a great deal more, but we also know how much more work we have got, so to speak, on our hands.

17942. That was in 1836?—Yes.

17943. At that time had Sir Charles Bell himself advanced our knowledge of the nervous system?—He stated the hypothesis that the two roots had different functions.

17944. Was it only a hypothesis in Bell's time?—That, of course, is a very disputed point as to how far his statement could be taken as a definite proof; it was definitely accepted 10 years later, in 1845. But perhaps I may just read you one passage which you will be interested to hear: "It is commonly believed," says Watson, "that disease affecting the *anterior* columns only of the cord, will be likely to disturb, or to suspend, the *power of voluntary* motion in the corresponding parts" (that is the direct outcome of Sir Charles Bell's statement) "to produce *spasm* or *palsy*, and that disease affecting the *posterior* columns alone will be likely to alter or abolish the *faculty of sensation* in the corresponding parts; to cause *pain, tingling, numbness*, or *complete anæsthesia*. But I have mentioned certain facts which contravene this opinion. Suspend your judgment respecting it. Neither the minute anatomy nor the physiology belonging to the question are yet conclusively settled. There seems no reason to doubt that disease affecting the *lateral* half only of the cord will be likely to derange both the sensibility and the power of movement, in the corresponding parts on the *same side of the body alone*." So you see the state of knowledge in 1845.

17945. Did Brown-Séquard make further observations on the functions of the roots of the spinal nerves?—Mainly on the functions of the spinal cord; most of his observations were on the functions of the spinal cord; there were some on the posterior roots.

17946. Did Brown-Séquard contest the conclusions that Sir Charles Bell had arrived at?—I am afraid I cannot tell you without looking it up. But Brown Sequard was led by experiments on animals to give an immediate answer to that last statement that Sir Thomas Watson put forward wrongly.

17947. You alluded to the localisation of the cerebral functions as as example of the advance that has been made in our knowledge of the nervous system. Is the association of the left lower frontal convolution with the function of speech, one of the cases of localisation of the cerebral function?—You mean the association of Broca's convolution with speech?

17948. Yes?—Yes, but that does not stand on the same level as others.

17949. Do you mean that it stands on a lower level or a higher level?—It has not been proved in the same way as others; that is just one point at present in dispute.

17950. Do you know a recent paper by M. Marie?—Yes.

17951. He, I think, asserts that the third left frontal convolution does not play any special rôle in the function of language?—Yes, it is just that very point that is in dispute. It is one of the points that we cannot settle by experiment, and it is one in which we have to wait for observations, and we have waited all these years.

17952. Was it one of the earliest localisations?—Yes, one of the earliest and still one of the most disputable.

17953. I think you laid it down that no man has a right to be the subject of experiment unless the whole circumstances are well understood?—Certainly.

17954. That was in regard to himself, and I suppose *a fortiori* in regard to others if they voluntarily agree to be operated upon?—Certainly. An accident may happen which may put things wrong; that is where the risk comes in, but you should take every possible precaution to avoid that accident.

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17955. And, as you said, experiments on animals cannot tell us, of course, what human beings feel. That, I take it, is why you call our special attention to your own experiment, as in your opinion solving a problem which no experiments on animals could have sufficed to solve?—No, what I worked upon were the experiments on animals which worked into the whole network of the research. Self experimentation, microscopial anatomy, and experiments on animals all worked in together.

17956. But previous to your experiment, no experiments on animals, I understand, had suggested that sensory fibres travelled with muscular fibres?—Most certainly they had. It was because Sherrington found by experiments on animals that such a large number of the nerve fibres which run with the muscular nerves did not degenerate like motor nerves at all, but degenerated in the opposite direction, that made us determined to test the thing on man.

17957. But I thought you claimed that this revolution in regard to our knowledge of sensation had been the result of your own experiment?—As regards sensation, that is perfectly true. All we knew was that these muscular nerves contained a very large number of fibres which were evidently not motor. Therefore in my experiment I determined to put that to the test from the point of view of sensation. Sherrington could not, of course, test sensation, but he told us that there were a large number of afferent fibres, whatever those afferent fibres did.

17958. But without your experiment on yourself, by experiment on animals alone, could this revolution in our conception of sensation have been attained?—Not of sensation, but the very fact that Sherrington had told us that there were these afferent fibres made it perfectly certain that there must be a sensory function attached to them. What that sensory function was, was a thing that only a human being could tell us, was what we set ourselves to show; but the idea would not have come to us had we not had this elaborate work of Sherrington's, which told us that a large proportion of the fibres in the muscular nerves were not motor.

17959. Was that arrived at by the method of Marchi, which you have already described?—Yes. Waller's law of degeneration and Marchi's method.

17960. (*Sir Mackenzie Chalmers.*) As regards this operation on yourself, did you have it done under local anæsthesia or general anæsthesia?—General anæsthesia.

17961. After the division of these nerves, did you suffer pain above the division; was there great pain above the division?—Do you mean in the other parts of the arm?

17962. Yes—you divided the nerve?—Yes.

17963. Above the division, of course, the nerve would be connected with the central nervous system?—Yes.

17964. Was there much pain there during the healing?—Absolutely none.

17965. Therefore, presumably, in these experiments on animals the animal suffers no pain?—There is nothing to show that it suffers at all, and I did not. I went about my work as usual, with my arm in a sling.

17966. And it healed in the ordinary way?—It healed by first intention.

17967. How long did it take before the nerves again rejoined and re-established sensation?—Fortunately, one of the stitches was cut out, so that I still have a patch of anæsthesia left.

17968. Insensitive?—No, in the peculiar form of sensibility which we are particularly interested in studying, so that I hope it will last. But all the others are healed.

17969. (*Mr. Ram.*) As I understand, the experimental investigation that you were carrying on at the same time as Professor Sherrington was making experiments on animals, was a case in which a correct theory originated by clinical observation, and was confirmed and proved by experiments on animals. Is that the correct way of stating it?—Hardly. It was the general trend of knowledge at the time, experimental and otherwise, led to the correct theory; it was the whole thing put together.

17970. But you were pursuing it by clinical observation?—Yes.

17971. And the theory that you were led to adopt by your clinical observation was proved and, perhaps, corrected by the experiments made by Professor Sherrington?—It was enormously helped, because it was only by his experiments, which were going on at very much the same time, that we knew that there was anything of this kind—namely, that the central nervous system was distributed in this sort of way. That is the way I put it. Let me be quite clear. Dr. James Mackenzie put forward exactly the same view, backed up by admirable observations on similar cases, but he did not carry it as far, because he was not so familiar with the work on the posterior roots.

17972. That is why I put my question as I did. With regard to the experiment on yourself, do I sum it up rightly by saying that it showed the co-existence of nerves affecting the skin with another series of muscular nerves which conveyed the sensation independently of the skin nerves?—Yes, that is quite right.

17973. Does the knowledge that you so arrived at, in your opinion, directly tend to the amelioration of future suffering by more skilful and expert treatment that that knowledge will enable?—Certainly, it has opened up and enabled us to understand things which we did not understand before.

17974. And it tends, therefore, to correct errors either in diagnosis or treatment, which may have existed in the absence of that knowledge?—Yes.

17975. (*Dr. Gaskell.*) I only wanted to make this matter quite clear if I could. Before your experiments had various surgeons sewn together nerves going to the skin which had been accidentally cut?—Yes.

17976. In some cases had they found that sensation was well localised immediately after the operation, when it was tested?—Yes, that is true.

17977. And in consequence of finding out that recovery of sensation, was the conclusion come to that those nerves had healed remarkably quickly?—Yes.

17978. And that therefore the whole process of re-regeneration as ordinarily taught was wrong, and that the two nerves could heal together by direct contact very quickly?—Yes.

17979. That was shown by your experimentation to be absolutely erroneous, because the outcome of it was that the localisation of the deep nerves was very much more accurate than ever had been before suspected?—Yes.

17980. The only other thing that I should like to know is whether in that regeneration of your nerves going to the skin there was a very great difference in time between the regeneration of those fibres which gave you tactile sensation and those fibres which gave you painful sensation?—Yes, there was a difference of nearly nine months before the tactile sensation began to recover so far as the shaved skin is concerned—so far as the naked skin is concerned.

17981. And you gather from that that those fibres which absorb the sensation of touch are not the same as those which absorb the sensation of pain in the skin?—Yes.

17982. And that the nerves which conduct the painful impressions from the skin regenerate more quickly than those which conduct the tactile sensation?—Yes.

17983. Can you tell me at all where you would say the perception of pain is located in the central nervous system?—It is very difficult to say.

17984. You would not say, for instance, that the perception of pain could be located in the spinal cord?—Certainly not.

17985. Therefore, an animal deprived of the upper part of its brain could feel pain?—No, certainly not. I do not think there is anything to show that.

17986. Do you consider that an animal deprived only of the cerebral hemispheres could feel pain?—I should not like to say. I have not enough experience of that at present.

17987. (*Mr. Tomkinson.*) The result of this experiment on yourself, I understand, was a valuable discovery of something that was not known before?—It was an addition to knowledge.

17988. And that was because you, as a human being with the power of speech, could tell the result?—Yes.

17989. And if that experiment had been made upon an animal the same result could not have been arrived at?—You would not have made it on an animal for

that reason—not on the sensory side of the nervous system.

17990. Then how does that bear upon the utility of the practice of vivisection on animals?—It was given as an instance of how the whole of these various methods are all woven together, and the one depends upon the other. That is the point—that they are all woven together; that the one is useless without the other. They are like the re-agents on a chemist's desk. You give him so many re-agents, and then you take away one of them from him; you say to him: You must do all your analyses, but you must not use one particular re-agent, say hydrochloric acid. There are a number of experiments which he could do without that particular re-agent, but in order that he may do his analyses properly he must have his whole set of re-agents in front of him to use. In the same way, these are all methods which are used; and it so happened that in this small research which I have instanced all the methods were used each in its own particular place.

17991. Still, in this particular instance the experiments might have been made on animals for a long time without the discovery which the experiment made upon yourself did bring to light?—That is why I submitted myself to the experiment, otherwise I should certainly not have done it. It would have been an illegitimate thing to do. *Mr. H. Head, M.A., M.D., F.R.S., F.R.C.P.* 27 Nov. 1907.

17992 (*Chairman.*) Had the experiments on animals been useless, do you mean?—No.

17993. Do you mean that you experimented upon yourself because you had not arrived at a final conclusion from experiments on animals?—No, because it would not be the kind of question to which you would expect an answer in that way. You would not expect an answer to a direct question concerning those finer forms of sensation.

17994. Your answer to Mr. Tomkinson's last question seemed rather to suggest that experiments on animals had been thrown away. I did not understand that to be your intention?—No, not at all.

17995. You had explained that before?—Yes.

Mr. A. D. Waller, M.D., LL.D., F.R.S., called in, and Examined.

17996 (*Chairman.*) You are a Lecturer at the University of London, I believe, and a Doctor of Medicine, a Doctor of Law, and a Fellow of the Royal Society?—Yes. *Mr. A. D. Waller, M.D., LL.D., F.R.S.*

17997. And you have been there a good many years, I think?—I have been five years at the University, but I have been a lecturer at a medical school for 20 years.

17998. You do not propose, I understand, to go into general questions about the value of experiments on animals or the morality of them—we have had a good deal of evidence on those points already, as you know—but principally you desire to vindicate, if I may say so, the University of London from some of the suggestions in Miss Lind-af-Hageby's book, "The Shambles of Science"?—Certainly, and I wish to restrict myself to facts within my own knowledge.

17999. You were present, either as lecturer or otherwise, at a good many of the operations therein described?—Yes, I have drawn up a list of the operations which I could identify.

18000. The first was a lecture on December 3rd, 1902. Dr. Pembrey was lecturing then?—Yes, I was present then.

18001. Then there were lectures on February 3rd, 1903; February 19th, 1903; February 26th, 1903; and another on the same date?—That is a point of doubt in my mind. I have identified the dates from my notes as the occasions on which I was present, but this date February 26th is twice quoted in "The Shambles of Science," and one of those dates must be wrong therefore.

18002. Do you mean to say that there were not two on that date?—There were not two on that date.

18003. However, as regards the one that Miss Hageby describes as "A dog injected with a substance derived from a lunatic," and the other as "A troublesome dog." Those two, whatever the date was, you were present at?—I was present at the lecture, and I can identify the "dog injected with a substance derived from a lunatic." As regards "a troublesome dog" on going through the description, I could only say that I saw nothing of the sort.

18004. Do you mean by that, that you were not present, or that you may have been present, but you did not see what she says took place?—I mean to say that I think it is a mistake. I think probably this is relating to some other place. I do not think that this lecture, so far as I can identify it, took place at the University. I cannot identify it.

18005. You say that it certainly did not take place at the date that Miss Hageby puts it at?—It cannot have done so, because I have the laboratory books here, and another lecture took place on that date, namely, "A dog injected with a substance derived from a lunatic." That lecture did take place on February 26th. In "The Shambles of Science" an experiment headed "A troublesome dog" is described as taking place at that date, but it cannot have done so.

18006. Your evidence about that is that you are confident that it did not take place on that date at the London University?—Quite.

18007. And you have not been able to find any trace which would enable you to identify it?—I cannot identify that lecture.

18008. It may have taken place somewhere else?—Yes, it is probably a mistake on the part of Miss Lind-af-Hageby.

18009. Then the next lecture is on March 12th. "The only completely satisfactory method." That is one of the lectures you were present at?—Yes, I was present at that.

18010. I would like to ask you generally about Miss Hageby's book; you read it at the time, I believe?—Yes.

18011. And you made annotations on your copy?—I annotated it.

18012. Which you are willing to leave with us?—Certainly.

18013. Generally, do you consider that the book gives a fair and accurate representation of what took place?—Oh, no; it is a highly coloured representation; a representation coloured by the sentiments of this lady—in perfect good faith, no doubt. I do not say that they are in bad faith at all, but they are obviously seen through her own coloured spectacles, and the amount of discoloration that those spectacles have produced is really very remarkable.

18014. As regards those lectures that you were present at, would you just tell us as regards December 3rd, 1902 (and I will take you through the others very shortly), what the points are that you picked out as being, in your opinion, mistaken?—I will quickly run through my notes. She states on page 11, "He inserts one point of a pair of forceps under the skin behind the skull for a moment."

18015. Are you quoting from the original edition?—Yes. I have not got the later editions. That means that he cut the head off. Then she says lower down, "The animal begins to struggle, quickly moves its limbs to its back, and tries with hands and feet to thrust away the instrument." My note is, "The ordinary reflex movements that do not imply any sensation."

18016. You were present at that lecture?—Yes, and I made these notes.

18017. Have you a remembrance of the lecture, and what took place?—I have none now, I had at that time. On page 12 she says: "The wound is cauterised." My note is, "Presumably the cord is destroyed." That means pithing.

18018. Would that involve a cautery?—No.

18019. You mean that there was no cauterising?—Yes. Then on page 13 is the episode of the frozen rabbit left in an ice-box, and so forth, and the description of its start with horror on the next page. I have made some comments on that, but it is an impossible thing.

18020. (*Sir Mackenzie Chalmers.*) Would you tell us

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what really happened?—What happened was that the rabbit was put in an ice-chest, a refrigerator, to lower its temperature, but the refrigerator was not cool enough as a matter of fact, and the animal's temperature was not lowered

18021. Was it anæsthetised or unanæsthetised?—It was not anæsthetised. It was simply put into the refrigerator to show that its temperature did not go down as that of a cold-blooded animal would.

18022. Nothing whatever happened?—No. I have this memorandum "Minimum temperature=8°." It was not low enough to act.

19023. 8° Centigrade?—Yes.

18024. What would that be Fahrenheit?—I make out 46°.

18025 (*Chairman.*) But what do you mean by its temperature? Do you mean its internal temperature?—That was the intention, no doubt. The temperature of a cold-blooded animal in a cooling box would go down; the temperature of a warm-blooded animal would not go down, or at any rate not so quickly. But there was no vivisection at all.

18026. I understand that a rabbit's natural temperature is a little higher, if anything, than man's?—About 37° or 38°.

18027. About the same. I do not understand your bringing it down to 45.—That is the temperature of the box, not of the animal.

18028. I was asking what you brought the rabbit down to.—I cannot tell you. I have no idea.

18029. (*Mr. Ram.*) But there was no vivisection?—There was no vivisection; there was no freezing of the animal. That is pure imagination.

18030. (*Sir William Church.*) When she states, "It was found to be beyond the stage for observation," that leads the reader to think that the animal was dead. I understand from you that the animal was not dead?—The description is, "The animal was taken out of the freezing machine quite unconscious, but frozen stiff, like a piece of wood. With all signs of terror the animal springs back trying to get away, but half paralysed by the cold and half fascinated." My comment upon that is simply that all this is "impossible."

18031. (*Sir Mackenzie Chalmers.*) Was the animal never exposed to a lower temperature than 45°?—No; the cooling box did not act properly, as a matter of fact. I remember that much; but the details about the animal I do not remember. I generally remember this description of an animal frozen stiff, and, with all signs of terror, springing back, and think to myself that was an imperfect description.

18032. (*Chairman.*) There must be some imperfection when they say at the same time, "frozen stiff like a piece of wood" and "the animal springs back"?—I have simply got a joking comment of this as being an obvious impossibility. Then my general comment upon this lecture at the time was, "No vivisection was performed at this lecture (at which I was present)."

18033. Do you mean that none was performed at that lecture or that none was performed whilst you were present?—I was present at the whole lecture, and no vivisection was performed while I was present. My comment continues, "One or two frogs were killed, and their hearts used for experiment. The lecture was on animal heat, and some animals hot and cold-blooded were cooled in an ice-chest and their temperatures compared. That experiment was on December 3rd, and that is misdated again in "The Shambles of Science" as December 2nd, but that is simply a mistake of Miss Lind-af-Hageby's. It should have been the 3rd, according to my laboratory book, which I have here.

18034. Then the next is February 3rd, 1903—"An experimental production of blood clotting by injection of a nucleo-proteid"?—I remember that particularly well, because I was there, and I gave the anæsthetic myself, and there is a description on page 30 of the anæsthetist looking "most careful, as if he were attending the precious life of some human patient," and so forth. It simply means this: It was one of the very first lectures delivered in the laboratory, and I laid great stress upon anæsthesia, and gave the anæsthetic myself, in order to, so to speak, show how it was to be done when done by other people in the laboratory. I have got a long comment here, but I do not think it is germane to anything that you wish to know.

18035. You have made a special subject of anæsthetics, have you not?—Yes; I have worked at it a great deal in the last fifteen years.

18036. You have nothing special with regard to that experiment that you wish to say?—There is a statement, "Have these preliminary operations been done under an anæsthetic?" on page 29, the inference being, of course, that they have not. My comment here is, "Yes, I took charge of the anæsthesia."

18037. What was the animal there, a grey rabbit?—I forget the colour of the rabbit, but it was a rabbit, no doubt.

18038. I might ask you now, as regards all these experiments that you were present at, was there any case in which the animal was operated upon either without anæsthetics or without sufficient anæsthetics?—Certainly not.

18039. Are you satisfied that in every case there was complete anæsthesia during the operation?—Certainly, and I state this in my summary. I state that no experiment has been performed in this laboratory in which an animal has not been completely insensitive to pain.

18040. If there were movements in the animal—if that is accurate—it would follow that it was not from sensation of pain?—Certainly not, in my opinion. They were reflex movements; the ordinary convulsive movements that happen.

18041. Is there anything else you wish to say with regard to the experiment on the 3rd of February?—They ask the question again on page 32, "Now, was the rabbit anæsthetised or not?" My comment is "Certainly." I was there. Then they say, "The animal began to struggle violently." My comment is, "The usual anæmic spasms."

18042. (*Mr. Tomkinson.*) Is that a dog?—No; the same animal—a rabbit. Then they go on, "And would not the tetanic spasms that followed counteract the effect of a possible anæsthesia." My comment in pencil is, "Certainly not." That is all I have to say about that.

18043. (*Chairman.*) Then we come to February 19th. "An experiment that is not supposed to be useful." Have you any observations to make upon it?—Yes; I was present at that lecture. On page 59 they say, "The lungs are exposed, and artificial respiration kept up by means of a pump attached to the operation table, and worked by electricity." My addition to that is "pumping air plus chloroform vapour"—that is to say, the artificial respiration apparatus was attached to an apparatus for producing anæsthesia.

18044. Was that the usual method?—That was the usual method.

18045. And it was satisfactorily performed on this occasion?—Yes, it cannot be otherwise with that automatic apparatus, I think. Then, on page 60, "We can be almost certain that the dog has been also curarised, because it is absolutely still, with the stillness that is characteristic of the 'Hellish Woorali.'" My comment as to that is, "It was not."

18046. It was not curarised?—No; it was not curarised. Then I have a further amplification to this effect: "The animal was absolutely motionless under artificial respiration through a chloroform flask giving between 2 and 3 per cent. of chloroform vapour to the insufflated air. I was present." I had been measuring the percentages at about that period.

18047. Would that be sufficient to account for its being completely still?—Absolutely; and it would run the risk of being overdosed with that percentage.

18048. Then there is the lecture on the 26th February: "A dog injected with a substance derived from a lunatic." Dr. Brodie is again the lecturer, as on the 19th?—Yes; that lecture did take place on February 26th, according to the notes in my laboratory book—"Lecture on the Circulation, by Dr. Brodie," and I find Miss Lind-af-Hageby and Miss Schartau's names as being present there, so that I identify that as the experiment of the 26th February; therefore I do not identify the other one.

18049. You say that it was a lecture on the circulation?—A lecture on the circulation, on the effect of choline, which was the particular thing—one of the alkaloids, one of the ptomaines.

18050. She described it as "A dog injected with a

substance derived from a lunatic"?—Yes; that is all right. It is a description, after a fashion, of choline; it is a substance derived from a lunatic. It was in that case.

18051. I think we have had that explained?—Yes. It is stated on page 76: "The demonstration begins. The lecturer tells us that the animal is anæsthetised with morphia and some chloroform." My statement is, "The animal was fully anæsthetised." I have no other remarks at all on that lecture.

18052. I will not ask you to repeat the general observations that you made about the choline that was given.—That applies to the whole.

18053. (*Sir William Church.*) Miss Hageby, when she was giving evidence on this case here, took exception to the way in which the dog was killed. She states in "The Shambles of Science" that the dog was killed "By blowing a little air into the jugular vein," and the first time that was done she said that the levers continued in motion, "Showing that the flame of life is not blown out." "The lecturer declares that the dog will now be killed. How exceptionally kind and considerate! It is to be killed by blowing a little air into the jugular vein." She says that it was not killed when the air was first blown into the vein, and she rather led one to think that in the intervals between the first blowing into the jugular vein and the second the animal was not under the influence of chloroform?—This is in 1903. I have no comment on that, and I cannot remember every detail of what happened to this animal in 1903; but it does not sound to me probable at all. I have this comment written down, that "The animal was fully anæsthetised"; therefore I do not think it probable. This was written down as soon as I got the book.

18054. Might I read the passage to you, because she made a great point of it in her evidence: "It is to be killed by blowing a little air into the jugular vein. The assisting demonstrator attaches a glass tube to the cannula in the jugular vein and blows some air down the tube, but the levers are still in motion, showing that the flame of life is not blown out. He repeats it and then sits down among the audience. The dog has been killed strictly scientifically." She rather implied I think, in her evidence, that there was a considerable interval between those two blowings into the jugular vein by the assistant demonstrator. Can you tell us at all whether that was the case?—I have no recollection of any such interval.

18055. (*Sir Mackenzie Chalmers.*) During which the dog was suffering?—I have no recollection of any interval, and I am absolutely certain that the dog was not suffering.

18056. The dog was fully under anæsthesia the whole time?—The dog was fully under anæsthesia the whole time.

18057. (*Sir William Church.*) That is what I wish to get out?—Yes, certainly.

18058. (*Chairman.*) Have you finished then with February 26th?—Yes, but I am puzzled by this other thing called "A troublesome dog."

18059. I understand you to say that you cannot trace it?—When I got this copy I realised that I had been present on February 26th, and then I set work to try and remember a troublesome dog, and all my comments are that I never saw anything of the kind; and now my comment is that I do not believe that that lecture ever took place at the University of London.

18060. So I understand. What you tell us with regard to that is that you do not believe there ever was such a lecture delivered at the University of London. Will you tell us about the 26th of February?—That is the one we have just finished.

18061. But Miss Hageby also puts this down as the 26th of February. In your laboratory book is there any mention of such a lecture?—No, there would not be. It would only be in my memory, and I do not remember the experiment.

18062. Dr. Brodie is down there as lecturer on the 26th February?—Yes.

18063. Is he down a second time?—No, only once.

18064. And you have given an account of that one?—Yes.

18065. Then there is a mistake obviously somewhere—I mean on Miss Hageby's part; but whether it is a mistake altogether for some other place, you do not know?—I do not mean that there is an invention at all. It is simply a mistake.

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18066. Now, as to March 12th, what do you say? That was a lecture by Dr. Brodie, I think?—Yes.

18067. And the title is "The only completely satisfactory method"?—Miss Hageby begins by saying: "He has doubtless found the remedy to be curare," on page 129. My comment is, "No," no curare was used.

18068. You are quite sure that there was no curare used there?—Absolutely. So far as my memoranda go, the footnote I put down is, "Incorrect. The animal was under artificial respiration through the chloroform flask, getting between 2 and 3 per cent.," in the usual way.

18069. You wrote your notes, you say, about how long after the lecture?—The lecture was in March, and the book was in my hands in July, that is four months.

18070. You were clear at that time that there was no curare used?—I was clear at that time there was no curare used.

18071. Is there anything else that you have a note of on that lecture?—Oh, yes. There is a remark made on page 134 to the effect that at the end of the lecture one other—the director of the laboratories—came to try the action of betain. I remember that even now, because it was a preliminary trial of betain that was made, and if any point has been made of that I will explain it; if not, I will not detain the Commission. This is germane to the inquiry of the Commission, I think, on page 135: "When an anæsthetic or narcotic has been given to the animal the students are told so, or they see the anæsthesia being kept up by repeated doses of the anæsthetising agent." My note is, "Not necessarily," but on this occasion the anæsthetic was certainly given.

18072. What do you mean by not necessarily?—One does not mention it on every occasion. At every turn the animal is anæsthetised, but it may often happen that one does not mention it.

18073. When it has been given, the students, she says, are told so?—Sometimes.

18074. She does not say that the students are told about every fresh dose?—No.

18075. "Or they see the anæsthesia being kept up." They would not necessarily see it?—No.

18076. As regards telling them at the beginning; is it usual to tell them?—I really do not know. I should think it might be or it might not be. I do not see any custom. I often do not say anything about it. I take it for granted. I do not think I should necessarily say so myself.

18077. Is it the invariable practice in these operations at a laboratory to give an anæsthetic?—Certainly. I believe that a lecture that I gave immediately after the publication of this book has been put in the hands of the Commission. The first two pages contain my immediate answers to the allegations in this book, but the University was cognisant of it, and took no action, because action was being taken from elsewhere, from University College, and as a matter of fact, the publication of that lecture was delayed for a week or a fortnight, until after a parallel action on the part of University College was over, on the part of Dr. Bayliss.

18078. We have that pamphlet already, have we?—Yes.

18079. There is only one thing, I think, that I want to ask you. You have told us that no operations have been performed in the laboratory except upon animals that were under complete anæsthesia?—That is so.

18080. And that they were so kept?—Yes.

18081. But some witnesses, and I think Miss Hageby amongst them, have drawn a distinction between light and deep anæsthesia, as if light anæsthesia meant that the animal was subject still to pain and deep anæsthesia meant that it was not. Is that so?—It is a question of definition. My definition of light anæsthesia and deep anæsthesia implies that there is no sensitiveness to pain in light anæsthesia, any more than there is in deep anæsthesia.

18082. That is a point I wanted to know. We have had it explained by other witnesses to us, and I do not want to take you through it at length?—I can do it very shortly, I think. I gave a definition of it a few months ago that I came across. "The finger-post between this first stage and the next is quite clear; if when the conjunctiva is touched the eye winks, the

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anæsthesia is 'light,' if the eye does not wink the anæsthesia is 'deep.'" That is my definition of light and deep anæsthesia, which I gave in the lecture.

18083. The point I wanted to know is, is the use of the word "light" or "deep" any indication whatever of any degree of pain?—None. In light anæsthesia all sensation and voluntary motion are lost. In deep anæsthesia the movements are more profoundly affected; the eye does not wink in reply to a touch on the conjunctiva.

18084. So that the use of the expression "the animal was under light anæsthesia," would not convey to anyone who understood the use of the word in anæsthetics, any suggestion that the animal was suffering pain?—Not to a well-instructed person who understood the use of language.

18085. As the word is used by surgeons or anæsthetists?—Yes.

18086. Is there anything else you wish to state to the Commission?—In answer to Question 7234, Miss Hageby refers to my "Exercises in Practical Physiology," and says that I give a caution in regard to the use an anæsthetics on page 47: "Unless the animal is too deeply anæsthetised, salivation is acceleration. I wish to state that it is not a caution, but an absolute statement of fact. The only implication contained in the statement is that the animal is liable to be too deeply anæsthetised.

18087. (*Sir William Church.*) I should like to ask you one question, not on the subject of "The Shambles of Science," but you have paid great attention to the administration of chloroform to both human beings and animals, have you not?—Much less in the case of human beings, but I make no distinction really.

18087A. We have had some evidence given before us by a witness, who said that the fall in blood pressure which takes place on the administration of chloroform is produced really by asphyxia and by that alone, and not by the chloroform itself. What would be your opinion upon that point?—I should say that chloroform of itself tends to produce a fall, but that asphyxia tends to produce a rise.

18088. I think the witness's view was that you only got a fall in the blood pressure when you got the respiration interfered with; is that so?—You have respiration interfered with and the heart interfered with, and the respiration activity diminishes and the heart activity diminishes, and the vasomotor centre relaxes, and you have a fall of the pressure and diminution all *pari passu*.

18089. You have no hesitation in saying that the action of chloroform does at times diminish the blood pressure?—No, I have no hesitation in saying so.

18090. Quite apart from any connection with interference with the respiration?—Yes, I should say so.

18091. (*Sir William Collins.*) Did the Senate of the University of London pass a resolution in favour of giving evidence before this Commission?—Yes, I think so. A vote was taken.

18092. What witnesses were to appear on behalf of the University?—Dr. Bradford, Sir Edward Busk and myself.

18093. I think you are the only witness appearing on behalf of the University?—I understood that Dr. Bradford was appearing on behalf of the University in respect of the Brown Institution, which is an institution under it, and I am appearing an behalf of the University in respect of the physiological laboratory, which is an institution under it. That is the status, I believe, respectively of Dr. Bradford and myself.

18094. Was a letter written by Sir Arthur Rücker to the Under Secretary of State for the Home Department in reference to statements contained in the work by Miss Hageby and Miss Schartau?—Yes, in reply to a communication from the Home Office.

18095. Would you desire to put it in?—I mentioned it in my *précis*.

18096. Perhaps you might read the last paragraph but one?—"I may add that when the laboratory was first established, and on several occasions since, I have discussed the performance of experiments on living animals with the director, Dr. Waller. I informed him of my personal wish that no such experiments should be used for the purposes of demonstration except those which were necessary for the instruction of senior students; and I found that this was in exact agreement with his own wishes and intentions. He used the phrase that a vivisection should be regarded as a 'solemn thing.' I have every reason to believe that this view is that in accordance with which the laboratory is being worked." That is the paragraph.

18097. May I take it that it is in accordance with that view that the practice in the laboratory is conducted?—It is so.

18098. You have, have you not, at the laboratory a panel of lecturers containing many distinguished names?—Yes.

18099. And many important publications have proceeded from the laboratory?—A considerable number.

18100. Would you care to make any quotation from the lecture which you have handed in dealing with the particular case of the "Shambles of Science"?—I only refer to it in my *précis* as indicating my state of mind towards the general question, and in reply to Miss Lind-af-Hageby and Miss Schartau's statement in the "Shambles of Science." I would like to place before the Commission what I said. "If the two young ladies under whose name the publication entitled 'Shambles of Science' has appeared, came simply to get copy,' the case is easily understood; but I am unwilling to believe this; and, indeed, the case looks to me much more like that of a pair of very ill-advised busybodies supporting each other through a trying ordeal for the sake of what they imagined to be an errand of mercy. I am sorry they came. I should have told them or any other non-professional women that they had better not come to such lectures, for they would run the risk of being misled and pained by experiments where no suffering to living animals was caused. Indeed, one would look very much askance at the young woman who should be able to look on while an animal was, as she imagined, undergoing torture. For the naked details of even a properly conducted vivisection, ignorantly considered, as must be the case if they are considered at all by an unprofessional person, appear revolting—so do the details of a surgical operation or of the slaughter of an animal for food, or of the proceedings in the dissecting room, and the *post-mortem* room. The minute and graphic account of such details by ignorant women is from every point of view mischievous and deplorable, and I very much regret that these two unfortunate ladies should have obtained the opportunity to torment themselves and their readers by their conscientious misrepresentations. Physiologists, when vivisection is necessary, do not take either pleasure or pain in its naked details, and they systematically take due precautions to fully anæsthetise animals required for experiment. The incredible motives sometimes attributed to physiologists by well-meaning people, and the unlimited adjectives and substantives by which they are held up to public reprobation can only be left to the antidote of their own excess. It is really labour lost to be constantly pleading 'not guilty' to this, that, or the other quite outrageous statement; we can only wonder that well-meaning people can so quietly harbour such infamous thoughts. Nor have we failed to question our own conscience. We well know that physiologists are subject to the common laws governing the human mind, and that habit must tend to engender inattention, and that inattention would be cruelty. We are on our guard against our own inattention, but knowing that we are human, we do not resent as indignantly as might be expected of us the reminder to be on our guard that is contained in the denunciations of our critics. We also know that there is evil in the appearance of evil, that things right and proper in themselves, but offensive of appearance ought not to be exposed. And if exposure by even competent authority must offend and injure the imaginations of ordinary men and women, how much more injury and offence to the public mind will be committed by incompetent and self-deluded women who have entered, what is to their minds, nothing more than a chamber of horrors, where in reality the 'quivering flesh' and 'palpitating heart' and 'tortured nerves' are more often than not only the living parts of dead animals, and where, in any case, an animal, even if alive, has been absolutely deprived of sensation. When one attempts to realise what kind of images must hold possession of an ignorant, prejudiced, and sensitive person—man or

woman—who has seen, without understanding, he ceases to wonder at bad language, yet what can he hope to say that shall reach the mind of the deluded fanatics whose pity has been fanned to hatred by agitators. And can it be expected of us that we should say anything at all to persons who can employ sensational literature to poison the wells of human sympathy. In this laboratory—and no doubt in others—our very first concern is to administer anæsthetics properly when anæsthetics are required. And I make the deliberate statement that animals in this laboratory are anæsthetised with as great certainty and accuracy as are the patients in any hospital in the United Kingdom. Can it be necessary to tell you that we do not juggle with bottles of 'colourless and odourless' liquid as imagined and stated by the two ladies who have visited these 'shambles'? You shall see for yourselves what takes place outside the lecture room whenever it is necessary to anæsthetise an animal. I have chosen cats for my demonstration, since the demeanour of these animals is most familiar to us."

18101. May I ask whether the evidence tendered on behalf of the University of London has been submitted to the Senate?—No, I put it to the Physiological Committee.

18102. Is there anything that you desire to add on behalf of the University of London?—No.

18102A. (*Sir Mackenzie Chalmers.*) Where is the laboratory at which these experiments were conducted?—At the Imperial Institute, on the top floor.

18103. Is it specially under your supervision?—Yes.

18104. Is that laboratory only used for demonstration experiments, or are there research experiments carried out there also?—Research experiments are carried out there as well.

18105. Under your supervision?—Yes.

18106. How long have you been engaged in research and demonstration experiments?—At that place or generally do you mean?

18107. Generally?—Twenty-nine years.

18108. And at this laboratory?—Five years.

18109. May I take it from you generally that as regards the experiments witnessed by Miss Hageby and referred to by her, you are satisfied yourself that the animals were properly anæsthetised and kept under anæsthetics until death?—Certainly.

18110. And that any movements which she saw, and such movements may no doubt have been seen, were reflex movements and not conscious movements of pain?—Yes.

18111. I suppose you have a good deal of experience of physiological laboratories?—Yes.

18112. May I ask whether you are a physiologist only or a practising physician also?—I am a physiologist. I do not practise.

18113. But you have medical qualifications?—Yes; in fact, I have practised formerly.

18114. Now you are wholly a physiologist?—Yes.

18115. Then I can ask you two questions that I want to ask. In the first place, we have been told by a witness that when artificial respiration has to be kept up, and for that purpose tracheotomy is performed on the animal, the tracheotomy is performed before any anæsthetic is administered, is that so?—Not in my practice, most certainly.

18116. Are you aware of any laboratory in England in which tracheotomy would be performed on an animal before the administration of some anæsthetic?—I know of none.

18117. That practice has never come before you?—No.

18118. Would you approve or disapprove of it?—I disapprove of it.

18119. So far as you are aware it has not been done, and it would be both wrong and illegal?—Yes, I think so, certainly.

18120. A great many physiologists now, as you know, are not medical men?—There are such physiologists, distinguished physiologists, even.

18121. And some very distinguished physiologists?—Yes.

18122. As regards the licensing of what I was going to call beginners, who are not medical men, is there not a difficulty. In the case of a medical man you have the guarantee of his profession; but in the case or a man who wishes to begin experimenting as a physiologist have you any such guarantee?—But surely he would be vouched for by some person of experience? I should imagine so. I should think it would always be the case.

18123. That is what I wanted to ask?—A young man would not be licensed in a laboratory to work by himself generally. He would be under the supervision of the director or professor of the laboratory if he were a beginner.

18124. Before his application for a licence was signed would there be sufficient opportunity for making inquiry?—Surely; it has to be endorsed by certain professors and authorities. That works all right, I imagine.

18125. You think that there would be no more difficulty in examining his qualifications than in the case of a medical man?—I do not think there would be. I should not make any distinction for that purpose.

18126. Are there any students of your own studying wholly as physiologists and not men going to take medical degrees?—I have, so to speak, no students directly. Everybody in the laboratory of the University of London is more in the nature of a colleague than a student. The students come to lectures, but I simply see them at lectures. That does not make them practical students of mine.

18127. They are not under your tuition in any way?—No, they are not.

18128. (*Mr. Ram.*) With regard to any of these experiments of which you have told us, and at which you were present, was any curare used?—No; there was no curare used to my knowledge at all right through.

18129. Have you had experience of the use of curare?—Yes.

18130. We have been told that, according to the belief of some witnesses, it is impossible to sufficiently anæsthetise an animal under curare. What do you say to that?—I have no experience of curare in combination with an anæsthetic. But are you asking me whether an animal immobilised by curare could be anæsthetised?

18131. Yes?—It could be.

18132. Have you ever known curare administered without anæsthetics?—Never.

18133. By an anæsthetic we mean, of course, one producing complete insensibility to pain?—Certainly.

18134. (*Chairman.*) I think the question asked was whether, if you gave an anæsthetic to an animal which had been curarised you could have an assurance that it really was under the influence of the anæsthetic?—Certainly.

18135. You could always ascertain and know whether you had it completely anæsthetised or not?—It is immobile, so is a plant immobile, but I know if you give it 2 per cent of chloroform vapour it is of necessity under the influence of the chloroform vapour.

18136. At any rate, you think that you could always satisfy yourself if you were giving an anæsthetic to an animal under curare that it was completely under the influence of the anæsthetic?—I should be satisfied. If artificial respiration is going on with the percentage of chloroform vapour, I should be satisfied.

18137. That is what would satisfy you?—Yes.

18138. (*Dr. Gaskell.*) Because you know that that percentage of chloroform vapour would always keep an animal of that size completely under the anæsthetic?—Yes, and I know that I am on the safe side as regards pain to the animal, and on the unsafe side as regards the life of the animal. I am more liable to lose the animal than to let it out of anæsthesia.

18139. May I take just a question with regard to this attack of Miss Hageby with regard to the dog injected with the substance derived from a lunatic, because she says in answer to Question 7507 "The method of killing is really what is mostly criticised in this chapter besides the nature of the experiment—that the assisting demonstrator did not even take the trouble to see that the animal was really dead," and then she goes on to say in reply to Question 7509: "The demonstrator attached a glass tube to the open jugular vein, and first we were told that the animal was to be killed. Then he blew a little air into the jugular

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vein and then sat down, so far as I remember, among the audience, and the lecture went on and the animal was supposed to be dead; but after a minute or two the dog began to struggle and to show signs that it was not dead, and for some time nobody observed it." "(*Sir William Church.*) In what way did it show signs that it was not dead? (A) By struggling. 'It has begun to struggle and seems to be in great pain, at last the lecturer happened to look at the "dead" dog. He says something to the assistant demonstrator, who leisurely rises from his seat, and strolls up to the animal.'" Do you remember anything of all that?—I do not remember the details. They do not sound to me very likely, but what I should imagine might be the explanation of what she saw, or thought she saw, would be that with air in the veins convulsions happened, and in order to make sure, as any person would do, as I should do under such circumstances, the lecturer said "Kill the animal," meaning simply to put the scalpel into the medulla. That would be one's natural action if one saw a convulsion on the part of an animal that had been so treated. One would oneself put a scalpel—I would have done it myself—into the medulla.

18140. Is it not a common thing by that method of blowing air into the jugular vein for struggling to take place?—Yes, it is common, of course, to have struggling take place when the air is in the vascular system.

18141. It is of the same nature as the struggles of asphyxia?—Or anæmia.

18142. (*Mr. Ram.*) There is no sensibility to pain?—Absolutely none. There is just one point which occurs to me which I should wish to mention about the marmot, as I have seen all about this marmot, and an extremely interesting point it is. It was a marmot coming out of its winter's sleep, and coming out of its winter's sleep the front half of the marmot gains its temperature in about one hour before the back part of the marmot, and that marmot came out of its sleep, and was fully awake as to the front part of its body and still asleep as regards the hinder part of its body, and bit the demonstrator.

18143. (*Chairman.*) That is at the time of the lecture?—At the time of the demonstration. I have seen a great deal about it in the evidence before the Commission, and it puzzles people that an animal—it puzzled Miss Lind-af-Hageby quite honestly no doubt—should normally for a time be half-paralysed; but, as a matter of fact, it is an extremely interesting phenomenon. If you study the temperature curve, given by those who have studied it, there is an interval of about an hour while the animal resembles a paraplegic animal completely as to its hind legs just as if it were paralysed, whereas the front end was all alive—so much so that it bit the hands of the demonstrator, a pretty good bite.

18144. (*Dr. Gaskell.*) With regard to the question of light anæsthesia, I should like you as an anæsthetist to tell the Commission how you can tell when a sufficient amount of an anæsthetic has been given, so as to prevent the animal from feeling pain?—One's ordinary quick and practical test is the conjunctival reflex. If the eye winks, more anæsthetic is wanted; if the eye does not wink, it is all right.

18145. You have just told us that that is evidence rather of a deep condition of anæsthesia, but when it is in light anæsthesia, how do you know it? We have been told that there is a very distinct difference between movements that are purposeful and movements which are in the nature of reflex movements?—I should have difficulty in giving you a sharp test while the eye reflexes are present. I suppose one would go by the character of the movement, but then that is not a sharp difference. The differences between the reflex and automatic movements and the convulsions are often very deceptive as indicative of conscious voluntary impulses; but still one would tell by the behaviour of the animal whether the movements were indicative of sensation or not, simply from their general character.

18146. From their general character you think you could tell. As an individual who has given anæsthetics to animals considerably, would you feel any doubt as to being able to tell. One allegation brought here against physiologists is that the physiologist keeps the animal under light anæsthesia, and that really is not true anæsthesia at all. The question has been asked again and again what is meant by light anæsthesia?—In the state of light anæsthesia, one knows that the first movements in the case of a man to disappear are the purposive movements, the obviously purposive movement, and one knows that when that happens there is no sensation; and one judges from the character of the movements that the animal is in an analogous state, having no sensation. Only, as a matter of fact, my practice, and I think the practice of most physiologists, is to go as far as the conjunctival reflex.

18147. Is it not the surgical evidence, and therefore one may say also with respect to animals that when you have once obtained complete abolition of the conjunctival reflex a very considerable time elapses, in which no chloroform is given, before any sensation of pain ever comes back?—Yes.

18148. So that there you have a very considerable time after that conjunctival reflex has been abolished, in which you may be perfectly certain that there is no sensation of pain?—Yes; and I will go further than that. You know that you can keep your animal anæsthetised; that if you have brought it down by 2 per cent. you can keep it there at 1½ per cent., 1 per cent., and a ½ per cent, as time goes on. I have had an animal as long as 12 hours under anæsthetics, and at that time it wants only a very low percentage indeed to keep it under.

18148A. But the practice of all physiologists is to anæsthetise the animal absolutely fully first, so that all conjunctival reflex disappears?—Yes.

18149. And then for a long-continued operation afterwards, smaller doses of chloroform are given, but still you feel confident that the animal is not feeling pain?—Certainly.

18150. (*Mr. Tomkinson.*) Do you consider morphia an anæsthetic?—I should not call it an anæsthetic myself. I should call it a narcotic. It is a question of words again.

18151. Do you consider that the state of stupor arising from morphia or alcohol is a state of anæsthesia?—Not in the ordinary way, but I can imagine the stupor being so profound as to amount to anæsthesia.

18152. You said, I think, that you had no difficulty in keeping animals for a very long period under anæsthesia?—No, none at all.

18153. Dogs?—I have kept dogs under anæsthesia for a considerable time, but my longest periods were with cats. I have kept cats under for a very long time, but dogs I have kept under for three or four hours.

18154. In perfect anæsthesia?—Yes.

18155. You have found no difficulty in anæsthetising dogs?—I have not found any. I have not lost a dog for several years.

18156. The boundary line between insensibility and death is not such a very perilously narrow one?—Yes, it is fairly perilously narrow, but if one administers the anæsthetic strictly quantitatively one can reduce the danger very materially.

18157. But whether the risk of death is run or not, you have no difficulty at all events in keeping the dog absolutely in a state of anæsthesia?—No.

18158. And immunity from pain?—Quite so.

18159. (*Sir Mackenzie Chalmers.*) I want to call your attention to Mrs. Cook's evidence on page 70 of the First Report, Questions 1947-52. What she said was: "I have witnessed some experiments at the Imperial Institute, but only baking and freezing." I asked her "On animals?—(A.) Yes, living animals. (*Sir William Collins.*) Were they cruel experiments in your opinion?—(A.) Certainly.—(Q.) Was there any anæsthetic used in those cases?—(A.) Yes, in one; and it was so difficult to give the anæsthetic successfully that the experiment was given up." Then later on she was asked who the experimenter was, and if you look at Question 2027, she said when "Dr. Waller works there." Have you any explanation that you would like to offer the Commission as to the kind of experiments: can you tell us what they were?—I believe it is the case of the stiff rabbit; it sounds to me like it. My attention is called for the first time to this evidence at this moment, but as I see it, it sounds to me like Mrs. Cook's description of the "frozen rabbit."

18160. (*Chairman.*) Was Mrs. Cook at that lecture?—I have the name of Cook occurring in my attendance book. I do not know what her initials may have

been. I will look it up. I forget which lecture the "frozen rabbit" was.*

18161. She said she thought it was 1903?—Yes, but I could identify it.

18162. (*Sir Mackenzie Chalmers.*) Perhaps you can tell us this. She was asked at Question 2028, "What animals were used, do you remember?" and she said, "I saw a black cat used, and I saw two cats used and a rabbit"?—I remember the black cat very distinctly; I think she is correct there. I have no doubt it was this experiment.

18163. Can you tell us what the nature of the experiments was; was there any pain involved?—There cannot have been. But I do not identify her description yet. First I jumped to the conclusion that it was the stiff rabbit that she was describing, the freezing box and baking looks like that; an ice-chest you may say is a baking machine if you like to think so.

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18164. Was any painful experiment performed?—Oh, no, none was performed at any time, of course.

18165. There was no experiment that would expose the animal to such a rise of temperature or such a fall of temperature as to cause real pain?—No heating experiments are ever made in the laboratory. The only ones ever made were on reducing the temperature.

18166. The baking is imagination?—Pure imagination absolutely, and the freezing was never there. I notice the temperature of the ice chest at the time is noted as 8° Centigrade.

18167. And no animal was exposed to a temperature below the freezing of Fahrenheit?—No, certainly not.

FORTIETH DAY.

Tuesday, 3rd December 1907.

PRESENT:

The Right Hon. The Viscount Selby (*Chairman*).

Colonel The Right Hon. A. M. Lockwood, C.V.O., M.P.
Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir J. McFadyean, M.B.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. G. Wilson, M.D., LL.D.

Captain C. Bigham, C.M.G. (*Secretary*).

Mr. J. Lorrain Smith, M.A., M.D., called in; and Examined.

18168. (*Chairman.*) You are a Doctor of Medicine?—I am.

18169. What position do you hold in the University of Manchester?—Professor of Pathology.

18170. You have been requested by the University, I believe, to give evidence on their behalf?—I have.

18171. I gather from your *précis* that they have passed a resolution or given you some sort of express mandate as to what they wish you to state on behalf of the University?—That is so.

18172. But, besides representing the University, I believe you have also been requested by the Pathological Society of Great Britain and Ireland to give evidence?—Yes.

18173. Then, perhaps, as regards the University, we had better take their expressed view, and we will examine you more generally as representing the Pathological Society?—If you please.

18174. I think you have in writing the views of the University?—Yes. The University is pleased to comply with the invitation of the Commission to give evidence regarding the experiments carried on in several of its departments. The laboratories of Physiology, Pathology, Pharmacology, and Public Health are registered for experiments on animals. Apart from original research, the experiments necessary for the public health investigations required by the Sanitary Authorities for Manchester and the surrounding districts are carried out in the Public Health laboratories of the University. The work carried on in the Public Health laboratories is highly valued by a large number of local authorities, representing about five millions of people, and Officers of Public Health find the reports issued from them of great value as regards the detection and prevention of the spread of disease. Some of the hospitals also avail themselves of these reports, which are often very helpful in diagnosis. The University authorities have endeavoured to comply with the regulations imposed by the Act to the best of their ability. The laboratories are visited by the Inspector several times each year, without previous warning, and the fullest information is given to him regarding the researches which are being carried on. The Inspector on his part thoroughly investigates and examines all the animals under experiment. Any representation which he may make regarding accommodation for animals, etc., is carefully considered, and, so far as can be gathered, has been hitherto dealt with to his satisfaction. The University authorities consider that adequate care is taken to further those objects which the Act was framed to accomplish, and that the present legislative provision for controlling this type of investigation is sufficient. The University has never had reason to find fault with any of the licensees for the manner in which the experiments have been carried out. In view of the care which has been exercised in conducting this type of work in the departments of the University and of the thoroughness of the administrative control by the Home Office, the University considers that further restrictive measures are unnecessary and undesirable.

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18175. You say, apart from original research, experiments necessary for the Public Health investigations and so forth. Are those experiments on animals?—Yes, for diagnosis in the Public Health laboratory.

18176. Does that include experiments by way of cutting operations, and also by way of inoculation?—It is practically all inoculation.

18177. In your laboratory, then, the experiments which are necessary for the Public Health Act are practically all inoculations, we may take it?—Yes, that is in the Public Health laboratory. The Professor of Public Health undertakes that work.

18178. When you say the Physiological laboratory, I thought it was the laboratory of the University of Manchester?—The University has a laboratory of Pathology and of Public Health; they are separate.

18179. But are they both under you?—No; the Public Health laboratory is under my colleague, Professor Delêpine.

18180. I was going to ask you whether the local authorities that you speak of lower down have laboratories of their own, because I understand the Corporation of Manchester has one in the University?—They arrange with the University laboratory that this type of work shall be carried out there.

18181. But have any other local authorities there a laboratory; has Salford, for example?—No, it has not a laboratory.

18182. (*Sir Mackenzie Chalmers.*) All the local authorities make use of your laboratory?—Yes.

18183. Do they contribute to the expense; do you charge so much for an investigation?—Yes, there is a scale of charge.

18184. There is a scale of charge for a particular investigation?—Yes.

18185. Could you give us one or two instances of the sort of investigation that is useful?—Diphtheria diagnosis and tuberculosis diagnosis.

18186. Will you tell us how the diphtheria diagnosis is done?—I have given you instances of the vivisection part of it.

18187. Will you tell us how animal experiment is used by the local authority, if it is used?—As a rule diph

* *The witness subsequently wrote: The name M. Cook occurs in the attendance-book under November 19th, 1902 (Lecture on Animal Heat, by Dr. M. S. Pembrey).—A. D. W*

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theria diagnosis is carried out simply by culture, but in a doubtful case you would inoculate a guinea-pig.

18188. Is there anything else?—Tubercle diagnosis is carried out in cases of milk which are suspected of being tuberculous, or in excretions from the human subject which require diagnosis if it is doubtful. They may be inoculated into an animal.

18189. What number of animals, for instance, are used for public health purposes?—Over 2,000 animals a year are used for this purpose.

18190. For sanitary authorities?—Yes.

18191. (*Colonel Lockwood.*) Do your experiments include inoculations in the eye?—Not for diagnostic purposes.

18192. But for any other purposes?—They might use the eye for investigation.

18193. (*Chairman.*) In the Public Health department do you mean?—Yes, if they happen to be doing an investigation on that subject.

18194. (*Mr. Ram.*) In research?—In research.

18195. (*Sir John McFadyean.*) Have you ever known a case?—No, it would only be for a specific purpose.

18196. (*Sir William Collins.*) In what proportion of cases of diagnosis of diphtheria do you find it necessary to resort to other than cultural or microscopic?—A very small proportion.

18197. Less than one per cent., would you say?—I could not say off-hand how many there would be.

18198. Are you able to speak with certainty as the result of inoculation experiments?—With practical certainty.

18199. What are the results in the animal which you regard as satisfactory in cases where the diagnosis remains doubtful as the result of cultural or microscopic test?—You get the evidence of pseudo-membranous inflammation.

18200. You get the false membrane in animals?—Yes.

18201. What animal do you use?—The guinea-pig.

18202. And in cases of tuberculous diagnosis do you find it necessary for Public Health purposes to resort to inoculation?—Yes.

18203. Is that because you are not satisfied with the finding of the bacillus?—Because you fail to find it in doubtful cases.

18204. Then in cases where you fail to find the bacillus are you able to establish the diagnosis as the result of inoculation with material on the animal?—Yes.

18205. Are there any other cases besides tuberculosis and diphtheria in which for Public Health purposes you find it necessary to resort to inoculation of animals?—Those are the chief ones; and possibly in anthrax, but that is a comparatively uncommon condition.

18206. (*Mr. Ram.*) And tetanus?—You might in tetanus.

18207. (*Sir John McFadyean.*) And meat poisoning?—Yes, meat poisoning; but these are things one comes across very occasionally.

18208. (*Sir William Collins.*) In the course of the past year have you had occasion to resort to inoculation of animals for Public Health purposes for other than the diagnosis of tubercle and diphtheria?—It is not my department, of course; I could not answer it without reference. I can add the answer afterwards by consulting Professor Delepine.

18209. (*Sir Mackenzie Chalmers.*) The animals used are guinea-pigs and rabbits chiefly for Public Health purposes?—Yes.

18210. (*Dr. Wilson.*) In injecting into a guinea-pig to test the diphtheria culture would you inject it into the peritoneum?—You might inoculate the throat.

18211. Is that often done?—No, not very often.

18212. Then where is the false membrane found; only when the inoculation is into the throat?—Yes.

18213. You would not expect to find it in the peritoneum?—You would get an inflammation that would not correspond closely to the mucous membrane inflammation.

18214. And what experiment do you carry out in your own department?—Investigation chiefly; I might have a diagnosis, but it is very rarely that that is asked for.

18215. You have conducted a good deal of original research, have you not, at Belfast?—Yes.

18216. And amongst those original researches, researches into conjunctivitis?—Yes.

18217. Those were inoculation, I suppose?—Yes, practically.

18218. Would any anæsthetic be used for the inoculation?—It was self-inoculation really; it was not inoculation in the sense of taking a definite bacillus or streptococcus of a known virulence; it was rather an accidental inoculation which might arise from merely wounding the surface.

18219. Introducing a foreign body into the eye?—In a wound of that sort.

18220. Would not those experiments be attended by a considerable amount of pain?—So far as I could observe there was no great pain.

18221. Did the eyeball slough?—No, never—only a little conjunctivitis. The aim of this set of experiments to which you refer was to avoid severe effects on the part.

18222. (*Colonel Lockwood.*) What happened to the animal after the experiment?—To which animal do you refer?

18223. This animal that you are referring to that was operated on in the eye. Was it destroyed?—Yes. The eye was examined.

18224. How long afterwards was it destroyed?—It was kept for a period, I forget exactly how long—I think a month or two afterwards. Then they were examined microscopically to see if you could discover any trace of inflammation or any degeneration, or even the presence of microbes.

18225. (*Mr. Ram.*) Examined after autopsy?—Yes, but it was all negative; we discovered nothing really by that method.

18226. (*Chairman.*) Is conjunctivitis very painful?—It may become painful, and in severe cases it is painful; but very frequently it is not painful.

18227. (*Sir William Church.*) With regard to Public Health work, could you tell us whether, in the event of suspected ptomaine poisoning, there is any other method by which a result can be arrived at so sure as that of animal experimentation?—No.

18228. (*Chairman.*) You have told us that you also propose to give evidence to us on behalf of the Pathological Society of Great Britain and Ireland?—Yes.

18229. Who are the members of that society?—The pathologists of the United Kingdom.

18230. That is to say, they are all of them educated pathologists?—There is a membership of about 200, and that, of course, includes physiologists and some anatomists, and also a good many physicians and surgeons who are interested in pathology.

18231. But they are what you might call professional men, either professional surgeons, or physicians, or physiologists, or pathologists who have not taken a degree?—Yes, I think you might say that almost without exception. There might be a chemist elected as a member; I am not quite sure. The basis of election is that they shall have done work which bears definitely on pathology.

18232. And there are about 200 members, you say?—Yes.

18233. What is the object of the Society?—To promote the study of pathology.

18234. That means the study of disease, I suppose?—Yes.

18235. Would you tell us a little more in detail how they follow it out?—The first aim of pathological investigation is to describe the structural and other changes which may be observed in the bodies of those who have died from disease. The tissues of the diseased organs are compared with those of normal organs, and it is found that certain changes are the result of the disease. It then becomes possible to explain how, with the occurrence of these changes, there arise the symptoms which the patient showed during life. Pathology has therefore, with the sciences of medicine or surgery, the common purpose of obtaining a knowledge of disease. It attacks the problem, however, by the exact methods

of the physical and biological sciences. The method of investigation by a study of the dead body has been a most fruitful one, especially since Virchow introduced the doctrine of cellular pathology as the basis of investigation, and the working out of this doctrine has been the main occupation of pathologists. For example, the process of inflammation has been carefully studied in order to discover the structural changes which it involves, and by this means much knowledge has been obtained regarding the part played by various kinds of cells. Similarly the processes of degeneration, in which the chemistry of the cell is disturbed, have been the subject of prolonged and careful inquiry, and in the main the observations have been made on the dead body. Till quite recently malignant growth was almost altogether investigated by the study of the structure of tumours obtained from the dead body, or by means of those removed at surgical operations. However valuable the study of pathology on these lines has been, it must be admitted that the method has clearly marked limitations. The tissues which are taken for investigation are no longer living, and therefore our observations are not direct. The evidence we gather concerns what has taken place in the past, but the actual occurrence we do not observe. In the next place a difficulty arises from the fact that we are observing the terminal conditions, and we are left to guess at the nature of the early changes. These conditions may be not only terminal, but also of a secondary nature, and it is of fundamental importance in pathology to distinguish the primary and secondary changes. The primary changes are those which occur when the process of the disease commences, and are directly due to the cause of the disease. It often happens, however, that when once the balance of the tissues is destroyed in the early stages, it is never restored, and in place of normally arranged tissues adapted to each other, and supplying the conditions whereby nutrition is maintained, we may have a progressive disturbance set up which ends only when it has brought about the death of the patient. In this stage of the development of disease the secondary changes may play the chief part. One element of a complex tissue may, for example, be stimulated to overgrowth by an irritant, while the other tissues of the organ are merely damaged by it. As the overgrowth continues, the other elements of the tissue are destroyed. Now since we are studying disease with a view to arrest its progress, it is of the first importance to gain knowledge of the earlier changes. It is during this stage that the opportunity of recovery is afforded. When secondary changes are in progress it is not recovery only, but regeneration, that is required. Recovery may be, and often is, complete, and health is restored, but regeneration is always imperfect. In order to make direct observations in the early stages of disease, we must have recourse to experimental methods. The study of the process by observations on the dead body may afford us certain hypotheses as to the laws governing the phenomena of disease during life, but when we desire to test the truth of these hypotheses we can appeal only to the experimental method. We must reproduce the phenomena and determine the truth of our interpretation of them. The nature of my argument will be most easily seen if I adduce two or three examples as illustrations. I am purposely leaving out of the argument all reference to the bacterial etiology of acute diseases, or to the treatment by bacterial vaccines or antisera. This side of the experimental work in pathology has already been dealt with by Dr. Martin and others. The Pathological Society, in asking me to give evidence before the Commission, desired me to deal with general pathological processes and diseases other than acute infectious conditions, and I have chosen my illustrations with this in view.

18236. This is as to the importance of examining the disease at all its stages in the living body?—Yes. and the fact that the point arrives in the investigation of disease when progress is practically stayed unless one has recourse to experimental methods. Arterio sclerosis is a disease of the walls of arteries of insidious origin, commencing usually at about middle life and gradually progressing until it becomes directly or indirectly a cause of death. It is an extremely important form of disease not only because of its grave effects but because it is so common. It leads to hæmorrhage and softening of the brain, to various forms of heart disease and to a slowly developing form of Bright's Disease of the kidneys. So noteworthy is this condition that it has been said with a rough approximation to the truth, that a man's life lasts as long as his arteries are sound. The cause of this condition is exceedingly obscure. It develops slowly in the course of years and it is therefore difficult to single out one definite factor which can be described as the essential cause. There has been endless investigation of it as it is seen in the dead body. Its various forms have been carefully described, but we do not know its cause, and to gain this information we require to have in our possession some agent which will set up the process. My argument is that this point is reached in all investigations of disease. The means of research which are known have been applied to the problem so far as it can be solved by the study of the dead body, and if we are then possessed of the requisite knowledge we have recourse to the experimental method. Josué made the important discovery in 1903 that repeated injections of adrenalin into the circulation of rabbits set up a form of arterio-sclerosis in the walls of the vessels. This discovery has already led various pathologists to take up the question, and the first paper on the subject from a laboratory in England was published in the Journal of Pathology last October.

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18237. (*Mr. Ram.*) October of this year?—Yes. The value of adrenalin for physiology has been explained, I observe, already, and also its value for surgery in arresting hæmorrhage. It promises to be of still greater value, since it may enable us to discover the cause of arterio-sclerosis.

18238. (*Chairman.*) That is, in larger doses than you would administer for the purpose of stopping hæmorrhage, I suppose?—Yes. You apply it locally to stop hæmorrhage on the oozing surfaces. The next illustration which I wish to describe is taken from the pathology of respiratory diseases. Pneumonia is one of the most common of acute diseases, and it may be studied in two ways:—(1) The bacteria which cause it, and (2) the conditions of the patient which render him liable to become infected. The second only concerns me now. We carry about with us in our mouths and elsewhere the bacteria which cause pneumonia.

18239. You mean that we carry them about in a healthy condition?—Yes. Hence the question before the pathologist is not merely to explain where the infection comes from, but the conditions of the patient which permit the pneumococcus to spread from its usual haunt in the mouth and throat and find its way to the lungs, where it sets up inflammation. In order to work out these conditions, one point to which to direct attention is the lungs themselves. Can we find an agent which in itself may be used to cause pneumonia without the aid of bacteria? Such an agent has been recently discovered by the experimental method. It has been found that to submit an animal to an atmosphere of oxygen will, ere long, bring on pneumonia.

18240. What do you mean by an atmosphere of oxygen—pure oxygen?—Yes, pure oxygen.

18241. Can an animal live on pure oxygen?—Yes.

18242. Without any of the other gases that make atmospheric air?—Yes. The simplest possible way therefore of working out these conditions so far as the lung itself is concerned is to place the animal in an atmosphere richer in oxygen than the air we normally breathe. Pneumonia has been very fully studied by the methods of morbid anatomy, and the data which have been obtained are of great value in enabling us to understand the cause of the disease but we have yet much to learn regarding the activity of the lung and its relation to the onset of inflammation. It is at this stage of the problem that experimental investigation becomes necessary in order to test the truth of hypotheses suggested by the data which we have obtained. The third illustration I have taken from a study of derangements in metabolism. The commonest and most important of these is fatty degeneration. When the cells of a tissue are damaged by the action of poisons they very commonly show in their substance the accumulation of globules of fat. Such a change is seen, for example, in cases of poisoning by arsenic; but this disturbance may be due to a large variety of causes. It occurs more or less universally in gland cells, and its importance may be

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gathered from the fact that in the muscular fibres of the heart wall it forms one of the most serious types of heart disease. In regard to this condition a great amount of information has been obtained by means of starvation experiments. It was thought that fat appeared in the cells, say, of the liver in such conditions from dilapidation of the cell substances, but by means of these experiments evidence has been obtained to show that the presence of fat is in many cases to be explained by the absorption by these cells of fat which has been transported by the blood from the tissues under the skin and elsewhere which normally contain fat.

18243. Would you tell us exactly what you mean by starvation experiments?—The starvation experiments which have been used in this case are those where the animal is deprived of food.

18244. What sort of animals?—Dogs and birds, in order to study the kind of fat which is found in the process of degeneration.

18245. What is the process of the experiment; do you starve the animal until it dies?—No.

18246. Or do you take off one or more of its meals as it were?—You deprive it of food until its body contains little or no fat.

18247. (*Mr. Ram.*) Are they kept absolutely without food?—Yes.

18248. With water only?—Yes, with water. You want to deprive them of fat.

18249. (*Chairman.*) You give them no food?—We give them no food.

18250. For how long, for example?—For a week or two, or even for a month, I have seen some experiments recorded.

18251. Will a rabbit or fowl live for a month with only water?—Yes, and dogs also.

18252. Will they live as long as a month without anything but water?—Yes.

18253. At the end of that time will they recover if they are fed carefully?—Yes, they are fed up again.

18254. Do they show any symptoms of pain?—No.

18255. They are unhappy, I presume?—Yes, they are unhappy; there are no particular symptoms. In fact, there is an experiment on record where the milk of a bitch was studied after a period of starvation in this way.

18256. You mean that it had puppies?—Yes.

18257. Was it able to suckle them?—Yes; it was a study of the kind of fat that appeared in the milk.

18258. Do you usually recover animals after a period of starvation?—Yes, for this experiment certainly.

18259.—For this experiment you are speaking of?—Yes.

18260. Your object would not be to kill the animal?—No, they are fed with fat of other animals which is of a different character.

18261. They are fed with fat?—After they have been starved of their own fat. Then you can follow the relation of this abnormal fat to other tissue changes.

18262. That is part of the experiment—feeding them up again with fat from other animals?—Yes, by that means a great deal of light has been thrown on the process of fatty degeneration. Another important branch of this investigation is exemplified by nervous disease. It has been for years a well established fact that one of the earliest changes in nervous disease is a breaking down of the nervous tissue system into fat, which is absorbed by lymphatics and disappears. These changes are found in the dead body in damaged nerves in the spinal cord and brain, and the investigation of them has been carried out by the methods of morbid anatomy with great energy and success. Since it has been discovered that changes of this nature are found in the spinal cord associated with the occurrence of local inflammation elsewhere, as for example, the presence of pus in the pleural cavity, this form of tissue disintegration has been taken up experimentally, and it has been shown that slow absorption of the poisons generated by bacteria takes place along the nerve sheath, and that these poisons on reaching the spinal cord set up fatty changes in the tissue of the spinal cord at the point where the nerve branches off. In other words, we have obtained a new means of attacking the obscure problem of the causation of such diseases as General Paralysis, Locomotor Ataxia and Recurrent Insanity.

18263. Do you mean to say in detecting the approach of the disease?—Yes.

18264. And stopping it?—And ultimately stopping it.

18265. Have you been successful with animals in stopping cases which you believe you saw?—It is rather early to say that, because the research is just in process. I have purposely taken researches which I know are going on at the present time for most of my illustrations.

18266. However, that is the object of the research?—Yes.

18267. And you consider that there is a reasonable prospect of success?—Yes, a very reasonable prospect. Finally, I wish to refer to a disease regarding which we have as yet no knowledge of the causation, rheumatoid arthritis and osteo arthritis. This is a distressing joint disease or group of diseases regarding which a large amount of work has been done, but so far it has been confined to clinical observation of the conditions and a description of the pathological changes in the structure of the joints affected. Quite recently a hospital has been opened in Cambridge for the study of diseases of this type. What in this case is required above all things is the means of experimental investigation of the disease. We do not know the cause, and our resources for arresting the disease are correspondingly small.

18268. Have animals that same disease—can it be created in animals?—Something very similar to it is found in them.

18269. At any rate they are not naturally subject to it, are they?—I could not answer that.

18270. It is not an ordinary disease?—It is merely an illustration, a disease which illustrates my argument that the research is arrested in a sense.

18271. I only wanted to see how far the research was carried on by means of living animals?—So far we have not been able to reproduce it.

18272. Are your researches on this particular group of diseases carried on at all at present by means of experiments on animals?—So far as I know they have not—at least not with any success. The idea of explaining it by bacteria of course suggests that various experiments have been carried out in that way, but there is no definite teaching as yet on this point.

18273. (*Mr. Ram.*) Is it contemplated in this school that is founded now for the investigation of this disease that animals will be employed?—Ultimately they will go on to experimental work. Meanwhile they have merely the hospital for observing and publishing accounts of cases. One volume of reports has been printed.

18274. (*Chairman.*) Are these curable diseases?—No.

18275. (*Colonel Lockwood.*) Have you any objection to the present restrictions placed by law upon operations on living animals?—No.

18276. Do you think that it has placed Englishmen at a disadvantage as compared with other nations?—I do not know that it has very seriously, anyhow.

18277. Can you suggest, or would you have any objection to, any further restrictions as regards, for instance, publicity of the names of those holding licences or a restriction of the number of licences?—You mean to make it more difficult to get a licence?

18278. I will not say more difficult, but to make more public the names of those who hold licences. I will put it in this way: should you have any objection to further restrictions upon the issue of licences?—I think I should be expressing the feeling of pathologists that further restrictions would delay research, and so far they would object to them, I think.

18279. Have you yourself seen many experiments upon living animals?—Yes, a fair number.

18280. Have you ever seen any cruelty connected with those experiments—what I roughly call cruelty?—Certainly not

18281. Have the animals always been under anæsthetics in all the experiments that you have witnessed—not inoculations, cutting operations?—In cutting experiments, yes.

18282. The animal was completely anæsthetised?—Yes, sufficiently anæsthetised.

18283. And the Act always strictly carried out?—So far as I have observed.

18284. As regards the starvation experiments, have you witnessed many of those?—None at all.

18285. Then what were those experiments that you alluded to as regards fat—you said with dogs and fowls—They were not my own experiments.

18286. Nor did you witness them?—No.

18287. Were they carried on in England?—No.

18288. (*Sir William Collins.*) I did not quite gather the point that you were pressing upon the Commission as regards investigations in connection with arterio-sclerosis. Would you be so good as to explain that?—That the investigations into the causation of arterio-sclerosis by the methods of morbid anatomy had practically come to a standstill until this observation of Josué.

18289. Had Gull's and Sutton's work been of value?—Yes.

18290. What have we learnt in addition to that by experiments on animals?—We have learnt that we have got a substance that will cause arterial sclerosis.

18291. Is there anything besides that that we have learnt as the result of experiments on animals on the subject of arterial sclerosis?—They are working out the effect of this substance.

18292. But in addition to the fact that you state that adrenalin is found to be productive of arterial sclerosis, have we added to our knowledge of the subject by experiments on animals?—So far as Josué's experiments are valid we have added to our knowledge.

18293. Does that involve anything more than the statements that you have made that adrenalin is productive of that degeneration?—That gives the means of studying the development of the condition. It is quite a new investigation.

18294. Has it given us the means of preventing it?—Not yet; it is quite a new investigation.

18295. Is that your own?—No.

18296. Did you witness the experiments in connection with it?—No.

18297. Then in regard to the causation of pneumonia, did I correctly understand you to say that the submission of the animal to an excess of oxygen contributed to the production of pneumonia?—It causes pneumonia itself.

18298. Apart from any organism?—Yes.

18299. That pneumonia can originate without bacterial cause?—Yes.

18300. Is that an example of inflammation without bacteria?—Yes.

18301. And is pneumonia in man also of non-bacterial cause?—No.

18302. Never?—The usual pneumonia is caused by bacteria.

18303. But is there any case in man of an analogous disease process called pneumonia similar to that which you can produce in animals by the administration of an excess of oxygen?—It is conceivable that in the caisson disease you might have a certain element of oxygen effect of that sort.

18304. Is the result of your investigations to suggest that oxygen ought not to be employed in the treatment of pneumonia?—I have debated that point with physicians like the late Dr. Dreschfeld. It is rather difficult to answer it definitely. I think he did not use it when he saw these experiments.

18305. If oxygen produces pneumonia, the use of oxygen in pneumonia would be rather a homœopathic remedy would it not?—Yes.

18306. We have been told by a previous witness that the use of oxygen in some respiratory diseases had been the outcome of experiments on animals. Is the result of your research to suggest that oxygen ought not to be employed in respiratory diseases?—That is a debatable point as to how far you should go in using oxygen in acute pneumonia.

18307. It implies a caution?—It implies a caution. I go that length.

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18308. And that is a very recent research, I understand?—Yes.

18309. When was it published?—In 1899.

18310. Did I correctly understand you to say that morbid anatomy had been of great value in arriving at conclusions in some of the cases to which you referred; I took down your words to that effect?—Yes.

18311. You do not wish to disparage the importance of morbid anatomy as a means of research?—Not in any way. I merely wished to indicate that the natural development of the study of any disease was to commence with morbid anatomy, and that the experimental work was required to test the conclusions.

18312. So I understood you. Then you indicated certain researches that you thought had a useful bearing upon the cause and possible prevention of general paralysis. Do you then accept the bacillus paralyticans as the cause of paralysis?—The experiments that I have seen myself were done by a variety of toxin-producing microbes; the bacillus coli was tried and Gaertner's bacillus and a mixture, I think, of bacilli.

18313. What was the result of those investigations?—That you get definitely degenerative lesions in the spinal cord at the point where the nerve emerges.

18314. Does the evidence point to spinal paralysis being a bacillary disease?—No, on this particular part probably does not; but there has been a large amount of evidence published lately to connect it with a definite bacillus, the diphtheroid bacillus. This investigation was rather to show that experiments have found out a mode of introducing the toxin into the nervous system in a form in which it has local effects.

18315. May the toxin be of various kinds?—Apparently, so far as I have seen the experiments, it may be of various kinds.

18316. Am I right in thinking that the bacillus paralyticans has been alleged as the cause of general paralysis?—Yes.

18317. Is the result of your investigations to prove it or to disprove it?—We have not worked with that particular point before us, as to whether that particular bacillus is the cause or not.

18318. Have you been able to produce the results that you identify with those of general paralysis without the use of this paralyticans bacillus?—Yes. I should say that we have, with ordinary bacilli such as you find in the intestines, been able to produce effects of this order.

18319. And with poisons derived from other than bacillary sources have you tried the same experiments?—No.

18320. Would you tell us where those fasting experiments on dogs were performed?—They were performed by Rosenfeld and Lebedeff.

18321. Where did they work?—That I am not sure.

18322. In Germany?—Yes. Rosenfeld published them in the "Zeitschrift für Klinische Medicin."

18323. When?—I will supply the date. I have not it in my mind at present.

18324. Is that also a recent research?—Comparatively recent.

18325. Have any similar investigations been made in this country?—Not so far as I know.

18326. (*Sir John McFadyean.*) Your own experiments and all those that have come under your observation at Manchester University have been carried out in strict conformity with the Act?—Yes.

18327. Have you any reason to suppose that anywhere else the law is violated in experiments on animals?—It has not come under my observation.

18328. You have seen experiments conducted in other laboratories?—Yes. I began work in Oxford at the Physiological Laboratory. I worked in Cambridge in the Pathological Laboratory. Then I was ten years in Belfast and saw experiments and a good deal of work going on there, both in physiology and pathology; and then I have worked lately in Manchester.

18329. So that as a means of ensuring compliance with the Act, it would be in your opinion entirely un-

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necessary to legislate, so as to give greater publicity to the names of those who are engaged in vivisection, and to give in fuller details for public information the nature of the experiments?—Yes.

18330. You are familiar to some extent, I suppose with what might be called anti-vivisection literature?—To some extent. I have not studied it very far.

18331. Have you read any of the evidence given before this Commission?—Yes, I have read some of it.

18332. You are aware that there is a pretty widespread opinion among the laity that a great deal of painful experimentation is carried out in this country under the Act, especially by physiologists?—Yes.

18333. You do not agree with that?—I have not seen it.

18334. Do you believe that there is painful experimentation carried out?—No, I have no reason to suppose there is.

18335. You are aware that great trouble has been taken to persuade the public that that is so?—Yes.

18336. Would it in your opinion be justifiable to call these attempts misrepresentations of the facts?—Yes, the statements about the cruelty and pain which are inflicted are misrepresentations, so far as I know.

18337. Do you think that that misrepresentation of the facts may be an honest misrepresentation?—Yes.

18338. But that it might be continued and even increased if the laity were provided with details even of painless experiments?—Yes.

18339. Would it occur to you that that would be one serious objection to publishing fuller information with regard to the details of pathological and physiological researches?—Yes.

18340. Might I ask is this oxygen pneumonia a progressive pneumonia; I mean progressive in the sense in which the bacterial pneumonia is progressive, that you can bring it on by causing an animal to respire an atmosphere of pure oxygen?—Yes.

18341. Does it continue and tend to a fatal issue afterwards?—Yes.

18342. And the tissue at the time of death is found pneumonic?—Yes, consolidated, congested.

18343. But germ free?—So far as I have been able to see I have discovered no germs.

18344. I just want to ask you a question or two about these milk experiments which are carried on so extensively at the Public Health laboratories. Am I right in supposing that these are carried out with the intention of taking advantage of the special powers which Manchester possesses regarding tuberculous milk?—Yes.

18345. The object of the experiments and of the tests to which the milk is subjected is to diminish the number of tubercle bacilli supplied to Manchester people in their milk?—Yes.

18346. Do you happen to know whether it is the opinion of the Public Health Authorities there, that the work has been in any respect successful in that direction?—I can judge from the Medical Officer of Health's report, which contains endless references to the work that is carried on.

18347. Approving of it?—Yes, and relying on it.

18348. From your own knowledge, can you offer any opinion to the Commission as to whether it would be of any real value in that connection to merely make a microscopic examination of milk?—I have tried it frequently without satisfactory results.

18349. Why is microscopic examination unreliable when the result of the examination is a negative?—The result of the examination is that bacilli may still be there, although you have been unable to discover them.

18350. Is that because it would be practically impossible to examine even half a pint of milk in six months?—Yes.

18351. Is there a difficulty in recognising the tubercle bacillus with certainty by the microscope in milk taken from a shop?—Yes, there is, you get it in various conditions.

18352. Are there other bacilli that have the same reactions and agree with it morphologically?—Yes, the butter bacilli and others.

18353. On the other hand, do you think that the inoculation test is a very delicate one?—Yes, it seems to be very delicate.

18354. Can you say whether, as a matter of fact, as employed in Manchester it has led to the detection on different occasions of cows which are actually the subject of tuberculosis of the udder?—Yes, I believe so.

18355. (*Sir Mackenzie Chalmers.*) As regards the inoculation experiments done in the Public Health laboratory, I suppose as soon as the animal unequivocally develops the disease you destroy it?—Yes, so I understand.

18356. It is not necessary to let it die?—No.

18357. You can destroy the animal as soon as the disease unmistakably shows itself?—Yes, say in the glands.

18358. The starvation experiments that you referred to were somewhere in Germany, but you do not know where?—Yes.

18359. In England we have starving men, of course?—Yes.

18360. But by mere observation of them you cannot get the necessary information?—No; you cannot define it; you cannot test it in the same way.

18361. In fact you would want to kill them and make a *post-mortem* examination?—Yes.

18362. What certificates do you hold?—"A" and "B" just now.

18363. "A" is inoculation. Under "B" the animal must be anæsthetised during the experiment?—Yes.

18364. But is allowed to recover?—Yes.

18365. Are you following any particular research under Certificate B now?—I have got the licence, but when I got it I was led off to another investigation.

18366. You are not using it?—I am not using it.

18367. You have experimented under Certificate B?—Yes.

18368. I want to ask you this. When you experiment under Certificate B, do you understand that not only the cutting operation itself, but any subsequent painful part of the operation, such as stimulating a nerve, or sewing up the wound, must be done under anæsthetics?—Yes.

18369. There is no doubt about it in the physiologist's mind?—I should think not.

18370. Are you a practising physician as well?—No.

18371. Your work is professorial?—Yes, and hospital work. I am pathologist to the Royal Infirmary.

18372. You represent the Victoria University. May I ask, do you come here giving this evidence on behalf of the University as a whole, or on behalf of the Medical Faculty?—I represent the Council of the University.

18373. I ask for this reason. In March last we had a witness from the Victoria University, Manchester, Mr. John W. Graham——Not from the University may I be allowed to say.

18374. He described himself as Principal of Dalton Hall in the University of Manchester?—Yes.

18375. Did he represent any Faculty of the University?—No, my evidence is from the Council of the University, which is the Executive Body.

18376. Is Mr. Graham a member of the Council?—No.

18377. (*Mr. Ram.*) What is Dalton Hall?—A hall of residence for students.

18378. Are they taught there?—There may be a little tutorial work.

18379. They study at the University and live at Dalton Hall?—Yes.

18380. (*Dr. Wilson.*) When you speak of the Council do you mean the Medical Council, or the Council of the whole University?—The Executive Body of the University.

18381. Arts?—Arts and Sciences, and Law, and Music, and so on.

18382. (*Sir Mackenzie Chalmers.*) How long have the laboratories been open at Manchester?—I could hardly say that.

18383. Some years?—Oh, yes.

18384. Do you know whether Mr. Graham made any inquiries there as to what went on in his own University laboratories. Has he made any inquiries from you?—Not from me.

18385. Would it be open to him to obtain information as to what went on if he wished?—I should be very pleased to show him anything he might desire.

18386. So far as you know, no inquiry was made by him as to what went on locally in his own University?—That is so.

18387. I suppose as Principal of Dalton Hall he holds a University position, or does he not?—I could not say offhand the relations of these halls to the University. They are controlled in some way or other, but I think he is appointed by an independent committee—the Governors of the Hall.

18388. One question with reference to what you said about general paralysis. You referred to general paralysis of the insane. Is it not commonly supposed that general paralysis of the insane is a sequel of syphilis?—Yes.

18389. Has your work tended to negative that in any way?—Not necessarily. Our work has been rather on the method in which toxins are absorbed so as to localise the lesions in the spinal cord. The toxin variety of toxins may be used for the purpose of the experiments described.

18390. Various toxins might produce the same effects?—Yes, the same degenerative effects.

18391. (*Mr. Ram.*) With regard to these starving experiments you told us that the animals appear to suffer discomfort and so forth?—I am speaking from hearsay only. I have never seen them.

18392. You have never seen a starvation experiment?—No.

18393. Can you tell us what generally happens to the animal at the end? Is it allowed to recover, fed back into life, or destroyed?—It is fed back and then examined afterwards.

18394. Can you tell us of any discovery which has had an actual beneficial effect upon mankind, which has resulted from these starvation experiments?—This of course, bears upon the pathology of fatty degeneration of the heart; that it why I adduced it.

18395. Has the treatment of fatty degeneration of the heart or medical knowledge with regard to it been advanced by these experiments on animals?—Yes, I think it has.

18396. May we put it so high as to say that knowledge has been acquired by these fasting experiments which has been directly beneficial to mankind in the treatment of fatty degeneration of the heart and other diseases?—Directly beneficial in the sense of contributing to our knowledge of these conditions.

18397. Has the treatment with regard to fatty degeneration of the heart, for instance, changed at all in consequence of knowledge derived from these experiments?—It would be difficult to say that.

18398. You do not know that it is so?—I do not know that it is so.

18399. When you say that the animals are brought back to life and are examined, are they examined as live animals?—No, they examine the chemistry of their organs; there is a chemical examination.

18400. The animal is killed?—Yes.

18401. And then by an autopsy the organs are examined?—Yes.

18402. (*Dr. Gaskell.*) Have you read the evidence of the Honourable Stephen Coleridge?—Yes.

18403. I think it would be rather a good thing for this Commission if you would just tell us a bit about that case of Mr. Cecil Shaw to which he referred. His statement is at Question 10491, "Is it now your suggestion that these two gentlemen were saying what is not true, and that Dr. Cecil Shaw did these experiments? (a) That is my suggestion." And then he goes on afterwards to say, "My complaint is with respect to the Irish Government, that they immediately accept what I call the absurd explanation of these two vivisectors." Is it true that Mr. Shaw did experiment without a licence?—No.

18404. Can you tell us what were the facts?—I did the experiments.

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18405. Is it necessarily the case that Mr. Shaw implied that he did the experiments when he speaks of "my own work"?—No.

18406. He uses the first person, as though that experimental work had been carried out by himself?—Yes, as if he had done the whole of it.

18407. Will you tell the Commission about it?—The circumstances of the investigation are these. The late Dr. McKowan, an eminent opthalmologist in Belfast, appealed to me to undertake this research. I was not specially acquainted with Opthalmology, but I tried to meet him. The best way of doing it was to associate myself with an opthalmologist and it was in these circumstances that the combination came about.

18408. And what part did Mr. Cecil Shaw take in it?—Of course we discussed the whole situation. He practically did the whole research except the vivisection part of it.

18409. Why did your name not appear in connection with it?—It should have done, I admit; it would have been less ambiguous.

18410-11. Still it was not your research really, although you did the experimental work?—Yes. I was working, of course, at this question and have been working at it since. These experiments which I have just recounted with regard to the absorption of toxins along the nerve sheaths is in the line of this investigation; it has engaged my attention more or less continually ever since.

18412. Mr. Coleridge states here "The letter from Dr. Lorrain Smith, who had a licence, to his very good friend Dr. Shaw, who had not, is as follows. . . . Both of them are dated the same day; they must have met in the street and exchanged them." He implies that you met Mr. Shaw and then in the street casually concocted this arrangement of letters and the evidence. Is there any truth whatever in that kind of suggestion?—No. What else could I do? Shaw had written the paper, and, as I explained in my letter to the Irish Office, "I appealed to him at once for what statement he had to make about this, and I enclose his reply." I do not see the ground for any such insinuation. I have explained the whole circumstance.

18413. And your Irish Home Office were satisfied with your explanation?—Yes, they were satisfied.

18414. The experiments were done in Belfast?—Yes.

18415. (*Sir Mackenzie Chalmers.*) At the time of these experiments Dr. Shaw held no licence from the Irish Government?—No.

18416. But you did hold an Irish licence?—Yes.

18417. (*Dr. Gaskell.*) The returns are sent in by you?—Yes. There is a reference that I do not understand to Dr. Shaw having applied with regard to monkeys and dogs to the British Medical Council on the 22nd July last. I know nothing about it whatever. I left Belfast in 1904.

18418. He called it corroborative evidence that he, Mr. Cecil Shaw, is the person who did those experiments, the corroborative evidence being that he applied to the British Medical Council in July last?—Concerning experiments which happened in 1897; I do not follow it.

(*Chairman.*) I do not think it impressed anybody very much as being corroborative evidence.*

18419. (*Dr. Gaskell.*) You mean with respect to the date?—Yes, I had been away from Belfast for over three years. I was attacked another time about doing experiments without a licence myself in a research on bacteria of the intestines of dogs, although I stated in the investigation that the intestine was removed after the animal was dead. I had the same sort of correspondence with the Irish Office as to whether my licence should be taken away and my laboratory shut up.

18420. Do you consider that these experiments did cause the rabbits a good deal of pain?—No, they did not. We did not cause the iritis; we did not succeed in causing iritis.

18421. So that the jequirity did not cause a great deal of pain afterwards, after the operation was over?—No, it was a very minute quantity of jequirity, just

* *The National Anti-Vivisection Society subsequently wrote* (31*st December*, 1907) *that the words "British Medical Journal of* 22*nd July last" in Q.* 10496 *should read "British Medical Journal of* 22*nd July*, 1899," *which correction Mr. Coleridge omitted to make in revising the proof of his evidence sent to him in June.*

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the least speck of dust on the point of a camel-hair brush.

18422. No inflammation was caused?—No, except conjunctivitis. It was very like an experiment with tuberculin, Calmette's experiment for the diagnosis of tuberculosis in man.

18423. Then the net result of those experiments is negative?—Yes, in no case did we cause iritis.

18424. There is one other question I want to ask you, and that is at Manchester do you demonstrate to students?—I do not.

18425. Are there demonstrations in pathological experiments and experiments involving vivisection?—No.

18426. Do you consider that such experiments might be useful to students?—I do

18427. Would you encourage them?—In pathology?

18428. I am rather thinking of pathology and pharmacology; I was thinking of the course as a whole?—I could quite imagine their being of enormous teaching value. I know how I was impressed the first time I saw a physiological experiment myself. One of the things that stands out in my recollection of Professor Rutherford's teaching almost more clearly than anything else in the experiment of blood pressure.

18429. (*Dr. Wilson.*) You have just mentioned Professor Rutherford. You graduated in Edinburgh University?—Yes.

18430. And, of course, you have often seen his experiments before his class?—Yes, that is to say, I was a year there, and I saw the two or three experiments that he did.

18431. In carrying out those experiments on blood pressure, when it was necessary to keep up the respiration of the dog, for example, by inserting a tube into the trachea, how did Professor Rutherford proceed, I mean as to the anæsthetisation?—I could not tell you what anæsthetics he used. It is no use trying to recollect the details of over 20 years ago. I could not charge my memory.

18432. Was the animal anæsthetised before the tube was inserted into the trachea?—Oh, surely. I have no specific recollection of the point, but the animal seemed to be anæsthetised throughout, and I am sure it was.

18433. That is to say it did not strike you that any cruelty was inflicted during even the severest experiments?—That is so.

18434. And, of course, in all these class experiments the animal was destroyed before recovering from the anæsthetic?—I believe so. I was only a student, of course, amongst hundreds of others.

18435. Then with regard to these experiments which are being conducted to elicit the conditions of arterial sclerosis, is it not generally assumed that experimentation on animals is justifiable in order to discover, if possible, the causation of the disease?—Yes, I think so.

18436. But you could not say that arterial sclerosis is ever caused in the human being by adrenalin?—We are not ready really to say anything about it yet. We are only working it out. We have not yet, properly speaking, defined the form of arterial sclerosis and its causes.

18437. Would you, as a pathologist, say that you can ever induce in an animal the exact disease that you find, even in a diseased animal?—If you mean absolutely identical in every respect, no, I suppose not.

18438. You think not?—Some bacterial diseases are extremely difficult to distinguish, anthrax, for example.

18439. You could not say, then, that this arterial sclerosis which Josué has been able to induce by administration of adrenalin is the same sort of sclerosis that you find in diseased conditions of human beings?—In any case we are not far enough on yet to say so. The first English investigation, as I say, has been published from Dr. Woodhead's laboratory in last October. There are a few others.

18440. Then you say that fatty degeneration of the heart can be caused artificially, or by experiments on animals by starvation experiments?—No, I did not say that. I said that starvation was one of the modes of investigating the nature of fatty degeneration of the heart.

18441. You do not cause it by starvation?—No, you cause it in other ways.

18442. But how is the condition brought about then if after starving animals you find a certain form of fatty degeneration?—You may cause degeneration, say by giving a little phosphorus or arsenic.

18443. But that is not the usual way in which it is caused in human beings?—It is caused there, too. It is a very curious thing that you can cause it in a great many ways. It is caused by lesions, toxins, poisons.

18444. A question was asked you about deciding as to ptomaine poisoning by experiments on animals. May I ask what is the condition of the animal which will enable you to say that a certain food contains ptomaines?—You would kill the animal with the ptomaine.

18445. But what conditions would you expect to see in the animal?—You might even get it infected with the bacillus. There are various forms of bacilli described.

18446. The Gaertner bacillus?—Yes.

18447. Would you expect to find that?—Yes, I have found it in a case.

18448. But you would not expect to find it always?—Not always. I found it in one case that I investigated, and it seemed to have come from meat poisoning, so far as I could make out.

18449. In those laboratory experiments which are carried out for the Corporation, how long would it take to be able to decide in a doubtful case of diphtheria, for example?—I should say within 48 hours.

18450. But supposing that you have to resort to a guinea-pig?—Yes, within 48 hours.

18451. Could you tell from the condition of the guinea-pig within 48 hours that you are dealing with the Klebs-Löffler bacillus?—Yes, I think so.

18452. How long would it take in using the guinea pig to decide whether the bacillus in question is the tubercle bacillus or not?—I think, as a rule, within six weeks. The usual time is about a fortnight or three weeks.

18453. It is now admitted, is it not, that it is exceedingly difficult to dissociate the tubercle bacillus from a number of other bacilli which are found in milk?—There are various ways that would dissociate them experimentally.

18454. The tubercle bacillus would bring about certain pathological conditions in the guinea pig?—Yes.

18455. Animals do not suffer from rheumatoid arthritis as far as you know?—No.

18456. You mean by rheumatoid arthritis, enlargement of the joints?—Yes, and wasting of the structure in the neighbourhood.

18457. But nothing definite has been yet discovered through experimentation with regard to that disease?—No.

18458. (*Sir William Church.*) You have had considerable personal experience in laboratories in which experimentation on animals goes on in Edinburgh and in Oxford, in Cambridge, and in Belfast, and in Manchester?—Yes, I did not have much experience in Edinburgh, except as a student. I have been in laboratories there a little, but not very much.

18459. And I think also you have probably visited other laboratories in this country?—Yes.

18460. We had a witness before us lately who seemed to be under the impression that when an animal was kept under chloroform by means of having a tube introduced into the trachea, and then vapour of chloroform supplied to the animal through that tube, the tube was introduced without the use of anæsthetics; that is to say, the wound was made without the use of anæsthetics. Have you ever seen it done?—Never.

18461. Not in Edinburgh when you were a student?—No.

18462. There is no difficulty whatever in keeping an animal under anæsthesia under those circumstances?—No.

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18463. (*Chairman.*) You, I believe, are Honorary Secretary of the Personal Rights Association?—I am.

18464. What is that Association?—It is an Association for the defence of the personal rights of men and women, of course, and secondarily of animals.

18465. That is what I asked you the question for. In personal rights do you include the personal rights of animals?—No, we should not call the rights of animals personal rights, but we are obliged to take into consideration the rights of animals for this reason, that if we did not recognise that the animals have rights, we should be obliged to consider every prosecution for cruelty to animals as an attack on the personal rights of the person prosecuted; therefore in that secondary way we are obliged to take into consideration the rights of animals.

18466. Has your Association any Hall or place of meeting?—32 Charing Cross.

18467. You have offices there?—We have an office at 32 Charing Cross. I may say that we have been in existence since March, 1871, and we have had quite an illustrious band of vice-presidents.

18468. And how many members have you?—From 120 to 150. We are only a small number of people meeting in an upper room, but I hope that will not militate against our evidence being considered valuable.

18469. It is a question we ask of most witnesses who come to represent particular bodies?—Quite so.

18470. I should like to say first before I examine you, and other gentlemen examine you, that we cannot admit as part of your evidence that pamphlet which is appended in print to your *précis* called "The Ethics of Vivisection"; we cannot print it bodily as evidence; but we shall ask you questions on the points mentioned in your *précis* and if there are any answers that you wish to support by the arguments used in your pamphlet, of course you can use them for that purpose?—I am very sorry for that, because although my *précis*, including the appendix, has been on our shelves for nearly twelve months, I consider that if you would read it through——

18471. I have done so?—I am very much obliged to you, my Lord. I consider that it is almost a sufficient answer in itself to the evidence that was given before this Commission by Lord Justice Fletcher Moulton, that is to say, it anticipates that evidence, and I think replies to it.

18472. I have told you what I propose to ask you; it may be that other members of the Commission will take a different view and go into this matter more than I should do, but I propose to ask you with regard to the first two pages of your *précis*, which, I think, open the whole ground of your views?—Yes.

18473. You are going to tell us what you consider to be the case set up by those who are in favour of vivisection as regards its morality?—Yes, that case is almost—if not entirely—based upon the direct balancing of the pain inflicted on the animal and the pain which the vivisectors suppose to be avoided by the knowledge which they may obtain in that way. I have contended, in the portion of my *précis* which is not to be considered now, and I believe that I have contended successfully, that that is not sound ethics at all—that the hedonistic ethics, which Lord Justice Fletcher Moulton put before you (and which I believe to be sound ethics at the bottom) cannot be applied directly in that way; that you have to obtain from it your *axiomata media*, as Austin contended, and apply them, and that any attempt to apply the hedonistic end directly, by the balancing of the evil and the good, is altogether a mistake and unsound ethics. I could show you that by taking a case. Supposing that a man who has a wife and children at home wanting food goes into a railway carriage, and sees there a bag of sovereigns marked "Rothschild." Shall he steal those sovereigns or not? By direct balancing, the ethical verdict would have to be in favour of his taking them; the loss inflicted upon Rothschild could be a very small matter indeed, while the gain in avoiding the pain to his family would be very great. If you decide that matter in the way of direct balancing, the decision will be in favour of the man committing the theft. The proper way, however, of dealing with the matter is to arrive at some mediate axiom—that of honesty in the matter of property, which would decide exactly the other way. And so it is with regard to this vivisection: if you argue it by way of direct balancing you come to one conclusion; if you argue it by your mediate axiom of morals you come to another conclusion.

18474. I see in your *précis* you state the grounds upon which those who approve of vivisection base that approval?—Yes, there are three grounds.

18475. You have dealt with the second of them just now; that is to say, that the pain or cruelty inflicted is more than compensated by the knowledge obtained which enables the medical profession to lessen pain and loss of life, so that the balance on the whole is in favour of happiness?—Yes.

18476. The first I see you state is that animals vivisected within the scope of British law feel no pain, or at all events not such a degree of pain as to constitute cruelty?—Yes.

18477. But do you contend that if an animal is operated on under complete anæsthesia, and is not allowed to come out of it, that is cruelty?—No, you will find on the next page of my *précis* that I deal with that; that if an animal is operated upon after the sentient life of that animal is completed; that is to say, if the animal is under complete anæsthesia and is not allowed to recover from that anæsthesia, so that it dies in that state, then the operation on that animal although technically during its life is after its sentient life is ended and, as I should view the matter, it is more one of anatomy than of vivisection; it is more properly to be called anatomy than vivisection.

18478. Then as regards the first ground, you say that if every animal that was operated upon was operated upon under complete anæsthesia, that is an answer?—Yes, if we could get any adequate guarantee that the animal is operated upon under a complete anæsthesia, and in the preliminary stages of putting that animal under anæsthesia there is nothing done that is objectionable, and the animal is not allowed to survive, that is to say, is not allowed to come out of the anæsthesia, but dies before it does so, then for all practical purposes that animal has been operated upon after its sentient life is ended, and therefore from that point of view may be regarded as a dead animal and the operation itself may be regarded as anatomy. But I am bound to tell you that I very much doubt whether you will get guarantees for anything of that kind being done—that is the point.

18479. You are not yourself, I understand, a physiologist?—I am not, and I do not propose to give evidence in that connection.

18480. We have had a very large number of physiologists before us, the purport of whose evidence has been that the Act has been enforced fairly and properly as regards anæsthesia; if that is so that would deal with the experiments under a simple licence?—Yes; but I do not share their opinion.

18481. But it does not deal with the cases in which the animal is allowed to recover, or where it is desired that the animal should recover afterwards, and be watched, or with the cases of inoculation for disease?—No.

18482. To those cases you object, I understand?—I do. I would like the Commission to understand that when I say that I should have no objection whatever to an animal being vivisected under complete anæsthesia, and killed or allowed to die in that state of anæsthesia, I do not commit myself to saying that any guarantees which have been proposed up to the present time appear to me sufficient for that purpose.

18483. But you would admit that that is a question of fact, would you not? The Act has been at work for 30 years, and it is a question of fact and evidence as to whether or not it has worked satisfactorily so as to prevent anything in the nature of cruelty?—I quite agree with that, provided that it is distinctly under-

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stood that I am not saying that I consider it satisfactory.

18484. Quite so. May I take it that you do not see any justification for allowing animals to come to after an operation in order to watch the results?—Provided that there were no pain connected with it of any serious kind, I should see no objection even to that; it is only a question of whether the animal is subjected to serious pain in the matter. I see no objection to an experiment on an animal as such. If anybody proposed to try the experiment of whether an elephant could carry a threepenny bit from, say, Trafalgar Square to the Houses of Parliament, I should not object, and if the load were increased I should also not object up to the point beyond which the load became such that the experimenter would be prosecuted under the Act for Prevention of Cruelty to Animals.

18485. And do you object to inoculation, giving an animal a disease, on ethical grounds?—I consider that the whole of the present practice of the propagation of disease through animals is an abomination which will be swept away at some future time.

18486. Would that be your opinion, although you were satisfied that good results for mankind, and the treatment of diseases of mankind, were obtained by reason of those experiments—would that alter your view?—I do not believe that good results can be so obtained.

18487. I understand that?—But even if you could show me some good physical results which could be obtained in that way, I believe that there are other considerations connected with the matter which ought to condemn the practice, and I feel quite sure that, in the future, the medical profession generally will condemn the practice.

18488. That is to say, that you do not think that the saving of a great number of human lives or a great deal of human pain would justify the operations by way of inoculation of giving a disease to a score of animals?—It is the same thing with regard to all questions of morals. This question does not arise simply with regard to vivisection or with regard to some animal disease; it arises with regard to every moral law that we have. Take the question of lying or the question of murder. Supposing that by committing a murder you think you can save several other lives, the same question arises. Take the case of Eugen Hahn, which I have put in that statement of mine, on which, my Lord, you are to examine me. This surgeon, who took a bit of cancerous matter out of a woman's bad breast and inserted it into her healthy breast. Suppose Eugen Hahn could show me that by his doing that to that woman he could save 50 other lives, 100 other lives, or a thousand other lives, I would say, "No, you do not prove to me that you have done what ought to be considered right. The evil to humanity by allowing such things to be done far transcends any medical gain that you could possibly get in that way." I would contend also that—as I have shown there, and shown most conclusively—these experiments are not confined to animals, and never have been confined to animals; they have always been performed on human beings, as well as animals. Any of you gentlemen who are acquainted with this book by Dr. Smidovich will find the eighth chapter of it full of instances, with references to pages in magazines and medical journals, in which the authors of those crimes have themselves put on record the fact that they have committed them.

18489. That case of Eugen Hahn to which you have referred was a case, judging from your account, in which a surgeon whose duty it was to do his best for his patient took the opportunity of poisoning her?—But why is a surgeon bound to do the best for his patient? Suppose that he can, by poisoning a patient, produce a great deal of good for the rest of humanity, and save a lot of lives, on vivisectionist ethics he ought to do it. Why, if it is a question of direct balancing, should it not be done?

18490. You are putting vivisection ethics somewhat differently from the way in which they have been put before us. You have a right to do it, I suppose, in arguing the ethical question?—I think I have a right to put it so. If it is a matter of direct balance, one way or the other, why should not Eugen Hahn do that which he did?

18491. You think that on the principles of the vivisectors he ought to do it?—I think that if they were carried out logically that is what they would do. I think that they dare not, generally, do it; that is to say, they dare not carry out their principles logically.

18492. Your second ground about the balance in favour of happiness you have already dealt with?—I have dealt with that, I think.

18493. Then the third ground which you put into the mouth of the vivisectionist is that vivisection is part of the procedure of science, and moralists have no right to dictate to science, but, on the contrary, should take their direction from it?—Yes.

18494. Is not that rather a controversial way of putting your adversary's argument for him?—Then let me put my adversary's argument in words out of his own mouth.

18495. Who is the person speaking?—I have prepared an answer to that.

18496. Out of whose mouth?—Not merely out of the mouth of one, but I will give you several. The vivisectionist doctrine, as an ethical doctrine, is the primacy of science over morals. In other words, vivisectionists claim, as scientific men, to be unrestricted by moral considerations, or by public opinion. Professor Metschnikoff, at the conclusion of the Harben lecture on the 30th May of last year, after advocating the vaccination of young girls with syphilitic material— with which he had succeeded in infecting monkeys—in order to fit them for a career of prostitution ("Journal of Preventive Medicine," page 459), declared that "morality should not attempt to lead hygiene, but should rather follow her," and that "modern hygiene, having become an exact and infinitely more precise science than it was formerly," ought "to reign supreme over all moralizing doctrines." This statement was met with loud applause and stamping by an English scientific audience.

18497. Were you present?—I have a letter from a person who was present, and I can put that letter in as evidence, if you like. The lecturer was complimented at the close of the lecture, special reference being made to what he had said about morals and hygiene; and amid enthusiastic applause he was presented with the Harben gold medal. Under these circumstances I cannot dissociate British vivisectors from their foreign coadjutors. One of the frankest of these, Dr. Smidovich, in his book, "Confessions of a Physician," page 152, says: "There is but one way out of the dilemma—that of stifling the reproaches of conscience, of choking down pity, and closing one's eyes to the living agony of the animals sacrificed." In his reply to Questions 3737 and 3738 Professor E. H. Starling said: "It is the greatest asset which a nation can have to have among itself a number of men endowed with this "mere curiosity," men who will put everything second to the advancement of knowledge." In "Nature," a fortnight before this was said (6th December, 1906, page 122), Professor Starling wrote: "A clamour has been raised by certain agitators for a restriction of experiments to those which can be shown to have a direct utilitarian object. Such a restriction is impossible. . . . These researches must be undertaken in a spirit of pure curiosity, from a love of knowledge itself." It will be observed that the argument is everything is second to the advancement of knowledge: vivisection is necessary to the advancement of knowledge; therefore everything is second to that for which vivisection is necessary. Dr. de Watteville wrote to the "Standard" (23rd November, 1883): "I think we, as medical men, should not attempt to conceal from the public the debt of gratitude they owe to the *corpora vilia*—for such there are, and will be as long as the healing art exists and progresses." He pleaded that moral and pecuniary support should not be "refused to hospitals on the ground that their inmates are made use of otherwise than for treatment." He contended that "no amount of hysterical agitation and so-called humanitarian agitation will alter the laws of Nature, one of the plainest of which is that the few must suffer for the many. Sentimentalists who think they know better, who uphold the abstract 'Rights of Man,' and want to push them to their logical consequences, have no other alternative in the question now before us than to condemn the modern course of medical studies." And he concludes: "Whilst defending the moral grounds upon which experimental medicine rests, I allow that there are limits, narrow limits, beyond which it would be

imprudent or criminal to go. But I must emphatically protest against the tendency of men nowadays—and I am ashamed to observe that a few are to be found within the medical profession itself—who act upon the supposition that the public at large form a proper tribunal to decide upon what constitutes a transgression of those limits. Those alone are competent judges who are able to form a correct opinion on the one hand of the ultimate utility, on the other of the proximate consequences, of any investigation *in corpore vili*." Will your Lordship allow me to add that Lord Justice Fletcher Moulton, in his evidence before this Commission, really sustained the same ground—although he put it in words that are more consonant with English feeling, perhaps—when he said in answer to Question 12783: "'Conscience' is too often used where the true expression should be 'emotion.' Emotion may be a good motive power, but it is a bad guide, and in my opinion there is a very great deal of force in the phrase which somebody has used: 'If you want to do good in a particular way and want to know how you can do it effectively, give your heart a rest and your brains a chance.'" You see conscience is turned into emotion, emotion is turned into the heart, and you have to put your heart away; that is to say, you have to do what Professor Metschnikoff tells you, you have to put your conscience away. That is just what Dr. de Watteville tells you; it is the same thing throughout. The ethical ground of vivisection, I contend, is really a repudiation of morals and a contention that the pursuit of knowledge is to be unrestricted by moral considerations.

18498. You are putting it now in a somewhat different way from that in which it is put in your *précis*. You say there that the argument is that vivisection is part of the procedure of science, and moralists have no right to dictate to science, but, on the contrary, should take their direction from it?—Those are almost exactly Professor Metschnikoff's words.

18499. That is not the view which has been taken by the great bulk, I think, of the witnesses who have been before us?—I am very glad to hear it, but it is undoubtedly the view of Professor Starling; it is undoubtedly the view of Professor Metschnikoff, and, so far as I can see, it is the view also of Lord Justice Fletcher Moulton.

18500. (*Colonel Lockwood.*) You said just now that you had reason to believe that anæsthesia was not always complete; have you any authority for that statement?—My idea respecting that has been gathered from what I have read and from the fact that curare is used along with the anæsthetic. I do not see how you can really guarantee that you have got the animal under an anæsthetic if you use curare at the same time.

18501. Then is it a sort of general statement, for which you have no authority?—My conclusion has been gathered from my own reading. As I have told the Commission, I have not come here to-day to give evidence upon any point except the ethical side of the question.

18502. (*Sir William Church.*) Why do you place a limit upon the rights of animals?—Rights must be limited by responsibilty; you cannot make animals responsible in the same way that you can make a human being responsible. I limit the rights of animals; I also limit the rights of children, and I limit the rights of idiots and lunatics, simply because you cannot make them responsible. And there is also another ground for the limit, namely, the ground of sensibility.

18503. You said, I think, that you thought that we had a perfect right to kill an animal for experimental purposes; that if the sentient life of the animal was dead you had no objection to experiments?—None whatever.

18504. What right have you to kill an animal to make an experiment upon it?—If the animal is a domesticated animal it is kept by us—we keep it, and there is no possibility of our carrying on any such intercourse with animals unless we have certain rights over them. We are bound to kill in order to live ourselves, with regard to a great number of animals. We are bound to kill them even for vegetable food; we cannot get any sort of food without killing animals, and there is no reason why this should be limited to the mere obtaining of food. If we can get any other good from them without inflicting upon them any wrong, then we should undoubtedly do it.

18505. If we have a moral right to kill animals to feed ourselves, why have we not a moral right to kill animals to protect ourselves from disease?—We have such a right; I have never denied it.

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18506. Then you maintain that we have a right to kill, but we have not a right to pain?—Not to torture.

18507. What do you mean by torture?—I mean serious pain that is inflicted for some unjustifiable purpose.

18508. Then it is justifiable to feed ourselves, but not justifiable to try and protect ourselves?—Exactly, it is not justifiable to protect ourselves in that way.

18509. I do not quite understand what you said with regard to the propagation of disease through animals. Animals are used experimentally for the diagnosis of disease, not the propagation of it?—They are used for the diagnosis of disease; that is to say, they are given certain diseases for that purpose; and they are also given diseases, not for the purpose of diagnosis, but for the purpose of experimenting on these diseases; and, in answer to a question from the Chairman, I said that I believed that the whole of that practice of the propogation of disease through animals was a wrong thing; that it would pass away, and that the medical profession themselves would come to that opinion. I give that as my opinion.

18510. I still do not understand what you meant by the propagation of disease?—I mean this. If you give a number of animals a disease, I call that the propagation of disease in those animals. Those animals would not take the disease in the course of Nature, in the ordinary way. If you give an animal a disease voluntarily and deliberately, I call that the propagation of disease.

18511. For the sake of a special observation and experiment?—I do not care for what you do it; that is the propagation of disease. I am not in the least denying that vivisectionists, in a large number of instances, at all events, perhaps universally at the outset, are actuated by good intentions. I have never said a single word against that.

18512. Have you any ethical objection to the use of what are now called sera in the treatment of disease?—If those sera are obtained by vivisectional processes, of course, naturally, I object to that, not because they are sera, but because they are obtained vivisectionally; it is only asking me the same question that I have already answered in another form. I would not allow myself, or any child of mine, to be operated upon seropathically. I may tell you, if you are interested in knowing it, that some years ago, when I was visiting Paris, I was attacked by some disease, and the doctor there proposed subcutaneous injection. I said—What are you going to inject; if it is serum or vaccine, I will not have it on any consideration. No, he said, it is morphine. I said—You may use morphine, if you like. But I would not allow myself to be operated upon seropathically.

18513. Why not?—In the first place, I do not believe in seropathy——

18514. That will do; you need not go any further.

(*After a short adjournment Sir William Church took the Chair.*)

18515. (*Sir William Collins.*) The paper appended to your evidence deals largely with a discussion which you had with Huxley, does it not?—Yes.

18516. I understand you to agree with Huxley that the field of morality is a reclamation from a state of nature?—Yes, I do hold that, and I hold it to be a very important statement of his, which is a landmark, as I think I have said, in British ethics.

18517. I gather that you think that to take the cosmic process as our moral exemplar is fatal to the ethical ideal?—Certainly.

18518. I understand that you quote John Stuart Mill as in agreement with yourself to the effect that the doctrine that man ought to follow nature, or, in other words, ought to make the spontaneous course of things the model of his voluntary action, is irrational and immoral?—That is certainly my principle.

18519. And you lay emphasis, I gather, upon the evolution of the moral sense?—Yes.

18520. That is to say, that things formerly regarded as not immoral may be held to be immoral to-day, and things regarded as moral to-day may be re-

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garded as immoral at some future date?—Undoubtedly.

18521. I see that you lay stress upon an opinion which you share that our consideration for animals is not so much subtracted from a fixed stock of sympathy resulting in less being available for our fellow-men and women, but is rather the sowing of a crop of tender emotions?—Certainly; I hold that to be a very important principle. Usually it is argued that it is the opposite—that philozoic sentiment is something subtracted from philanthropic sentiment. On the other hand, I think that not only could it be shown *à priori*, but it could be verified from experience that those who are kindest to animals tend to be kindest to human beings also.

18522. You think that it is part and parcel of the same moral sense that dictates our action towards mankind and towards animals?—Certainly; I think that the basis of all our moral actions is sympathy, and that that sympathy ought to be as wide as sentient existence.

18523. In regard to the question of the balance of advantages, the theory put forward by Sir John Fletcher Moulton, I see that you call attention to the judgment of Lord Chief Justice Coleridge in the case of the "Mignonette"?—Yes, that is a very important point. You will notice there that Lord Chief Justice Coleridge's decision is almost the exact opposite to that of Lord Justice Fletcher Moulton.

18524. I see that the Lord Chief Justice then said:—"Law and morality are not the same; many things may be immoral which are not necessarily illegal; yet the absolute divorce of the law from morality would be a fatal consequence"?—Yes, certainly it would.

18525. You agree that there may be things which may be regarded as immoral, which we ought not to punish by law?—Undoubtedly; it would be fatal to attempt to regulate our whole field of morality by law. And I may say with regard to this question of vivisection, that, although I think something may be obtained by law, still I myself rely more upon such arguments as are put forward in the appendix to my *précis* to affect the medical profession, and scientific men generally, and I think that they will gradually come round to the view which I have put forward.

18526. You agree that not everything that is morally wrong need be punishable by law?—Certainly.

18527. And that it may be a question for this Commission to decide, or to advise the Government upon, as to whether vivisection is or is not one of those things which, though you regard them as morally wrong, should yet be not punishable by law?—Yes, I think that there may be some things in vivisection which we may hold to be wrong, and endeavour to discourage, but still we should not be wise to interfere with them by means of penal enactments.

18528. I see that you rather sum up the opinion of Lord Justice Coleridge in the "Mignonette" case by saying that what would otherwise be a crime is not justifiable by the plea of a specific balance of advantage?—Yes, that is my contention throughout the whole of that essay.

18529. We have often had the question raised in this Commission, whether it is right to ride a horse to death to save a human life. What do you say with regard to that?—If it is a question between the life of a horse and the life of a human being, I should ride the horse to death as I would risk my own life.

18530. Is that adopting the balance of advantages argument or not?—I am placed between two alternatives. I have to do one thing or the other; I have to let the human being die or the horse die, and I am bound to take my choice. But I would not vivisect a horse to save a human being.

18531. You think that is a sound position?—I think so. It is one that has been put to me, and which I have to answer just offhand. I think I would take the risk of the horse dying.

18532. Have you not rather put to us that in the case of vivisection there may be a conflict between the objects of science and the dictates of morals?—Yes, certainly there may be.

18533. Do I correctly understand from you that when that conflict occurs morals should be supreme?—Yes, morals should always be supreme over every thing; otherwise moral law has no meaning at all.

18534. I did not quite understand how Sir John Fletcher Moulton applied his balance of advantages theory in the case of man, because I see that in answer to Question 12735 he says:—"I unhesitatingly say that our right to experiment on men is extremely limited. In the first place, we have no right voluntarily to allow death to occur, even though the man would be willing to permit it." And he says:—"I do not think that we should have the right for experimental purposes to allow a man to infect himself under special circumstances, even though they would assist science with a disease like syphilis in order to add to knowledge." And again he says:—"I do not think that you ought to lessen in any way the feeling of sanctity of human life." What do you say with regard to that?—I do not think the sanctity of human life would be decreased by a man voluntarily submitting himself to experiment, but it certainly should not be done except under the strongest public guarantees that no wrong is being done, because evidently anything like that would be open to the gravest abuse if the public had no means of verifying what was done.

18535. Do you think it would be sound ethically to say that while the balance of advantages argument is applicable in the case of animals, it is inapplicable in the case of man?—No, that is not the case. If an animal could voluntarily give itself up to be experimented upon, I should see no objection to the animal doing it, but I object to an animal being made a vicarious sacrifice in the way that is done, and I object to a man being made a vicarious sacrifice in the same way. If the woman in Eugen Hahn's case, which I have mentioned already, had said:—"I allow you to experiment with my healthy breast," under due supervision, so that it could be verified by the public, I would see no harm in it.

18536. You draw a great distinction between cases in which the voluntary assent of the patient has been obtained and those in which it has not?—Certainly; voluntary self-sacrifice happens every day on the battlefield and elsewhere.

18537. As Lord Chief Justice Coleridge said in his judgment:—"To preserve one's life is, generally speaking, a duty; but it may be the plainest duty, the highest duty, to sacrifice it"?—Certainly; and as that is so in military matters, I see no reason why it should not be so also in scientific matters.

18538. We have had the name of Pirigoff mentioned before the Commission already. I see you make some reference to his work; do you desire to quote from it?—That is a quotation in my essay from the book of Dr. Smidovich.

18539. What does he say?—Pirogoff says, "In my younger days I was pitiless to suffering. One day, as I remember, this indifference to the agony of animals undergoing vivisection struck me with such force that, with my knife still in my hand, I involuntarily exclaimed, turning to the comrade who was assisting me, 'Why, at this rate one might cut a man's throat.' Yes, much can be said in favour of and against vivisection. There can be no doubt that it is an important aid to science . . . But science does not entirely fill the life of man; the enthusiasm of youth and the ripeness of manhood pass, and another period of life ensues, and with it an inner call for introspection; and it is then that the recollection of the violence used upon, the tortures inflicted on, and the sufferings caused to another creature commence to pull at one's heartstrings involuntarily. It seems to have been the same with the great Haller; so it was with me, I must confess; and in these later years I would never be able to bring myself to perform the same cruel experiments upon animals which at one time I carried out so zealously and with such nonchalance." You will observe that Dr. Smidovich says in reply to this, "None of this can be denied. *Mais que faire?* To renounce vivisection were to place the future of medicine in jeopardy, to condemn us doctors for ever to the uncertain and barren paths of clinical observation."

18540. Is it you or Dr. Smidovich who makes this further observation, "As one of the ablest thinkers on medical matters has said, it is not so much the amassing of further facts that is wanted as the ability to think out the logical consequences of the facts already within our knowledge"?—It was I who said

that, and the doctor referred to is Dr. Rablagiati, of Bradford.

18541. Is Dr. Smidovich alive?—I do not know at all. I think he is. This book of his has only recently been published.

18542. What is the date of it?—It is quite recent.

(*Sir William Church.*) It is 1904. It appears to be a young Russian doctor, and it is more a work of medical romance.

18543. (*Sir William Collins.*) Then you made some allusions to some experiments in regard to syphilis?—Yes.

18544. Are they vouched for by Smidovich?—Yes, if you turn to Chapter 8 in his book you will find a heap of experiments on human beings with syphilis, and the references to the medical and other papers in which they were confessed by their authors; and he points out that those experiments in venerology took place because animals could not be experimented upon for that purpose. Since that time, of course, as you all know, Professor Metschinkoff has succeeded in communicating syphilis to monkeys.

18545. Were those experiments made on human beings without their knowledge and consent?—So it appears in Dr. Smidovich's book; some of them with their consent, some of them not.

18546. Is there anything else that you desire to add in reference to the ethical evidence given by Sir John Fletcher Moulton?—I want to point out in the first place that his doctrine is hedonistic, but one-sided; he treats it only from the negative side—namely, salvation from pain. You see at Question 12704 that it is based on direct balancing. We have these words, "He would look at the inflicted pain, which would be to the bad side of the ledger." Those words evidently mean direct balancing. Now this view of direct balancing is argued against by me in detail in the *précis* respecting which I have just been answering, but, in reality, when you come to the after part of Lord Justice Fletcher Moulton's evidence the specific balancing really—or rather I should say the *quid pro quo*—is withdrawn, because at Question 12717 you find him arguing that you should experiment even without that *quid pro quo*—that it should be done as "a matter of pure curiosity," as Professor Starling says. Now this, as I have already said, is exactly the opposite to the view of Lord Chief Justice Coleridge. Lord Justice Fletcher Moulton proceeds by steps. He first says: "I say that if you look round the world the only way in which we can diminish pain is by human action." Well, I suppose it is; the only thing we can do in any direction is by human action; in fact, what we do and human action mean exactly the same thing. But I would point out that science never has any right to dictate human action. However far your generalisations may carry you in science, and to whatever height you may get in what you do in science, your conclusions will always be summed up in the indicative mood. When you pass from the indicative mood to the imperative you pass out of science altogether, and you come to ethics, and the categorical imperative must be decided by ethical considerations. Then Lord Justice Fletcher Moulton said at Question 12708, "I shall trust to show to this Commission that the pole star by which we steer is knowledge." Surely there was no need to demonstrate any such thing to this Commission or to any human being outside of Bedlam. We all agree, of course, that the pole star by which we steer is knowledge. But then he proceeds further on to say that no man who knows anything of science has any doubt that the right way to advance knowledge is by experiment, and then at Question 12716 you have the statement that "The more complex the subject is the more factors there are at work the more essential the experimental method is, and the most complex of all phenomena are those that relate to living beings." I believe I know something of logic, and this startles me as a great novelty, because the fact is really that the more complex phenomena become the more impracticable does experiment become, owing to intermixture of effects, and that is why experiments on animals or on human beings are to a large extent illusory; all sorts of contradictory conclusions are drawn from the same experiment performed by different individuals.

18547. (*Sir William Church.*) Do you make that statement from your own knowledge?—Yes; I am speaking as a logician.

18548. Not as an experimenter?—No; I am speaking as a logician. I am speaking as one who has for a great number of years studied the science of evidence, and who knows the conditions under which knowledge can be obtained. But it must not be supposed for that reason that I am in any way opposed to experiment; that is quite out of the question. As I have said earlier to-day, I am not opposed to experiment, even on animals, under limitations.

18549. (*Sir William Collins.*) Sir John Fletcher Moulton's point was largely that if medicine was a merely observational science it would crawl, but that if it were an experimental science it would advance rapidly?—Yes; I know he said that; he drew a distinction between an observational science and an experimental science, and he said, "You can take the whole range of the sciences, and I would challenge an opponent to name one in which advance, if it has been rapid and striking, has not been through experiment." I suppose it will be allowed that great advance has been made in astronomy and mathematics, and I suppose it would also be allowed that no experiment has helped in that advance.

18550. Did not Sir John Fletcher Moulton say that he thought the science of volcanoes was the only observational science that he knew?—Yes; I did not know that there was a science of volcanoes separate from seismology, but it is evident that that was chosen because it was a laggard piece of science. The fact is that the words "rapid and striking" afford a loophole of escape from any criticism. If they refer to modern medicine it begs the whole question, because those who hold the view that I am putting forward challenge, of course, the advances which have been made in this respect—that is to say, with regard to Pasteurism especially I certainly would challenge it. Then how does Lord Justice Fletcher Moulton prove his hedonistic theory? He gives us first a history of what was done with a dozen guinea-pigs.

18551. Have you been able to find out the origin or source of that experiment?—No; I have not.

18552. I did not get it from the Lord Justice himself?—I observed that you asked him what those experiments were, and that Lord Justice Fletcher Moulton said: "I cannot tell you the name of the doctor because it was not given me, but I heard it long ago in the early days of research in tuberculosis, and I believe it to be true." If that had been brought forward here, not by a Lord Justice, but, say, by some ignorant anti-vivisectionist, I venture to say that you would have laughed very much at such a piece of evidence being brought forward. Then Lord Justice Fletcher Moulton went on to say: "If it was not actually performed it would still hold good as a typical example." A typical example if not actually performed! That is a very curious sort of example. If it is a typical example at all, it is an example of the sort of reasoning that we are expected to accept in this matter. Then further on, at a suggestion of the Chairman, he said: "You may take it simply as an illustration." If we get no evidence that the thing occurred at all, what is the value of it as an illustration? Then there was another illustration that he gave—the illustration of rats in a ship. This is an analogical argument, and, as anybody knows with regard to analogy, the whole of the value of the argument depends upon the justice of the analogy, and there is no analogy here at all. The rats in the ship were not vivisected. I suppose that the passengers, if they were rational human beings, would have been quite willing that the rats should be destroyed, whether they were the cause of the plague in the ship or not; certainly I would, and I do not know any anti-vivisectionist who would take up the ground that Lord Justice Fletcher Moulton there puts forward as that of an objector.

18553. Does that complete all that you have to say with regard to the evidence of Sir John Fletcher Moulton?—Yes, I have looked through the remainder, and I can find nothing in Lord Justice Fletcher Moulton's argument, except what I have put forward—nothing at all. I consider that my essay appended to my *précis* is a complete answer to the main position that Lord Justice Fletcher Moulton took up.

18554. (*Sir John McFadyean.*) I understood you to say that in determining for oneself whether any action was right or wrong one should not endeavour to cast up any account of gain or loss, but should apply cer-

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tain mediate axioms. Will you tell us how you arrive at those mediate axioms?—You arrive at them either by induction or deduction from your main principle of ethics, that is to say, the promotion of happiness.

18555. Is that the greatest happiness of the greatest number?—No, the greatest happiness. I have nothing to do with the greatest number.

18556. The greatest happiness of whom?—Of all sentient beings.

18557. That is the greatest happiness of the greatest number, is it not?—No.

18558. Well, what is it?—It is the greatest happiness.

18559. The greatest happiness of all sentient beings?—Yes.

18560. But supposing that they cannot all be happy, is not that deciding what course of action will bestow the greatest happiness on the greatest number. Distinguish, please, between that and the utilitarian doctrine?—There is no distinction; it is the utilitarian doctrine.

18561. You accept that?—Yes.

18562. Do you only object to the hedonistic principle, because you think the account is cast up wrong?—I do not object to the hedonistic principle. If you read my *précis* you will see that I accept it.

18563. But I understood you to say a minute ago that we were not to apply the hedonistic principle, but mediate axioms?—No, what I object to is not the application of the hedonistic principle, but a certain mode of applying it—applying it in the mode of direct balance of advantages, of pleasure and pain. That is what I have endeavoured to show.

18564. Yes, I understand. You think that supposing there might be freedom from pain there are countervailing disadvantages?—No, if you look at the portion of my *précis* in which I have quoted from Austin you will see that Austin himself tells you exactly.

18565. Perhaps you can explain it yourself?—I would rather read what Austin says. He says, "According to that theory"—that is the hedonistic theory—"our conduct will conform to rules inferred from the tendencies of actions, but would not be determined by a direct resort to the principle of general utility. Utility would be the test of our conduct ultimately, but not immediately; the immediate test of the rules to which our conduct would conform, but not the immediate test of specific or individual actions. Our rules would be fashioned on utility; our conduct on our rules."

18566. So that if vivisectors believe that by vivisection they are promoting the happiness of sentient beings, they are acting logically and in a way that you would commend?—Certainly, so far as our judgment of the men is concerned; but, in judging the morality of acts, motive is irrelevant.

18567. That is all I want on that point. You were asked a question as to the rights of animals, and I understood you to say that these rights were limited by the fact that animals are irresponsible, and you classed them along with lunatics and children?—Yes, for some purposes.

18568. It appeared to me that that was an unfortunate classification, because the fact that the individual is a child or lunatic extends his rights as regards protection from pain. Are the rights of animals extended with regard to protection from pain, owing to their being irresponsible, more or less?—I deny your premise. I deny that the rights of children and idiots are extended.

18569. Well, in what respects are they affected? I am merely quoting, so far as I can, what you said yourself; you said that they were limited?—Yes, they are limited; they are limited by being interfered with. A child is not allowed to do exactly what it likes in the same way as an adult human being is.

18570. That is beside the question. We are discussing pain; we are not concerned with limiting the actions of animals?—You asked me a question with regard to the limitation of their rights, and I am answering that question.

18571. I will put it to you again. You were asked with regard to the rights of animals, not with regard to their limitation, and you said that their rights were limited by the fact that they were irresponsible?—Yes.

18572. How does that bear on the question of causing pain to animals? Does the fact that they are irresponsible limit their rights as to freedom from pain caused by human action?—Yes, it does.

18573. That is what I thought?—A moderate chastisement to a horse would be perfectly justifiable.

18574. But is it more or less justifiable owing to the fact that the horse is irresponsible?—It is more justifiable on account of the horse being irresponsible, and the horse is placed under restraints which would not be justifiable if it were responsible as a human being is.

18575. But that is only pain inflicted for that individual horse's good?—I do not know that it is for that individual horse's good. If I drove a horse and gave it a slight flick with the whip, it would not be altogether for the horse's good, it would be for my good in getting forward.

18576. So that that is a case you would think in which it is quite permissible to cause pain to a horse?—Yes, a very slight amount of pain.

18577. Where do you draw the line; what mediate axiom enables you to draw the line between permissible pain and unpermissible pain, which is not for the animal's good?—You cannot do it in practice.

18578. I am content with that answer?—You cannot do it in practice, and you will find that under the Cruelty to Animals Act (Armstrong *v.* Mitchell, 88 L.T. 870), the question of cruelty is reserved as a question for the judges; that is to say, it must be decided in each case by men who have got some empirical idea of what amount of chastisement shall be allowable and what shall not. In all those cases it is so. Nature is not made up in separate compartments for us.

18579. May it be decided in the same way how much pain it is permissible to inflict on animals with a view to extending useful human knowledge?—Certainly.

18580. May I ask whether you would consider it a greater crime to kill a man or to cause him a considerable amount of non-fatal pain?—Certainly, I should say to kill him.

18581. Does that apply to animals also?—No.

18582. What is the mediate axiom which enables you to discriminate in these cases?—With regard to animals, the killing is a necessity of our life upon earth. No man can help killing animals; we could not help it, as I said previously, if we were vegetarians. The ploughshare, when it goes through the ground to get the corn to make my bread, kills, vivisects, if you like, innumerable earthworms; do what we will, we cannot avoid killing animals, but we can avoid torturing them, and it is avoidable evil that we should avoid.

18583. I am afraid I do not quite follow that. What is the evidence that man must everywhere kill animals?—Can you tell me how to avoid it?

18584. Certainly; I could suggest that if men ceased to interfere with the domesticated animals they probably would die out, and it is only the domesticated animals that I am dealing with?—Yes.

18585. Then, supposing that they had died out here, do you suggest that it would be impossible to maintain existence without them?—No, not without them, but you would be obliged to kill other animals in order to live.

18586. Why? Are you a vegetarian?—No, but that does not matter to the point you are asking me about.

18587. Except that we have had people here who are vegetarians, and who are maintaining their existence well enough?—Never mind; those persons who are vegetarians, as I have explained just now, cannot obtain their bread without killing earthworms, and they cannot obtain their cabbages without killing caterpillars.

18588. So that you freely admit that man is entitled to cause a certain amount of pain to animals in protecting himself, for instance, and in feeding himself?—Yes.

18589. In some of which cases he may be justified in killing them, although his life is not threatened but his comfort simply is threatened?—Yes.

18590. What is the mediate axiom that justifies a man in killing an animal and causing pain in killing it to prevent discomfort, but yet forbids his causing it any pain whatever in order to extend useful human knowledge?—I have never said that he may not cause a certain amount of pain to animals in order to extend useful human knowledge.

18591. If you admit that then I am quite content?—Very likely you and I would draw the line at very different places.

18592. Yes, I think that is very likely. Is it justifiable to cause pain to one animal in order to secure benefit to another animal?—It might be so. If I could tie up my dog, and cause it a certain amount of discomfort by being in its kennel, in order that another dog should be free from annoyance by it, I should feel perfectly justified in doing that.

18593. I will put a concrete case to you. Supposing that one had a lot of cattle in which a serious disease had broken out, and one had knowledge amounting to certainty, that by taking blood from one of those animals and injecting it into the others, one could save their lives, and save them a certain amount of suffering too (I ask you to assume that that is so), would a man be justified in doing that?—I think so, so far as I can see.

18594. Would you explain what is the difference in principle between that and infecting an animal in order to secure from it some material which would be valuable for treating other animals?—In the first place, there is no absolute certainty with regard to that. You have given me absolute certainty with regard to the other case. There is no absolute certainty in this.

18595. But I ask you to assume in this case also that there is absolute certainty that by infecting an animal which at present is healthy, you can afterwards obtain from it material which would be valuable for saving the lives of other animals. Assuming that to be certain, would it be permissible?—I do not know all the circumstances. I could not decide that offhand.

18596. Then why did you decide the other one so easily?—It appeared to me that that is a perfectly easy case, so far as I can see. You must recollect that it is by no means an easy matter to decide offhand at a Board like this all the cases of casuistry that you might possibly put to me.

18597. Apparently not?—I am quite willing to acknowledge it. I do not in the least deny it.

18598. Do you mean that it is difficult to decide on the spur of the moment, whether it would not upset a conclusion previously arrived at?—I do not mean that in the least. If, instead of asking me these cases now, you were to write them down and give me a little time to think out each case, then I should be able to give you a much better answer.

18599. If I may I will re-state it, and make it absolutely concrete?—I mean to say this, if you take the greatest lawyer in the land and you put him down at this table and then ply him with all sorts of difficult questions of the application of the law he will not be able to answer you offhand. Very often judges take days to consider their verdict upon these matters, and I should say the same with regard to these matters of casuistry.

19600. Would you like me to understand that the question is too complicated for you to answer offhand?—The question is one that requires consideration.

18601. May I call your attention to this statement in your *précis*: "If there is any validity in the vivisector's logic it cannot stay at the lower animals." That is to say, the logical consequence of performing an experiment on the lower animals is to go on to experiment with man?—No.

18602. Well, what does the statement mean?—The logical consequence of vivisection—that is to say, the direct balancing—is that human beings, as well as animals, will be vivisected if there is a direct balance of advantage.

18603. I beg to call your attention to the fact that you yourself suggested it to be logical that they should go on to vivisect criminals and lunatics?—Yes. I do say that.

18604. Supposing that I point out where the flaw is in this reasoning?—I should be very glad.

18605. If there is any validity in the logic of those who justify the killing of animals for human food, you cannot stay at the lower animals. Then why not kill idiots, lunatics, and babes for human food? It seems to me to be just as logical to stop at the one as at the other?—I do not think it is at all logical; at all events, it does not point out any flaw in my reasoning.

18606. Is it not quite logical for vivisectors to start with an axiom which says that one thing may be permissible with regard to one's actions on the lower animals, but not permissible on man?—One may quite do that. But I say that, given hedonistic direct balancing, you cannot stop at the lower animals logically, and I will go further and say that vivisectors have not stopped at the lower animals. If you take Dr. Smidovich's book, the whole of Chapter 8 is full of experiments in which vivisectors have not stopped at the lower animals, and they have not stopped with regard to cruelty.

18607. Does your objection to causing pain to animals extend to the whole of the animal kingdom?—Of course it varies with regard to the sensibility of the animal. One would not say the same with regard to a black beetle as one would with a dog. There is a difference in sensibility. When the question is one of cruelty the sensibility of the animal must, of course, be taken into consideration.

18608. What do you mean by cruelty?—I have already defined it as serious pain inflicted for an object which cannot be justified.

18609. But as applied to pain caused by vivisection it is begging the question, is it not. To call the pain caused in vivisection cruelty is begging the question which is in dispute?—I never called all the pain inflicted by vivisection cruelty.

18610. Which part of it do you call cruelty?—I say that a great part of the pain inflicted by vivisection is cruelty, and if I had come here to give evidence on that portion of the matter, which I have not done——

18611. Then please do not?—I could give you plenty of proof of it.

18612. That is like saying that you have not come here to give it, but you are ready to give it?—Yes.

18613. But you would not offer to give expert evidence on that?—No; I am not an expert on that matter. I should have to give you the evidence of others.

18614. What do you think about the right of man to cause pain in field sports?—I have noticed that questions of this kind have been put to other witnesses—questions designed to show that there are grievous inflictions of pain on the lower animals, in connection with the rearing of animals designed for food, with the slaughter of those animals, with what is called "sport"; and that those who desire to prevent vivisectionist cruelty are inconsistent in not proceeding at one and the same time against all these forms of cruelty, that by eating ordinary butcher's meat they are *participes criminis* in the castration of animals and the horrors of the slaughter-house. I wonder that the argument was not pushed a step farther, and that it was not pointed out to vegetarians that they obtain their bread by the painful extermination of animals. When the ploughshare—which is an instrument used in the procuring of the corn from which bread is made—passes through the ground it cuts up alive innumerable earthworms; and rabbits and other animals which compete with us for the possession of our vegetable food are killed by methods which undoubtedly involve serious pain. Under the conditions of human existence I do not see how we could avoid killing and the infliction of some pain. In so far as, in the matters referred to, the infliction of pain is avoidable, it ought to be avoided. But I decline to pursue the matter farther. I am reminded of the case of the boy who was told that he might have all the nuts in a certain jar, but that he must take them out with one handful. He found, however, that when he took them in his hand his fist was too large to get through the opening of the jar. I would be glad to abolish all forms of cruelty at a single blow; but I would find the matter in hand too large, and would be unable to get my fist through the opening of the political jar. But why commence with vivisection? Because vivisection sanctifies cruelty by investing it in the garb of science, by making knowledge the primary object of pursuit. It is not only hideously immoral in practice, but sets up an ethical theory of the primacy of scientific curiosity, which is ruinous to the ethical ideal, debauching to the intellect, and demoralising to mankind.

18615. Do I gather from that answer that you would approve of field sports, in so far as they are painful, being put down by law?—No; I must decline to go into that.

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18616. Do you mean that you decline to answer?—Yes.

18617. Why do you decline to answer?—That is my answer—simply because I will not go into any further matter now. I am attacking vivisection. I do not want my hand too full to get through the opening in the jar.

18618. But that is not the question. I think one has a perfect right to put questions to find out how far you are consistent?—I do not think you will ever find out that I am not; you may try that as much as you like.

18619. But if you do not answer we cannot find out?—I have given you my answer.

18620. No, you have not, excuse me. I asked you whether you would approve of field sport being put down by legislation in so far as it is painful?—That does not appear to me to be the question.

18621. You decline to answer?—Yes; further than I have already answered.

18622. Do you justify the killing of animals in order to convert their hides into leather?—Yes.

18623. You think that is justifiable?—Yes.

18624. Could substitutes for leather not be found, perhaps to man's discomfort, but still, you would not represent it as absolutely essential that men should be shod with leather?—No, I say that if by progress of dietetics and economic progress of the kind you show that we can get rid of killing animals for food and leather and other things altogether, no one would rejoice more than I should, and I think that the future may bring us to it.

18625. But is any question of right or wrong involved in rearing animals in order to kill them to provide us with shoe leather?—I do not think the thing is carried on. I never heard of it being done.

18626. Do you think that the value of an ox's hide does not enter at all into the agriculturist's calculations?—It does, but the ox is not reared for that only.

18627. It is reared in part for this?—It enters into the consideration, but it is reared for its flesh and its horns, and its hide, and its hair; it is reared for the whole. It would be putting a part for the whole to say that it was reared for the purpose of its hide.

18628. I said, was it reared in part for the purpose of its hide?—Yes.

18629. Is there any ethical side to that question at all?—I do not think that there is, as we are situated now.

(*Sir John McFadyean here took the Chair.*)

18630. (*Sir Mackenzie Chalmers.*) In reference to Professor Metschinkoff's lecture, were you quoting from the lecture, or were you giving us a summary of what you thought it meant?—It was a quotation. I have the "Journal of Preventive Medicine" here for you to verify it, if you like.

18631. I refer especially to a passage which I did not understand to be a quotation about the inoculation of syphilis?—That is not a quotation.

18632. What is that?—It is a description of a passage in Professor Metschinkoff's book, evidently too short to be a quotation.

18633. I read the lecture myself, and I should not have abstracted it in the same way?—Perhaps not, but I contend for the accuracy of my abstract, and I shall be quite willing to append to my evidence the passage of which it is an abstract, in order that readers may see whether it is a fair abstract or not.

18634. That perhaps might be useful.* As regards the case to which you have referred of inoculation of cancer by somebody named Eugen Hahn, can you tell us who Eugen Hahn was, and where he performed this experiment?— You will see it in Dr. Smidovich's book.

18635. Can you tell us the date and where it was performed?—You will find it on page 125. "In March, 1887, a woman, suffering from cancer of the mammary gland, applied to the surgeon, Eugen Hahn, of Berlin. The performance of an operation was impossible. 'Not wishing to divulge before the patient the hopelessness of her condition by declining to operate upon her, and so as to relieve and reassure her by the psychical illusion of having performed the operation,' Dr. Hahn removed a portion of the tumour of the patient's diseased breast, and transplanted it into the other healthy one" (*handing in the book*).

18636. The object of this experiment seems to have been to prevent the poor woman understanding that her case was hopeless?—No, that was not the object of putting a portion of the cancerous material into the healthy breast.

18637. I am only reading the words of the book: "'Not wishing to divulge before the patient the hopelessness of her condition by declining to operate upon her, and so as to relieve and reassure her by the psychical illusion of having performed the operation,' Dr. Hahn removed a portion of the tumour of the patient's diseased breast, and transplanted it into the other healthy one; the inoculation was successful." It conveys that idea to me?—But it does not convey that idea to me, and if you notice, the book itself from which this is described is called "Ueber Transplantation der Carcin"; that is to say, the book itself is on the transplantation of cancer, and this was an experiment in the transplantation of cancer.

18638. (*Sir John McFadyean.*) Then do you maintain that that is a false suggestion?—No, I do not.

18639. How do you reconcile that with your interpretation of it?—What do you mean?

18640. This represents that the main purpose of the experiment was to reassure the patient; is that false?—That was a reason for performing some operation on her, but it is not the reason for the transplantation of a portion of the cancer from the one breast to the other breast.

18641. (*Sir Mackenzie Chalmers.*) The passage speaks for itself?—I do not think it does in the sense you mean.

18642. Then we cannot get any further. This book was translated in 1904?—It was published in 1904. I do not know when it was translated; it might have been translated in 1903.

18643. I see that the author of the book "The Confessions of a Physician" seems to have two names?—He published the book under the name of Veresaeff, but his real name is Smidovich.

18644. Is anything known about him except that he was a young Russian doctor?—I do not know very much about him. I know that his book has been translated into various European languages, and from the reading of the book, I judge that although he is a very strong and passionate vivisectionist, at all events his truthfulness has been unaffected, and it gives me the idea of a man who speaks very frankly indeed respecting what he has done. That is the idea the book gives me, certainly.

18645. Have we any reason for knowing that any of his statements were personal experiences, and not a sort of medical romance?—I do not think that anybody who has read that book would say that it was a romance; and the reviews that have taken place (you will find one of them, from the *Times*, appended inside the cover of my book), are of a kind to show that it is a serious book.

* The passage is as follows:—"Only to those persons who are exposed to the greatest risk of acquiring human syphilis could one propose vaccination with an attenuated virus. In point of fact, the hypothesis that this vaccine might provoke more serious complications than true human syphilis is not admissible; at any rate, it is not based on any scientific data. The persons liable to benefit from vaccination would at first have to be selected from among the beginners in prostitution. It is an established fact that these women only in exceptional cases escape syphilis. As almost all of them are, therefore, most liable to acquire this disease, vaccination would be an advantage for them as well as for the men having intercourse with them; but in practice the carrying out of this proposal meets with serious difficulties, since the beginners in prostitution are very young, and almost always minors."

FORTY-FIRST DAY.

Wednesday, 4th December 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, M.D., LL.D.
Captain C. BIGHAM, C.M.G. (*Secretary*)

Mr. W. E. DIXON, M.A., M.D., called in; and Examined.

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18646. (*Chairman.*) You are, I believe, Professor of Materia Medica and Pharmacology at King's College, London?—Yes.

18647. And also Assistant to the Downing Professor of Medicine in the University of Cambridge?—Yes.

18648. And you come here as representative of the Therapeutical Section of the Royal Society of Medicine?—Yes.

18649.—You have given us several heads under which you propose to give your evidence; we will just take them in order, please. First of all, you propose to deal with the advantages which Pharmacology has derived from experiments?—Yes.

18650. Will you give us your views upon that, please?—Perhaps I might say, first, that I do not want to re-duplicate evidence unnecessarily, and that I understood subsequently to sending in my *précis* that I was specially required to give evidence on anæsthetics, so that if you think this first head is unnecessary, I am perfectly willing to eliminate it.

18651. Speaking for myself, the first of your heads seems to be one on which we have had an adequate amount of evidence, and really you are only saying ditto to witnesses whom we have had before, whose views put the matter fully before us?—That is true, I understand.

18652. I think if you will come to anæsthetics, in which you have had considerable experience, there is a good deal in what you say there that we have heard before, but still there has been so much said about anæsthetics, and especially about the efficiency of particular anæsthetics, or rather of different drugs which are used as anæsthetics to produce complete anæsthesia, that perhaps you will give us now in your evidence about anæsthetics and narcotics?—The first point that I wish to make perfectly clear is that there is no essential difference between an anæsthetic, a narcotic, and a hypnotic. There are two classes of narcotics, the one I am not dealing with. There is the class of drugs acting on the brain, which we call specific; I refer to morphin and hyoscin; I am not dealing with those; I am dealing with those drugs of the alcohol series which act on the brain. That is a very large group including alcohol, chloroform, ether, paraldehyde, chloral, urethane, and many more. These drugs all have fundamentally the same action on the brain, they act in precisely the same way, and their differences are determined entirely by differences in the rate of absorption and excretion. For instance, chloroform and ether are absorbed extraordinarily rapidly, because we administer them by inhalation, and they are absorbed from the huge area of lung capillaries, so that they are taken into the blood almost immediately, and their effects are produced almost at once. And in the same way, when you stop administering these drugs they are excreted equally rapidly, so that the effect is a transient one; as soon as you have stopped, or very soon after you have stopped, administering a volatile anæsthetic, its action ceases. If you administer one of the solid substances like chloral, or urethane, it requires a certain length of time to act; it is not absorbed quickly. Chloral, by the mouth, takes from half an hour to three hours getting into the circulation, and it is being absorbed all that time. Supposing it is taken by the mouth, it passes slowly from the alimentary canal, and when it reaches the blood it is not excreted rapidly, so that you get chloral existing in the blood over a very prolonged period. Perhaps I can make my point quite clear if I say that if chloroform had been a solid substance (of course, it is not), then it would have had exactly the same action as chloral, and if chloral had been a liquid substance it would have had exactly the same action as chloroform. With animals, of course, I personally prefer in many experiments to use urethane (urethane I use mostly myself) to chloroform. First of all, perhaps you give the animal chloroform to get it under quickly; then you give the urethane either intravenously or subcutaneously, or into the peritonial cavity—it does not matter which—and it gets into the blood and produces anæsthesia, which lasts from six to eight hours; and then, of course, the total effect does not pass off probably for 24 hours afterwards.

18653. But with a dose of that kind the animal would survive if it were not killed by some other cause?—Yes, it would survive. Perhaps I may make my point quite clear if I may be allowed to read what is the usual teaching to students at the present time, from a page in my Text-Book on Pharmacology. I say there on page 50:—"Narcotics or hypnotics administered in large doses give rise to complete anæsthesia. Urethane, for example, when given to a rabbit in doses of one or two grains behaves as a mild hypnotic, but if an injection of 100 grains is made, it forms an excellent and complete anæsthetic. Such an anæsthetic would not be suitable for man on account of the prolonged action of the drug, about 24 hours, and on account of the difficulty of just gauging the dose, so that whilst anæsthesia is complete, the medulla retains its activity." Of course, the medulla retains its activity, or the respiration would stop. In the case of an animal it does not matter

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a bit if respiration does stop; we simply do artificial respiration, everything is ready. In a great number of experiments you do give so much anæsthetic that the medulla gives out, and then you simply perform artificial respiration by means of the pump, which is always handy. The term anæsthetic has come to be used in a limited sense for drugs which are very rapidly absorbed and excreted, *i.e.*, the volatile drugs, chloroform, ether, etc. With such the effect only lasts so long as the administration is continued. They are quite unsuitable as hypnotics because they act for too short a time." There is no fundamental difference between the action of chloroform, ether, ethyl-chloride, and ethyl-bromide, on the one hand, and chloral, urethane, chloretone-paraldehyde, etc., on the other.

18654. You have some evidence at the foot of page 5 of your proof as to the doses for rabbits and man in particular; would you just give us that?—The dose of chloral per os or per rectum necessary to produce complete anaethesia in rabbits is one gramme per kilo of body weight; that would mean for a healthy man weighing 70 kilogrammes a dose of over two ounces; whereas the common hypnotic dose for a man is only 10 to 30 grains, or about 1-70th part of this quantity. In dogs and cats smaller doses are given relatively to the body weight to produce complete anæsthesia. In man chloral is also employed as an anæsthetic. Perhaps I ought not to have put it in this way. It is very little use at the present time for obvious reasons, although I give an example where a Frenchman has used it as an anæsthetic for children in 10,000 cases. He gives it in 3 to 10 gramme doses.

18655. (*Sir Mackenzie Chalmers.*) As an anæsthetic?—Yes, as a complete anæsthetic. It causes, as he says, complete anæsthesia lasting from three to six hours.

18656. (*Chairman.*) We used not to go into that. It is not generally used for men?—No, it is not generally used for men; it is dangerous.

18657. (*Sir Mackenzie Chalmers.*) Dangerous to life?—It is dangerous because it might make the medulla give out; and moreover we do not want to keep the men under it in a trivial operation for six or eight hours.

18658. (*Chairman.*) We are not examining into operations on human beings except in so far as they throw light on the question of operations on animals. But is chloral used for animals generally as an anæsthetic?—Yes, chloral is used for animals.

18659. You were going into the experiments of Mr. Bouchut, speaking of what he could give to a child?—Yes.

18660. You can take it rather short?—Then I give the doses necessary in cases of urethane. The dose necessary to anæsthetise a rabbit is 1½ grammes per kilo body weight, and for a dog or a cat about one gramme per kilo body weight. According to this reckoning the dose to produce anæsthesia in man would be about 70 grammes or 2 ozs., but for reasons already mentioned, it is not employed for that purpose.

18661. Then the next drug you wish to mention is morphin?—Morphin or morphia as it is sometimes called, is of course a little different in its action; it belongs to an entirely different group of drugs. These drugs which I have been speaking of up to now act on all the nerve cells in the brain in much the same way, except that in every case they paralyse the sensory nerve cells before the motor—that is a most important point—so that you can have complete anæsthesia whilst you can still get movement; that is to say, on stimulating an afferent nerve you can get movement when the sensory cells are all obliterated from the brain.

18662. Are you speaking of all drugs now?—I am speaking now of the drugs that I have previously mentioned, the alcohol group of hypnotics.

18663. (*Colonel Lockwood.*) Of which morphia is one?—No, not morphia; drugs other than morphia, those alcohol drugs which act on all nerve cells, motor and sensory, but sensory first. In the case of morphin you have a difference. Morphin is what we call a specific drug; it is not allied to other drugs at all. Morphin acts quite differently; it is quite a distinct thing altogether, and it only attacks the sensory nerve cells; so that if you produce complete anæsthesia from morphin you can still get, indeed you do get, very distinct reflexes. You can administer morphin to man in anæsthetic doses, and you can still—I have frequently done it—obtain quite distinct reflexes, although when the man has subsequently recovered he has never felt anything at all.

18664. (*Chairman.*) When you say that you can obtain reflexes, what exactly do you mean?—I mean that if you simply do something quite as trivial as stroking the abdomen you will produce motion; but if you apply what to an ordinary man is a very painful stimulus such as pressing the supra orbital nerve with your thumb you get no effect at all, not even a reflex, and as I say subsequently the man knows nothing about it. So that we do not use (or very rarely) morphin alone as an anæsthetic, not because it is not one, but because it leaves the motor cells active, and the animal is reflex.

18665. You mean that it would disturb the operation by reflex action?—Yes; by movements. The action of morphin on dogs is precisely the same as that on men.

18666. Is it used for dogs?—Nearly always.

18667. Alone?—No; not alone, because it is necessary to give something to cut out these movements. I should have gone on later to say that it is not used for man for that reason. But it is quite commonly used for man now in Germany, when it is given with some drug which also paralyses the motor cells as well as the sensory, and so in Germany, and to a limited extent in England, it is given with hyoscin (the so-called morphin scopolamine narcosis) for operations where chloroform is deemed unsuitable.

18668. In that combination it both produces complete anæsthesia and prevents reflex movements?—Yes. Then there are two other minor points that I should like to draw attention to. The first is the fact that when surgical anæsthesia is once produced the amount of anæsthetic necessary to continue that anæsthesia is very small. Thus, if it is necessary to give 1·5 per cent. chloroform to anæsthetise a dog, the anæsthesia may be continued indefinitely by the subsequent administration of 0·2 or 0·3 per cent., or even a smaller quantity. Of course, that is perfectly well known to every anæsthetist; it is hardly necessary, really, to draw attention to it.

18669. How are you guided as to when you administer the additional doses—by the appearance of the patient or the condition?—The test which is commonly employed is what we call the reflex test; you simply put your finger on the cornea, and if there is a reflex then you know that the motor cells are beginning to recover their activity.

18670. I was asking the question rather with reference to a case, of which I understand there are some, in which curare is given, as well as a complete anæsthetic. How would you know, in that case, when to renew your dose?—Do you mean a volatile anæsthetic now?

18671. Like the one you have been describing—any anæsthetic which would be sufficient to produce complete anæsthesia, and a proper one to administer to a dog. In some cases we have been told that curare is administered in cases where great stillness is necessary?—May I leave that point? I have put a note on curare at the end.

18672. Very well?—The second point is the great value of giving morphin before administering chloroform. This combination produces a very perfect anæsthesia, the quantity of chloroform necessary being much smaller than would be required if no morphin had been given. These facts have been commented on by many, and are universally recognised, so that I will only refer to the observation of Bernard on dogs, and of Nussbaum, of Munich, Rigault and Sarasin, of Strasburg, and Labbé and Curon on men. Then I refer again to the paralysis of the sensory cells; all anæsthetics paralyse the sensory cells before the motor.

18673. Then you come to curare?—What I would like to say about curare is, first of all, to say briefly what its action is. It is not an isolated drug, having this peculiar action all by itself. There are lots of other drugs having the same type of action. It is, perhaps, the one which has the most characteristic action on the motor nerve endings, but there would be no difficulty in picking out a whole host of others that do the same. All this group of drugs paralyses the nerve cells, the brain, and every one of them paralyses the motor nerve endings, and they may all cause convul-

sions by acting on the spinal cord. Those three facts apply to all of them. Some members of the group have one action well defined, and others another. Thus, nicotine first paralyses the nerve cells, and later the motor nerve endings, whilst hemlock (conium) paralyses the nerve cells and nerve endings almost together. Curare first paralyses the motor nerve endings and later the nerve cells, whilst hemlock (conium)—the poison that killed Socrates—paralyses the nerve cells and nerve endings, roughly, about the same time. I picked those three examples from a group to show the various stages, how one produces its action at one time and another at another time. I mention this to show that even curare given alone is a complete anæsthetic, if enough is given, although we in England, conducting experiments, assume that curare has no action on the nerve cells, and always give enough of some other anæsthetic to completely paralyse the brain. Of course Claude Bernard really started this idea that curare acts on the motor nerve endings and not on the sensory nerve endings or cells. But Claude Bernard's experiments only apply to the spinal cord; he did not prove anything else at all; all that he showed was that the sensory cells in the spinal cord are not paralysed by curare. That was all his experiments meant. None of these other drugs paralyse the sensory cells in the spinal cord in moderate doses. Chloroform, except in the largest doses, does not paralyse the sensory cells in the spinal cord.

18674. But what we are told is that curare is consistent with the animal feelng pain, although it is unable to move, and that that is not the case with chloroform: that if the animal is reduced to apparent insensibility under chloroform it does not feel?—My point is that these ideas about curare originated with Claude Bernard, whose experiments simply showed that the sensory cells in the cord were not paralysed. They showed nothing else at all. Nevertheless, we believe that small doses of curare would paralyse the motor nerve endings before the brain cells were paralysed, but that large doses of curare will paralyse the whole of the brain like chloroform.

18675. But (reducing it to purely lay language) does that mean that the animal would cease to feel pain with a large dose of curare?—Absolutely.

18676. But that with a small dose it might continue to feel pain, though unable to express it by action?—Yes; it would, I think. That is the point I just wished to bring out there—that curare was a member of a certain definite group of drugs.

18677. There is one matter with reference to anæsthetics that I would like to ask you about as it is not in your *précis*. When you are operating on animals, do you have an anæsthetist to assist you, somebody who is skilled, I mean, in the use of anæsthetics?—No, I usually have a box; I put the animal in the box, and pour in chloroform, in the case of cats, and ether in the case of rabbits, and in the case of dogs one has to be more careful, of course, and I give the chloroform by hand very slowly. The whole point about anæsthetising a dog is to go slowly; dogs very easily die of chloroform, but if one goes sufficiently slowly they never die. I never kill a dog by any chance whilst administering anæsthetics to them now.

18678. So far as regards giving an anæsthetic then you do give the animal the best chance; that is to say, you are the most skilled person in your laboratory and you are the person who administers it and reduces the animal to anæsthesia?—Yes.

18679. But as regards the renewal doses which you say may be very small and should be administered at the proper time, of course, whenever necessary, who looks after that?—Usually there are no renewal doses. The animal is placed on the table, and I give it the urethane or chloral (usually, almost always, urethane), and then the animal is anæsthetised and would remain deeply anæsthetised for very much longer, of course, than is required.

18680. I think you told us for six or eight hours just now?—At least that. I have never tried it longer.

18681. (*Mr. Tomkinson.*) How do you give the urethane first?—Inject it into the blood.

18682. (*Chairman.*) And that is what you usually do. And how do you give chloral?—Chloral I do not use much, but I generally give it into the peritonial cavity. Chloral, like chloroform, depresses the heart.

18683. Does it produce the same effect as urethane, complete anæsthesia lasting a long time?—Yes, exactly the same.

18684. So that in almost all operations, I understand that the one dose would be enough, and would not require renewal?—The one dose would be enough.

18685. Then supposing it is an operation in which for one reason or another, the dose requires to be renewed either because of the nature of the drug used for anæsthetising, or from the excessive length of the experiment, or whatever it may be, who does that?—I should do it always.

18686. Would you if you were performing the operation at the moment be able to watch better than or as well as anyone else?—I should watch it the whole time; that is what I should be doing. You watch the experiment the whole time. The preparation of the experiment perhaps takes half an hour for an average experiment. Then when it is prepared your duty is simply to watch and observe.

18687. Supposing, for example, that the managers of the hospital or laboratory in which you were working were to say that they were prepared to give you a skilled anæsthetist to assist you in these operations, would you reject it as unnecessary?—Absolutely. There is no anæsthetist required at all. We are not giving anæsthetics—the anæsthetic has been given. There is one other point about curare which I think is extremely important, and that is that curare is excreted with extraordinary rapidity; so that if you give an animal curare it only remains curarised for a very short time, so quickly is curare excreted that you cannot poison a man by the mouth, because as fast as the drug gets from his stomach into his blood it goes out by the kidneys. I myself have taken a large dose of curare, and have never had any symptoms at all, yet if I had had a cut on my skin and let the curare get in, the curare would, I have no doubt, have paralysed or depressed my motor nerve endings. The explanation of that is that the curare is simply excreted, we know this absolutely, very rapidly. The importance of that is that if you give an animal an anæsthetic plus curare, the curare disappears very quickly, the anæsthesia still existing.

18688. If you give it through the mouth?—No, supposing that I injected curare into a vein, the animal remains curarised, say, for half an hour to three-quarters of an hour. If that animal has got urethane plus curare, the curare action is over in half an hour or three-quarters of an hour, while the anæsthetic action of the urethane continues for eight hours at the least.

18689. But supposing that the animal were not completely anæsthetised, the most painful part of the operation would be without anæsthetics?—But you anæsthetise the animal before you give the curare.

18690. But it is not everybody who is satisfied that the animals are completely anæsthetised, and one has to deal with that. It is suggested that animals are not completely anæsthetised, and at the same time are given curare. In that case, the fact that the curare was worked off in half-an-hour would not be so material because the animal would have suffered the greater part of the pain in that half-hour?—Yes, the only condition in which that could apply would be when giving volatile anæsthetics. Supposing that you were administering chloroform with curare, then it might be said that you are not giving enough chloroform. But you can see whether you are giving enough by looking at the blood-pressure. The blood-pressure is just as delicate an indication of the action of chloroform as the reflexes.

18691. (*Mr. Tomkinson.*) For how long do you take the blood-pressure?—It is always taken.

18692. During the whole operation?—During the whole operation, at least during the whole of my operations. I am speaking of my own operations.

18693. (*Chairman.*) By what is it taken?—By a cannula connected with an artery to what we speak of as a mercurial manometer. It is being recorded automatically the whole time.

18694. (*Mr. Tomkinson.*) Is that inserted under anæsthetics?—Yes, the animal is given an anæsthetic to begin with, and it never recovers from it.

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18695. By nerve endings I suppose you mean what we call nerves?—The nerves are white fibres, and they end in either glands or muscles, or something or other, and the drug can act on various positions in various places, and the nerve endings are just one place. A nerve ending is not a muscle, and it is not a nerve; it is something in between the two.

18696. (*Chairman.*) Do you often use curare yourself?—No, I use it very rarely.

18697. In what cases do you use it?—I should use it where vaso-motor nerves, vessel nerves, were concerned where we require to investigate the action of a certain kind of nerves which go to the vessels.

18698. Blood vessels?—Yes, because those vaso-motor nerves, the vessel nerves, are mixed up with the motor nerves, and you must separate the one from the other. You can cut out the nerves to the voluntary muscles by curare and leave the nerves to the blood vessels.

18699. How is it that you can do that under curare and not without curare?—That is what we call the specific action; the drug has just got that specific action; it just attacks those particular nerve endings and leaves the others.

18700. That does not explain it to my mind clearly?—Do you wish to know why the curare goes for those particular nerve endings?

18701. I want to know why curare renders it easier—what is the state of things produced by curare which makes you able to perform an operation when the animal has curare and not able to do it when it has not?—Supposing that I wish to stimulate a nerve, I excite the nerve in one way or another, and I should get, if the muscles were intact, simply contraction of muscles, and I could not observe my blood vessels at all. But if I have eliminated the muscle effect I have only got the vascular effect left.

18702. Those are the cases in which you would use curare?—Yes. It is very rare.

18703. (*Sir Mackenzie Chalmers.*) How often in the last two years have you actually used curare yourself?—I should say three or four times, but I could not give it to you just offhand. It is not common at all; I mean that it is not once a week or once a fortnight—it is nothing like that.

18704. Two or three times in the last two years?—Perhaps more than that—perhaps four or five times. I could let you know, of course.

18705. (*Chairman.*) If you were forbidden to use curare it would only operate upon you, at any rate, to that extent. You would be prevented from performing some two or three operations a year?—Yes, but they would be operations of the greatest importance. It might spoil a whole research.

18706. I am not offering any opinion about it. I am only wanting to know the extent of it. Have you been always satisfied that the animal has not felt pain under curare?—Always—absolutely.

18707. As satisfied as if it had not had curare given it?—Yes, absolutely.

18708. Is that all that you have to say about curare?—Yes.

18709. Then as to your head of modern work, I think really we have had all this before?—You have had some of it, but I would just like to mention one thing, if I may; and, to show you the great advantage of the pharmacological laboratory, I would just like to mention the investigation into West African boxwood. Up to Lancashire, when the masters introduced a new wood, the men making shuttles got symptoms of poisoning, and, of course, the men said that it was due to the wood, and the masters said that it was not. I should also like to say that the doctors said that the men suffered from symptoms which agreed with a cardiac action, and that this drug must have an action on the heart. We got samples of this wood, the masters sent us samples, and we investigated it. We got the alkaloids out, and we found that the alkaloids had a curare-like action; in fact the stuff, so far as it was concerned, might have been curare. It is not curare, it is a different substance, but it is the same type of drug. The essential thing that the men suffered from was great difficulty in getting their breath because their motor nerve endings to the chest wall were depressed—not absolutely paralysed.

18710. How did the poison reach them, because they do not handle the shuttles much?—No, it was the turners, the men who live in an atmosphere of sawdust in the workshops, and get the dust into their chests.

18711. Not the men who look after the working of the looms?—No, it was the men actually mixed up in the sawdust. The place in which they work is, I am told, a cloud of dust.

18712. (*Mr. Tomkinson.*) When once made the reel was inocuous?—Yes.

18713. But it was in the process of making the reel, all the dust in the turning?—Yes, and then in regard to the first part of my *précis*, may I just mention the difficulty of obtaining evidence from clinical experiments, because I really think it is of some importance. I happened to be on a Committee of the British Medical Association for suggesting what drugs shall go in the new British Pharmacopœia, when it became necessary to find out the value of certain new silver compounds. Accordingly we sent out notices—I think we sent out 300 notices—to the leading ophthalmic surgeons, and surgeons attached to Lock hospitals, asking them what was the value of these new compounds, and the results which came in were the most remarkable I have ever seen. It would be thought that in dealing with the action of silver compounds used for eye work, conjunctivis, and various forms of inflammation of the eye, that if it is possible to make any definite statement from clinical observations it would be about a disease which you can actually see; you can apply the drug and say at once whether it has any value or no value. And yet the decisions of these expert physicians and surgeons as to the value of these new drugs were exactly divided. Some of them said, "This drug is our sheet anchor in the treatment of disease." Others regarded the same drug as quite inert and worthless. How are you going to determine between those two views? Of course, the explanation is simple. It means that the conditions in a clinical experiment are never the same; there is no control experiment. You have not two men exactly the same in every respect, except that one is being treated for this particular disease. You have no control, and you never know what action the drug has. You can give a drug, and the patient may get well or get worse; you cannot say in the case of an unknown drug whether it has made him better or made him worse.

18714. Have these compounds that you are speaking of been tested by animal experiment?—No; they have not. It is not necessary to do that by animal experiment. I was simply referring to the difficulty of making clinical observations and determining the action of drugs from clinical observation. It is practically impossible.

18715. I do not quite follow why, if you can get no satisfactory results from clinical observation as to the action of the drug, you cannot expect to get any information from experiments on animals?—In an experiment on an animal you have got exact conditions and control.

18716. But at the same time you say that it is not necessary to test it on animals?—Not these particular drugs, because they are not used for absorption at all; they are used only externally for inflammation.

18717. With regard to the discovery of the poison in boxwood and its operation, was that done by experiments on animals?—Entirely.

18718. Who was applied to to make the experiment?—In the first place the men—the Union—sent to Liverpool and got the wood examined and reported upon by a botanist, Professor Harvey Gibson, who isolated the alkaloid, and tried the effect of the poison on an isolated heart—a heart taken out of the body and working by itself—and he got an effect which corresponded to potassium.

18719. A heart taken out of a human body?—Taken out of an animal body. He got the alkaloids out, and then he tried those alkaloids on isolated hearts obtained from animals.

18720. (*Sir William Collins.*) Frogs' hearts?—Rabbits.

18721. (*Mr. Tomkinson.*) What is an isolated heart?—A heart taken out of the body and hung up and perfused.

18722. Made to beat by electricity?—No; it beats by itself if you send fluid through it. Any part of

the body will go on working by itself outside of the body if you treat it properly.

18723. For how long?—For many hours. A child's heart has been taken out six hours after death and been made to beat for several hours—not in this country, of course.

18724. (*Chairman.*) Detached from the body?—Yes; cut off and hung up.

18725. Made to beat by electricity?—No; by sending fluid through the vessels to it.

18726. You mean if a certain artificial action is applied to it?—No; it beats by itself. The reason why the heart beats is because fluid is going through it's vessels.

18727. That is what I meant when I said some artificial action.—Yes; that is what I meant.

18728. (*Dr. Gaskell.*) Would you not rather say some nutritive action?—Yes. Then the masters, of course, did not approve of this report, and they sent the stuff to us, and a chemist, Mr. Harrison prepared the alkaloids and I did the experiments on animals, which showed that it was a curare-like poison.

18729. (*Chairman.*) You did the experiments?—Yes, and I gave evidence about this drug to the Dangerous Trades Committee.

18730. Was that at King's College?—No; it was at Cambridge.

18731. (*Colonel Lockwood.*) How many experiments on dogs have you conducted during the past year?—Perhaps 18 to 20. I cannot remember.

18732. I understand from you that there is no difficulty in keeping a dog under an anæsthetic provided it is given slowly at first: that you never experience any difficulty?—Never. The danger is that if you give a dog 6 per cent. of chloroform vapour it dies in a minute or half a minute. If you give it 0·1, then go to 0·2, then to 0·3, and so on, it never dies.

18733. How long did these experiments that you have been conducting generally last?—Perhaps two or three hours; never more than three hours, I should think.

18734. And the animal you operate upon is always killed before it recovers?—Yes, always.

18735. Who does that?—I do that.

18736. How?—Generally by injecting air into a vein. You see I have a vein always prepared. I blow in a little air which goes straight to the heart, gets behind the valves of the heart, and the heart stops beating. That is the simplest way.

18737. (*Sir Mackenzie Chalmers.*) You blow into a vein?—Yes, you blow air in.

18738. (*Colonel Lockwood.*) And that is done at the conclusion of the experiment?—Yes.

18739. Before the class?—If it is a class experiment, before the class.

18740. You show these experiments probably among others to classes?—I give a series of demonstrations regularly to classes.

18741. Have you ever seen any levity such as is alleged by one of the witnesses on the part of students?—I have never seen any levity.

18742. They look upon it as a serious business?—It is the most important part of their curriculum without any exception. Lectures can be got up out of books, and they do not matter a bit. If I may just show you, that is a record taken in one of my class experiments (*exhibiting a diagram*). *There* is the heart. *That* is the effect of chloroform on the heart. The blood pressure goes down because the chloroform is given: the heart beats more feebly, therefore the blood-pressure goes down. The men see that actually taking place.

18743. Are you in favour of conducting experiments, the result of which is already known, for the instruction of students?—I think it is of vital importance. I could not over-estimate the importance of it.

18744. In your opinion it is absolutely necessary?—Absolutely necessary, and all my colleagues at Cambridge agree with me.

18745. Do I correctly understand from you that chloroform and curare, if the curare is given in sufficient doses, have both practically the same effect of anæsthesia?—If curare is given in big doses, not in small doses.

18746. But it paralyses the sensory nerves of the spinal cord?—The sensory nerves in the brain. Chloroform does not necessarily paralyse those in the cord.

18747. To put plainly, an animal that was thoroughly curarised with a big dose would absolutely not feel any pain?—I believe not; it could not.

18748. Is that your own idea only, or is it supported by other evidence? Is it a well accepted fact by the profession?—Yes, I think it is a well accepted fact.

18749. Then why do the profession always give curare accompanied by an anæsthetic?—I think there are very few professions which are quite so conservative as the medical profession, and when a drug has once been stated as being used for a certain purpose, although it is shown for years and years to have no such action, it is still given for that purpose. Tannin is still given to stop bleeding from the lungs, although it has no effect; it is all destroyed in the alimentary canal; yet it is still given.

18750. I suppose you are aware that a great portion of the public have looked upon this question of curare with very great feeling?—Yes, I am perfectly aware of it, and I think it ought always to be given with an anæsthetic.

18751. You do think that?—Certainly, because as I have said, it is only in large doses that this central effect, so far as we are certain, comes on.

18752. Then if it is effectual, given in large doses, why not give a large dose?—One objection to that is that it is very difficult to get curare; it is almost unobtainable.

18753. Is it not on the market?—No; you can buy stuff that is called curare which does not contain any curarine.

18754. At all events the public may understand that you can obtain complete anæsthesia from curare alone?—Yes, certainly.

18755. (*Sir William Church.*) We have had a great many positive statements made to us by witnesses that morphia is not an anæsthetic. I should like to ask you one or two questions on that point. On page 25 of the third volume at Question 7308 with regard to the evidence given during the trial of Bayliss v. Coleridge, the witness said: "These attempts to prove that morphia is an anæsthetic are futile," and a little lower down in the same paragraph: "Claude Bernard quite appreciated the utility of morphia from the vivisectors' point of view, though he was honest enough to acknowledge its utter uselessness as an effective anæsthetic for vivisected animals. These are his words: 'The animal still remains sensitive, a touch on the cornea induces the closing of the eyelids, but he is quite still, and lends himself without a movement to the most delicate operation. . . . He feels the pain, but has lost the idea of self-defence.'" You would not agree with that?—I should like to point out in the first place that there is a mis-translation there. The animal remains, the word is "*sensitive*" in French. That is not correctly translated as "sensitive"; "reflex" it the correct translation I should say there. The animal still remains reflex, not sensitive. That is quite a different thing. That is the first thing that I should wish to say. And then, secondly, of course Claude Bernard was perfectly correct in stating that morphin is not a suitable anæsthetic in vivisection experiments, because the animal is reflex, it can move; it is only the sensory cells which are really depressed excepting in enormous doses, of course.

18756. If you would turn to Question 9266, which is on page 105 of the same volume, you will see there "Dogs are but slightly susceptible to the action of morphia." "'Caldwell gave a dog 17 grains without producing serious symptoms; whilst to a number of dogs he gave 12 grains hypodermically, with no bad effect.' Then Claude Bernard refers to a dose of 30 grains." Do you agree with what is stated there to be a fact, that Caldwell gave a dog 17 grains without producing serious symptoms?—No, I do not know how he gave it. Supposing this is absolutely true, then I say that the morphin was given by the mouth, and was never absorbed. People are always talking about drugs; but if they give the drugs by the mouth, how do they know that the drugs have got into the circulation? Many drugs are not absorbed at all.

18757. He is also stated to have given 12 grains hypodermically, with no bad effect. That did not

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mean, I imagine, without any effect at all, but without any prejudicial effect to the particular class of experiment that he was conducting?—Yes, you can give enormous doses of morphin to dogs, and nothing happens, except that the respiration gives out, and the animal dies in that way. But morphine has not a depressent action on the heart, so that you can give grammes upon grammes to dogs if you do artificial respiration; indeed I have done so.

18758. (*Sir Mackenzie Chalmers.*) Can you give grammes and grammes without producing anæsthesia; that is the point?—No, you cannot; you will produce anæsthesia then. But you would not kill the heart.

18759. (*Sir William Church.*) So far as regards the action upon sensibility to pain goes, is the action in the dog different from that which it is in man?—It is exactly the same.

18760. Does it require a very much larger amount of the drug to produce insensibility to pain in a dog than it does in man?—No, I should say much about the same.

18761. Therefore you would consider that 3 grains of morphia would undoubtedly produce insensibility to pain in a dog?—Yes, I should.

18762. And as Professor Starling says here, 15 grains would be quite an excessive dose for that purpose?—Quite an excessive dose, quite an unnecessary dose.

18763. By excessive I meant unnecessarily large?—Yes.

18764. At Question 9498, on page 113—this again is with regard to what Claude Bernard says about morphia—a quotation is made from Claude Bernard that the action of morphia differs entirely from anæsthesia. You would not agree with that?—Morphin, I tried to point out, does differ from chloroform. I made two distinct groups.

18765. That is to say, in its action it belongs to an entirely different series of drugs?—Yes.

18766. But I am speaking only with regard to anæsthesia alone?—There would be no difference.

18767. As to insensitiveness to pain it has much the same action upon sensibility to pain as the other series of drug?—Exactly the same.

18768. And it affects, we believe now, the same cells in the nervous system?—Yes.

18769. You have done much work with regard to the standardisation of drugs?—Yes, I have done a good deal.

18770. Is there any way by which many drugs can be standardised, except by watching their effects upon animals?—There are five drugs—digitalis, stropanthus, squill, ergot, and cannabis indica, the chemistry of which we know so little, that we cannot standardise them in any way other than physiologically. Of these two drugs, namely digitalis and ergot, are of the most important in the Pharmacopœia. There are ten different ergots there (*handing in a list*). Those ergots were bought at different chemists' shops. This is simply to show the variation in the action of ergot. A practitioner, say, having to treat with a case of a woman in labour, gives one of those ergots, and probably in three cases out of four it is inactive. The same applies to digitalis.

18771. (*Chairman.*) How do you get rid of those difficulties? Do you go to some special places for your drugs, where you avoid that risk?—You have your drugs standardised.

18772. (*Sir Mackenzie Chalmers.*) When you say that you standardise your drugs, do you mean that you standardise once for all, or do you have to standardise each brew?—Each brew. Certain wholesale chemists standardise each lot that they sell to the public.

18773. (*Chairman.*) The only way by which you can ascertain their action is by experimentation on animals?—Yes.

18774. (*Dr. Wilson.*) What is the test; is it blood pressure?—In that case it is blood pressure for ergot—the height to which the blood pressure rises.

18775. (*Sir William Church.*) Has the absence of this accurate knowledge of the strength of the drugs led to most diverse opinions being expressed by medical practitioners as to the value of certain drugs?—Certainly.

18776. In fact, it has been held that even ergot has no action upon the uterus?—Yes, and the stuff that is sold in many chemists' shops has no action upon the uterus.

18777. Cannabis indica is another drug about which there has been a great difference of opinion?—Yes, it is more usually non-active than active as bought in shops.

18778. Are there any drugs in which chemical processes cannot at present show their real composition and strength?—Yes, for those drugs that I have mentioned chemistry is at present quite worthless.

18779. In these drugs through your not being able to isolate the alkaloids from them with ease, is it very difficult to tell what is the strength of any given specimen?—Yes, that is so; sometimes active principles are sold, and in one case, for example, the "active principle" was extremely active. In another case the so-called "active principle" was beautifully crystalline, and it had no action at all; as soon as you begin to purify them they lose their activity.

18780. Ergotin is an example?—Yes.

18781. (*Sir Mackenzie Chalmers*). Is that passive or active?—It may be either.

18782. (*Sir William Church.*) In Chemistry, microscopy cannot tell the difference between an active crystal and a non-active one?—No.

18783. (*Sir William Collins.*) Then I rightly apprehend that it is not so much by the composition or the ingredients of the pharmaceutical compounds in future, but by their action based on standardisation that we must look for their therapeutical differences?—Not at all; most drugs in the Pharmacopœia can be, and are standardised by chemical means. It is only certain kinds that cannot be, the five that I have mentioned.

18784. Does the remark that you have been making apply only to those five?—There are a few others, but only of those five have I an experience. No one would dream of standardising atropin in any other way than by weighing it, or morphin. Nor would we do so with these drugs if we could standardise them in any other way.

18785. Is the composition of curare ascertained with any certainty?—Curarine has been prepared in Germany, but, as I say, you cannot get it; you cannot buy it. I have once seen a crystal of it as a great curiosity, and that is all. I do not think its chemical composition has anything known about it.

18786. I was going to ask you whether you could give its chemical formula?—I do not think it is known.

18787. I gathered from what you told us that amongst the gauges for determining whether anæsthesia is present or not, are the reflexes and blood pressure?—Yes.

18788. Are there any other gauges besides those?—Those are the two which I myself use. There are other things, of course, like the pupil and the depth of respiration, and so on, showing the condition of the medulla. But the two that one chiefly uses in animals are the reflexes and blood pressure; at least, those are what I go by.

18789. Is it possible in human beings to go by the blood pressure?—No, it is not.

18790. We have been told that there is a mechanism by which blood pressure in human beings can be accurately gauged?—That is so. When I say that it is not possible, I mean that it would be very inconvenient while the anæsthetist was anæsthetising, to have a man taking the blood pressure and recording it all the time, besides which the blood pressure mechanism as at present made for man will only give you the blood pressure for any one moment. You will then have to start it over again.

18791. I wanted to ask you whether any attempt has been made to utilise the blood pressure gauge for the purpose of determining the amount of anæsthesia in human beings?—No, not so far as I am aware.

18792. Is that because you think that the mechanism employed for gauging the blood pressure, apart from the manometer, is unreliable?—My point is, that in

man (we have been doing a lot of work on this point lately), you can take the blood pressure, say, in the arm, at a certain moment, but you cannot take it continuously. You see, it is taken by blocking the pulse, but you cannot keep the pulse blocked; you have to let it out and then take it a minute or so afterwards, so that you get periodic readings.

18793. Did I correctly understand you to say that in the case of morphia the test of the presence or absence of reflex was inapplicable?—Yes, that is so, of the lower reflexes.

18794. By a reflex action do you mean a conscious action?—Oh, no.

18795. An unconscious action?—An unconscious action. Most of our reflexes, of course, are unconscious.

18796. But may not a reflex action be conscious?—No, not conscious.

18797. We have had a notable case, that a Commissioner suggested, that sneezing was a reflex act, and that sneezing was conscious. What do you say to that?—I should say you would still sneeze if you were unconscious. Breathing is a reflex action; we do not know that we are breathing, but, nevertheless, you can breathe consciously, and then it is no longer reflex, so to speak.

18798. Is a reflex act a voluntary act?—No, it would not be reflex.

18799. Is it part of the essence of being reflex that it is an involuntary act?—Certainly.

18800. (*Mr. Ram.*) Would it be accurate to say that a reflex action may be sometimes controlled by consciousness?—That is quite correct.

18801. But that consciousness is not necessarily present to a reflex action?—That is quite correct.

18802. (*Sir William Collins.*) You spoke of the sensory cells in the spinal cord in connection with the curare point, I think?—With chloroform.

18803. Was it not curare?—With chloroform.

18804. At any rate you spoke of the sensory cells in the spinal cord?—Yes.

18805. Is the spinal cord sensitive?—I do not quite know what you mean by sensitive.

18806. Can the spinal cord feel?—Oh, no.

18807. When you speak of the presence of sensory cells in the spinal cord, you do not wish us to assume that the spinal cord can feel?—I mean the cells that receive impulses. There are receiving cells.

18808. Cells receiving afferent impulses?—Yes.

18809. The last Commission referring to curare and Claude Bernard, put this on record in their report: "It has, however, been positively stated by perhaps the highest authority on such a subject, Claude Bernard, to have no effect in producing insensibility to pain," that is curare. Do I correctly understand that you contest that view?—Yes. You cannot expect anything to stand still. That was the best knowledge that the Commission had at the time, but it is not the best knowledge now.

18810. We were told by a previous witness in answer to Question 3617 in the first Report, that "the evidence before us really is not sufficient to my mind to demonstrate conclusively that curare does or does not abolish sensation. On general considerations I would think that it probably abolishes the functions of the higher part of the brain, that is to say consciousness; but we have not sufficient evidence for that." What do you say in regard to that?—In the first place you will remember that I told you that curare must not be regarded as a sort of unique drug, or by itself, that it belongs to a definite group of drugs which all have these three actions; they all paralyse the brain; all of them produce convulsions under certain conditions, and all paralyse the motor nerve endings. Curare first of all belongs to that group very definitely—that is the first point. And secondly, I have repeated Claude Bernard's experiments, and given large doses of curare, and have succeeded in completely cutting out the sensory effect.

18811. Do you, or do you not, agree with that previous witness, who said that we have not sufficient evidence to say whether it abolishes the functions of the higher parts of the brain?—I think there is sufficient evidence that in big doses it does. In small doses I agree with him, no; in big doses, yes.

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18812. Is that the result of your own experiments?—To some extent it is the result of my own experiments.

18813. When did you conduct these experiments with curare?—Some three or four years ago now. There have been odd experiments done at odd times, many of them on dead animals, of course—technically dead.

18814. Then did you test whether or not the higher functions of the brain were abolished?—In those experiments that I was referring to, I was doing them on the spinal cord, the receptor cells of the spinal cord.

18815. I was asking you for the evidence by which you were led to conclude that the higher parts of the brain were abolished, as regards their function, by curare?—The only reasonable evidence of that is that curare, as I say, belongs to this definite group of drugs which have these three actions in common.

18816. Do you argue by analogy from its congeners, as it were?—Yes; it is practically impossible to obtain direct experimental evidence.

18817. Why?—Because you have the motor nerve endings gone. You cannot stimulate the brain, of course.

18818. Must it ever remain in doubt then as regards curare itself, apart from analogy, whether it does or does not abolish consciousness of the higher parts of the brain?—No, it need not; but the only experiment which would prove definitely whether small doses of curare abolish pain would be on man. Any volunteer must have the curare injected and artificial respiration must be ready, and some operation must be done on him.

18819. I understood you to say that you had taken curare yourself?—Yes, by the mouth.

18820. In what doses?—A big enough dose certainly to completely curarise a dog.

18821. Did it have any effect upon your own sensibility?—It had absolutely no effect at all.

18822. Or on your motor nerves?—It had no effect of any kind at all. I ought to say that every dose of curare varies. You get one sample and it is very active, and the next sample is inactive, so that you cannot weigh out a dose and say I am taking so much. The only way by which you can be sure that it is very active is by trying it on frogs.

18823. Is it possible to standardise curare?—Yes, on frogs.

18824. (*Mr. Ram.*) Did you standardise all that which you took?—Yes, I knew all about it.

18825. (*Sir William Collin.*) Was it an active or inert specimen?—It was a very active specimen.

18826. Does the commercial curare vary in quality?—Yes, tremendously; it varies from great activity, which is rare, to inertness, which is common.

18827. What is the price of curare in the market?—I would not like to say, but I believe it is about 5s. a gramme. I am not at all sure. Of course, it is a resin, and there is very little active alkaloid in it.

18828. You told us, I think, that there were a great many other drugs that acted in the same way as curare?—Yes.

18829. You said a host of them?—Yes.

18830. You mentioned nicotin, I think, as one?—Nicotin I mentioned.

18831. Conium has been mentioned before this Commission already. Is conium a powerful poison in man?—Yes.

18832. It was denied or doubted by a previous witness. Is it a powerful poison?—Yes, it is the poison that Socrates died of, you remember—hemlock.

18833. Would you name any others in this host of drugs that act in the same way as curare?—Nicotin, cocaine, lobelin, galseminin, apacodin, as I say, this boxwood, and a great number more keep cropping up in literature periodically. I am not personally acquainted with them.

18834-5. You have named some six or seven, but when you speak of a host one rather assumes a larger number?—I meant perhaps twenty or thirty.

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18836. You know that there is a restriction under the Act with regard to curare. Should the same restriction apply to the host of other drugs that resemble it?—As I pointed out at the time, most of the other drugs paralyse the brain before they paralyse the motor nerve endings. Let me give you an example. Nicotin paralyses the brain first. Then there is no question there. In the case of curare, it happens to be the other way round; it goes for the nerve endings first, and then to the brain. So that we have difficulty. That is why there is all this fuss about curare.

18837. Would not that suggest that the analogy between curare and the other members of this host is not very close?—Certainly not. I think in all text books on pharmacology you will find them grouped together as one definite group of drugs, having certain three or four actions in common.

18838. And you think you can argue from the action of some of these of which we know more to what the action of curare should be?—You can gauge it at once. For instance, when I got this African boxwood, I found that the African boxwood paralysed certain nerve cells. As soon as I discovered that, I classified that drug immediately into this group, and the other action simply fell out, as a matter of course. I knew what was going to happen before I tried as regards all the other effects.

18839. Is it a characteristic of this group that the ends of the motor nerves are paralysed?—Yes.

18840. Or is it that the ends of the motor nerves are paralysed before the abolition of the higher centres?—No, it is sometimes one and sometimes the other; they vary; and as I told you conium comes in between the two.

18841. The important point for us is to know, if possible, the order in which that paralysis takes effect—as to whether a person's or an animal's consciousness may remain after the absence of movement has ceased?—The only two with which I am acquainted, where the effect comes on first in the motor nerve endings, are curare and the boxwood. In the others the paralysis begins at the nerve cells first.

18842. So that in this important question there may be a lack of resemblance among these members of the same group?—I do not follow you.

18843. You suggested that there are a host of drugs which act in the same way as curare?—Yes.

18844. And that one may argue the action of curare from the known action of some of these congeners?—Yes.

18845. But the very important question of whether the paralysis of movement precedes or not the paralysis of consciousness, is a matter upon which there is a variation within that group?—Yes.

18846. Then is it possible to argue, from what we know of some members of it, what the action will be in the case of other members, in respect of the order of paralysis of motion and consciousness?—No, not with an unknown drug. The order varies. The type of action is the same in all.

18847. It may be that while in the case of curare the paralysis of motion precedes by a considerable length of time the paralysis of consciousness, in other members of the group the reverse may be the case, or there may be a great difference in the time intervening?—Yes, that is correct.

18848. Then you cited the replies of some practitioners in regard to the use of silver compounds in diseases of the eye?—Yes; may I pass you the Report (*handing in the same*).

18849. Thank you. I see you say, "Wide differences of opinion are, however, expressed. Regarding protargol, an ophthalmic surgeon remarks that it is 'inert and quite useless'; while another observer states that, in his experience, it is 'by far the most reliable salt for injecting or irrigating in cases of chronic gonorrhœa.'" I understand that your point in quoting this was to show the variation of opinion by observers of the drug under observation?—Yes, and of the extreme difficulty of making use of clinical experience.

18850. But it points rather to inaccuracy of observation, does it not?—At first sight it does, but that is not, I believe, true. It points to the fact that the conditions are so extremely variable that it is very difficult to draw conclusions.

18851. You may have a plurality of causes in operation?—Yes; quite so.

18852. I suppose that may be true also in the case of an experiment upon an animal as well as in clinical observation?—You have always got the control—that is the point.

18853. Do you mean that in the case of an experiment on a living animal you never have to do with plurality of causes?—Yes, you have plurality of causes; but you have the cause and the effect. You know that the effect is due to the cause, but you do not know exactly the intermediate steps between; you can differ on that.

18854. Is it not a fact that different observers watching the same experiments on animals have drawn different conclusions?—No; they do not differ on the facts. The facts remain the same. They only draw different conclusions as to how the effect is produced.

18855. But even in the case of watching an experiment accuracy in the observation of facts is required?—Yes, absolutely.

18856. Have you not come across instances in which facts have been interpreted differently by different observers in the same sets of experiments?—What I mean is that, supposing you observe such a simple thing as a rise in blood-pressure, there is a fact written down, and you cannot deny it. As to the origin of it, how it has been produced, you may have two schools, one saying that it is produced one way and the other saying it is produced the other way.

18857. Given agreement in observation, you may have variety of interpretations?—Yes.

18858. (*Sir Mackenzie Chalmers.*) You mentioned, I think, that you had a Therapeutical Section of the Royal Society of Medicine?—Yes.

18859. Would you kindly tell us, who are not members of the profession, what the Royal Society of Medicine is?—It is a collection of most of the learned Societies of Medicine joined together recently into one big society, which they call the Royal Society of Medicine, and with various sections; that is to say, the Therapeutical Section is in one group, Diseases of Children is in another.

18860. Has it a local habitation?—Yes, in Hanover-square.

18861. And is it representative of a large body of medical opinion?—A very large body.

18862. There were some parts of your evidence that I did not quite understand, which I should like to know a little more about. You referred to morphia as exercising a specific selective action?—Yes.

18863. I suppose there are a good many other drugs in the same group with morphia, or are there not?—There are a few others, but not very many.

18864. One, for instance, is the substance used in India called Bhang?—Yes, that is Indian hemp, cannabis indica.

18865. Does hachish belong to the same group or not?—Yes; that is the same thing. I classify it in the same group myself, although it has a different type of action.

18866. Does it destroy sensation?—It produces such a weird kind of intoxication that it is difficult to observe. It does to a very large extent, but nothing like to the same extent as morphin.

18867. In your opinion, for certain animal experiments, morphia is a safe, proper, and effective analgesic?—Yes.

18868. You are perfectly satisfied of that?—Absolutely satisfied.

18869. Do you think there is much room for idiosyncrasy in animals as regards the action of morphia?—No; no room at all, I think.

18870. We were told, I think, by one witness that pigs were absolutely insensible to morphia. What do you say to that?—How was the morphia administered?

18871. We were not told that?—There are all sorts of statements of that kind made; one is continually coming across them. For instance, they say henbane is not poisonous to hogs and it is to hens; but that is not true, of course. It is poisonous equally to both of them if you give it in the correct way. Some drugs are given by the mouth and are not absorbed, and people say they are not poisonous.

18872. What I wanted to get at was this: Assuming that the morphia is injected in the way in which you use it in laboratories, has it a very different effect on different animals?—No; it has not. It has precisely the same effect, although it is interpreted differently. If, for instance, you inject morphin into frogs you first of all produce complete paralysis and narcosis; 24 hours afterwards the frog gets strychnine-like convulsions; that is to say, a frog gets convulsions and man does not. How is that? Because you cannot give a dose of morphin to a man like that and then let him recover. If you could he would undoubtedly get similar convulsions; indeed, it is quite common to see a man with these twitchings in exactly the same way, on recovering from morphin poisoning.

18873. Take the case of a man attempting suicide by morphia. Would you get similar results?—Yes.

18874. Is there any reason for supposing that a dog is less sensitive to the action of morphia than other animals of the same size and weight?—There is no reason at all. I should say that it is more sensitive.

18875. And you do not think that there is much idiosyncrasy in dogs?—None at all. I have never seen any.

18876. You have had large experience of giving morphia to dogs?—Yes.

18877. I think we were told that there was one particular family of human beings who are curiously insensitive to morphia?—Now we are coming to a different question. If you take morphin for a certain length of time, you learn to destroy it just as if you take alcohol for a certain length of time in sufficiently big doses, you learn to oxydise it completely.

18878. Or a pipe of tobacco?—Yes, and you learn to use your morphin just as if it were bread and butter. That is done by means of a ferment. This ferment, this power of destroying morphin, might conceivably (although I do not know it, I never heard of such a thing) be hereditary.

18879. Then as soon as you got to that stage you would have what is called an idiosyncrasy?—Yes.

18880. But as regards many drugs there is a marked idiosyncrasy, is there not; for instance, quinin. Some people can take quinin and some cannot?—Some people cannot take quinin because it produces an objectionable action so extremely easily.

18881. (*Sir William Collins.*) Would what you say amount to this: that given a substance which is a poison to any animal if that substance is only properly got into the blood of any other animals it would be poisonous to them too?—Yes, it amounts to that. That is correct. You see there are no drugs with which we are acquainted which have any different action on the heart or on the kidneys of animals from that in man. The only differences which occur are on the brain. For instance, cocaine in big doses causes convulsions. Cocaine acts on the cerebral hemisphere, the big part of the brain. In the frog there are practically no cerebral hemispheres. You can give an enormous dose of cocaine and you can hardly get any twitchings. In the rabbit you want a relatively smaller dose, but still a very big dose. In the dog, in which the cerebral hemispheres are better developed, you want a still smaller dose, in a monkey still smaller, and in man the smallest. So that you can tell the dose of cocaine necessary to produce convulsions by weighing the cerebral hemisphere and then the body; the dose of cocaine varies in relation of these two exactly.

18882. (*Sir William Collins.*) What you are saying if true of chemical poisons will not hold in the case of bacterial poisons, will it?—You mean such as diphtheria toxin?

18883. I am thinking of the case of septicœmia of mice, which we have been told is harmless to one variety of mouse and fatal to another?—They are proteid substances, or bodies allied to proteid substances, and that would not apply, of course. The horse serum, the serum from a horse may be poisonous to one animal and not to another.

18884. You were speaking of the chemical poisons of the pharmacopœia?—Yes.

18885. (*Sir Mackenzie Chalmers.*) Chemical poisons do not multiply in the blood itself?—No.

18886. Some bacterial poisons multiply in the human body?—Yes.

18887. However, the practical point is, that you feel no doubt that as morphia is administered in laboratories in England, it absolutely protects the animal from pain?—Absolutely.

18888. Urethane, I understand you, is a general anæsthetic?—Yes.

18889. I am sorry to say that I do not know what it is?—It is a curious substance, because Schiemerderberg thought out the sort of substance that he wanted. He thought to himself, "I want to get a substance different from chloroform which will have the action of chloroform but which will not depress the medulla." So he made up a formula on paper and then built up urethane from it. It is a combination of urea and alcohol, so to speak.

18890. It is a synthetic substance. Is it made in the laboratory?—Yes.

18891. Then does it produce a sort of uræmic poisoning, or produce alcoholic paralysis?—No, ureia does not produce uræmic poisoning. It produces an action very similar to that of chloroform, but without any depressent action on the heart. It has the exact action of chloroform, or any other member of the group, on the brain. All these drugs of which I call the alcohol group collect in the brain.

18892. (*Dr. Gaskell.*) And you can tell that it will have that action by its formula?—No, you cannot, but Schmiederberg thought that you could, and he built up the formula.

18893. (*Sir William Collins.*) There is a considerable relationship, is there not, between the chemical constitution and physiological action?—There is some relationship, but it is not considerable to the pharmacologist at present, because a small change produces a tremendous difference in action sometimes.

18894. (*Sir Mackenzie Chalmers.*) It is not worked out?—It is not worked out. There must be a relationship.

18895. In your opinion for animals, urethane is a useful and reliable anæsthetic, and an analgesic?—I think it is the best.

18896. There is no danger, if it is properly administered, of the animal suffering any pain?—No.

18897. I did not quite understand, I am afraid, what you said to Sir William Collins, that when either chloroform or curare is administered, the sensory cells of the spinal cord still remain active?—No; it was chloroform. I said that chloroform paralyses the sensory cells. I said that all the anæsthetics paralyse both sensory and motor, but they paralyse the sensory in every case first.

18898. Then I misunderstood you. I understood you to say that even after you had administered chloroform, the sensory nerves of the spinal cord might still be active?—Yes, they might still be active in light anæsthesia. That is quite true. It is complex, and it is difficult to explain everything at once. When you administer chloroform you begin by paralysing the higher centres first. You paralyse control in the brain, power of paying attention and judgment; they go first, and therefore you get, as we may say, a riot of the lower centres; you begin to reel in your gait and do not consider what you say, you lose your politeness, and so on; you lose all external conformities with a polite existence. Then you get the depression progressing still further, and it goes to the motor area, and then it runs from the brain down to the cord; it misses out the medulla; lastly, it paralyses the medulla; but always, in whatever part of the central nervous system the action occurs, you have evidence to show that it paralyses the sensory cells before the motor. Nevertheless, paralysis progresses in this order.

18899. But as long as the sensory cells in the spinal cord would reply to a stimulus might there be pain?—No, appreciation of pain may have gone long ago. But with a smallish dose of an anæsthetic you may pinch the tail or hind limb and get a leg drawn up.

18900. That is purely reflex action?—Yes, that is purely reflex; but the sensory cells are intact in the spinal cord. With a big dose of anæsthetic that action is gone; I mean that the sensory cells are paralysed. In the second case, probably the motor cells also, although these become paralysed only very late.

18901. You spoke of the effect of a large dose of cocaine, but cocaine acts as a local anæsthetic simply

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and simply cuts off communication with the brain, does it not?—Yes, it paralyses the fine nerve fibrils. If you inject a little the fine nerve fibriles all round are paralysed by the cocaine. It does not paralyse the motor fibrils because there are not any in subcutaneous tissues.

18902. It simply stops the message going to the brain?—Yes.

18903. When you told us how a large dose produces convulsions, that was not applied locally?—No, that was given internally, of course.

18904. By injection?—Yes.

18905. There is no doubt whatever, I suppose, as regards curare, that it paralyses the motor before it paralyses the sensory nerves?—There is no doubt about it; but it paralyses the sensory cells as well.

18906. That being so, it is essential to keep the provision in the Act that it must only be administered with another anæsthetic?—Yes, I think that is a good provision.

18907. Because otherwise there would be a period of time during which there would be sensation without motion?—Yes, that is the point; that is right.

18908. May I ask you what was the object of your personal experiment in taking curare?—It was taken accidentally. We commonly take our drugs up in pipettes, and while I had a pipetteful to my lips I was attempting to talk to someone, and sucked up the whole, and swallowed it before I knew what I was doing.

18909. And you had an uncomfortable quarter of an hour, I suppose?—Well, I did not know what was going to happen, but nothing happened at all.

18910. Has any attempt been made to determine chemically the constitution of curare?—Yes, in Germany an attempt has been made; they have got pure crystals of curarin, but the supply of curare has given out. We are still dependent, you see, on the natives, who keep the sources of curare a secret.

18911. And for the purpose of chemical analysis you require a considerable quantity, I suppose?—Yes.

18912. You told us about some interesting experiments with boxwood. The result of those experiments was that the dust of boxwood was capable of poisoning human beings through inhalation?—Yes.

18913. Producing heart symptoms?—No, no heart symptoms. The essential symptom was this tremendous difficulty in getting breath; the men turned blue in the face and gasped for breath. The doctors who examined them found nothing wrong with their hearts, except that the heart sounds were very feeble, but they regarded it as and called it cardiac dyspnœa; that is to say, they thought the difficulty in breathing was due to the heart, whereas in reality the difficulty of breathing is due to partial paralysis of the motor nerve endings.

18914. As the result of that, what has been done practically?—I have not seen the report of that Departmental Committee, but I fancy that they are either going to forbid the use of the wood altogether, or they are going to compel the people to wear respirators.

18915. If they wore efficient respirators it could be handled with safety?—Yes, it could be handled with safety.

18916. You mentioned that you thought it was most important to do certain experiments before students. What number of students pass through your hands annually?—I should think 150.

18917. Would your experiments be pharmacological experiments or physiological experiments?—Entirely pharmacological.

18918. Do you think it is important that students should see any physiological experiments or not?—Yes, there are certain things which I assume that they know all about when they come to me.

18919. May I ask, are your students third-year men?—Yes, the third-year men; after they have done their anatomy and physiology and whilst they are beginning their work in the wards.

18920. And you think that at that stage it is essential to show certain experiments on animals to the students?—I regard it as the most important part of their education.

18921. It has been suggested that it has a deteriorating effect on their character and humanity. Have you any opinion on that matter?—I regard that statement as absolutely ludicrous. Watching an operation does not produce that effect; it rather produces the other effect.

18922. It is said that in watching an operation you approach it with a different mental attitude, because the operation is entirely for the benefit of the patient, whereas the other is simply to illustrate a scientific fact?—It is entirely for the benefit of the watcher certainly.

18923. We have been told that it would have necessarily a harmful effect on the students' mind?—I do not think that for a minute. I cannot see how it could possibly affect their minds detrimentally at all.

18924. You mentioned in your *précis* a series of researches, some of which we have not had any reference to?—These are only some of them. They are typical ones.

18925. Have any of these been carried out in your laboratory?—Yes, all of them either are being or have recently been.

18926. Take the first—lymphocytosis. The causation and mode of production of this condition, you say, has been determined. Is that a recent research?—Yes.

18927. Has it any practical effect on treatment?—All knowledge, of course, must have a practical effect. May I explain what it is roughly? There are, roughly, just for the purpose of description, two kinds of white blood corpuscles. There is one kind which lives, so to speak, in the spleen and lymphatic glands, and the other, which has its habitat in bone marrow. In the spleen and lymphatic glands there are muscle fibres, and what we have shown is, that we can send out at any moment many of these leucocytes which live in the spleen and lymphatic glands. And we can do that at any moment by means of pilocarpin. If I want to produce lymphocytosis in a man, I can do it in a few minutes by giving a small dose of pilocarpin.

18928. Excuse my ignorance; these white corpuscles are what are sometimes called phagocytes?—Yes, but of these are mainly the bone marrow variety.

18929. And not a phagocyte?—Not to the same extent as the others.

18930. So that its action would not have a direct curative effect on the wound?—I do not think it would personally, but you know the tendency in phthisis (or it was the tendency a short time ago) was to give drugs supposed to increase the leucocytes. I do not think it is used much now.

18931. But still this research may have, at any time in connection with some other research, an important effect on treatment?—Certainly. It is a fact about pilocarpin which I expect all students to know now.

18932. I suppose this work is still going on?—Yes.

18933. Then the next is "arteriosclerosis." You say that it has been shown that this condition arises from any cause which temporarily raises the blood pressure?—Those are most interesting experiments, because they show this. You know that as we get older we all get certain changes produced in our vessels. One type of this change we can produce artificially. It is due to any cause which raises the blood pressure. In order to produce arteriosclerosis, we raised the blood pressure with one's fingers compressing the abdominal aorta in a rabbit. A rabbit has got a stiff abdomen as we have; you can put your hand down and quite easily compress the aorta. We did that once a day every third or fourth day. Then what happened? The rise of blood pressure cracked across into the elastic fibres in the blood vessels—the blood vessels are full of elastic fibres. Inflammation occurred round these, fibrous tissue developing, and calcareous material subsequently being deposited in the artery. It is a practical point, too; it teaches us what not to do as we get on in years.

18934. But as we get older it is a more important thing to prevent this arteriosclerosis than to produce it artificially?—Yes, but if we know how to produce it artificially we are a long way towards prevention.

18935. So that the pressure caused by nicotin in smoking might be the cause of arteriosclerosis?—Yes,

any cause which raises the blood pressure to a fair extent will produce arteriosclerosis.

18936. Would you feel inclined to say, then, that as we get older we should diminish our smoking?—No; but there is this other point to consider, which you can learn from experiments. If you take a boy and give him a cigar his blood pressure goes up, and later he gets collapsed and blood pressure comes down; if you give him another cigar a week afterwards his blood pressure goes up as before. If you, however, give that boy a cigar every day, after a time his blood pressure does not go up; he has learnt to destroy the nicotin just as you may learn to destroy morphin.

18937. Is the moral of that to begin smoking as early in life as you can?—No, but I think the moral is that it is better to be a moderate and continuous smoker than to smoke in fits and starts.

18938. And never to discontinue your smoking?—Yes.

18939. I see you mention, among other things, the action of tobacco smoke and the origin of nicotin immunity. Is that at present being investigated in your laboratory?—Yes.

18940. In tobacco smoke itself very little nicotin is contained, is it not?—There is a good deal. Practically the whole effect of a cigar is caused by its nicotin; the other constituents are unimportant.

18941. Has it a slightly paralysing effect, or what is its effect?—The first effect of nicotin is to excite. It excites and raises the blood pressure for half an hour; it stimulates everything for half an hour, and then after that you get the depressant action.

18942. (*Sir Mackenzie Chalmers.*) I see among other matters that you are investigating in your laboratories are the action of new cardiac tonics, the action of caffein, urea, and saccharin on the kidneys?—Yes.

18943. Are those experiments animal experiments?—Yes.

18944. Have they an important bearing on human treatment?—In connection with caffein, urea, and saccharin on the kidneys. The point is that we know nothing, at least I think I am right in saying nothing as to the cause of renal disease, how it originates. I had some sort of conception that renal disease might be caused by taking tea and coffee, and therefore we used caffein. Or it might conceivably be due to excessive urea, and therefore we tried urea. Urea has a marvellous effect on the kidneys. Thirdly, saccharin is a drug which is used to produce sweetening in the case of people who suffer from diabetes. A drug allied to saccharin I know will produce renal disease. We knew that, and so these drugs are being given at the present time by the mouth to dogs to see whether there is any effect; that is to say, they are simply being fed, some taking caffein as we take tea, some having their food sweetened with saccharin and some having urea put in their food.

18945. Saccharin at any rate is largely taken by certain classes of people, and it is important to find out whether it has any evil effects as well as good effects?—Yes.

18946. And to some extent, I suppose, the result of your experiments may be either to acquit or convict tea and coffee and certain other agents?—These are excessive doses of caffein.

18947. Still, long continued small doses may possibly have some effect?—Yes, as we know that they have such a marked effect on the kidneys.

18948. You are also investigating the relative action of the new local anæsthetics?—Yes.

18949. Including cocaine or eucaine among the new ones, or strovaine?—No, cocaine is at present a local anæsthetic in the pharmacopœia. e object to cocaine because in a considerable number of cases it produces toxic symptoms, and because it is irritant. Accordingly this Committee of the British Medical Association asked me to have the newer local anæsthetics investigated, so that we could make a report as to which of the many on the market supplied by wholesale druggists and chemists was the best, and this research is just about completed.

18950. It will have a most important effect on minor operations on human beings you think?—Most important. We are every day doing more and more operations under local anæsthesia. Now we inject the local anæsthetics in quite a number of cases into the spinal theca. You inject it through the back, you make the patient sit up and lean a little forward, and inject it into the spinal theca, where, of course, it paralyses first the sensory cells, and the man loses all sensation to pain. Then touch goes next. Pain goes first, then tactile sensation, and lastly motion. That is the sequence that always occurs; no matter what anæsthetics you use the paralysis goes in that order—pain, then touch, then motion. The man cannot feel anything, but he can still flex his toes. If you put these drugs——

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18951. (*Mr. Tomkinson.*) Pain goes first?—Yes.

18952. What second?—Tactile sensation; that is to say, a man can feel that you stroke his leg, but he cannot feel any pain. You can put a hot iron on his leg and he would look at it and feel you do it, feel all the sensation, but would feel no pain.

18953. And what goes last?—Motion.

18954. Where does consciousness come?—Consciousness is retained all the time. This is local anæsthesia.

18955. (*Sir Mackenzie Chalmers.*) How do you tell with an animal when the sense of pain is abolished by local anæsthetics? I can understand that you can investigate the action of these things with a human being, who will tell you his sensations, but I cannot understand how you can get any trustworthy results from an animal?—There are several modes, of course, of using those drugs. In this case a number of experiments on the anæsthetic point were done on men, on ourselves. I mean that a man had his arm bared and one drug was injected there, and another drug given here, and you can test it in that way. But where the animals came in mostly was as to the toxicity of these things. The anæsthesia is one property, the toxicity is another. The irritability is another, the toxicity and irritability are tested on animals.

18956. The problem is to find a thing which is a perfect anæsthetic, and at the same time a non-toxicant and a non-irritant?—Yes.

18957. Your name has been referred to in the evidence, I do not know whether you have read the evidence given before this Commission, but I think Dr. Gaskell will ask you about those particular experiments. Has your attention been called to that?—Yes, I have not read it, but I know the point to which I am referred.

18958. (*Mr. Ram.*) Sir Mackenzie Chalmers has asked you nearly all I wanted to ask you, and there are only two or three questions left that I wish to put. With regard to these local anæsthetics, of which you have just spoken of, you said that you could inject one or more of them into the spinal cord and produce anæsthesia by that means?—Yes.

18959. Would that be local anæsthesia or general anæsthesia?—I should not like to say which. It is anæsthesia below the point of injection. You inject it just where the spinal cord ends into the sub-dural fluid there, and it paralyses the lower part of the spinal cord. It is local, of course, in that it does not affect the other part of the body.

18960. It affects the portion of the body below the point at which you insert it?—Yes.

18961. And affects that part of the body universally?—Yes.

18962. And equally?—Yes.

18963. And completely?—Completely.

18964. When once you have arrived at a local anæsthetic which is not toxic and not irritant, is it safer to use a local anæsthetic than such an anæsthetic as chloroform or ether or the A C E mixture?—It is not a question so much of safety as of convenience and necessity, I think.

18965. But there are certain dangers we know affecting what we generally know as anæsthetics, chloroform, ether, and so on?—Yes.

18966. Are there any dangers, so far as you know, incident to the use of local anæsthetics?—With some of them there are considerable dangers. With cocaine you may, in certain people, accidentally inject the cocaine into a vein when you inject it, in which case it produces a toxic action on the brain and a toxic action on the heart. Cocaine is a toxic body. And, secondly, you may get sufficient irritation at the seat of injection to produce objectionable symptoms. I

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think we have now got a local anæsthetic; as the result of this work we have certainly determined the best, and are going to recommend that that one should be put in the pharmacopœia.

18967. With regard to all these researches that you have told us of, I gather from what you said that experiments on animals are not necessary for all, but for some?—Yes.

18968. In the case, for instance, of subjecting animals to the action of caffein, urea, and saccharin to see how they act on the kidneys, do the animals suffer any pain?—No, none at all. You can give saccharin to a man; it simply sweetens his food. He may object to having his food sweetened, but you cannot call that pain.

18969. And the same with animals?—Yes.

18970. (*Chairman.*) The animal is not operated on afterwards to find what the result has been?—No, we kill it afterwards.

18971. After what?—After three or four months; after they have been subjected to this experiment for three or four months the animals are killed—they die.

18972. (*Mr. Ram.*) And are dissected as dead animals?—Yes; and sections are cut of their various tissues.

18973. Have you at present found any result from the experiments as to those three matters, caffein, urea, and saccharin?—We have only killed one dog at the present time, and we killed that dog because we found that it was getting ill, that it began to lose its hair, and began to pass albumen in its urine, so that it was killed and its kidneys were found diseased. That was with urea.

18974. Do you attribute the disease of the kidneys to the action of the urea that you had administered to the dog?—We do. We cannot be certain until we have done two more experiments to make sure that it is a constant effect.

18975. With regard to the effect of drugs on blood clotting, have you experimented on animals with regard to that?—Yes, they were all done first on animals, only subsequently on man.

18976. What is the nature of such an experiment as that?—The nature was to take a drop of the animal's blood first. There is a convenient vein in the ear of a rabbit, and you just make a prick with a needle, you get a drop of blood, and take its clotting time with a certain instrument. Say, it is three minutes; then you give the animal a dose of the drug, the effect of which you want to determine. Supposing you give citric acid, you take its clotting time at various periods afterwards, and you find that the citric acid will prolong the clotting time from three to four, and five, and ultimately to fifteen minutes; and then there is gradual recovery.

18977. Does it make the blood more slow to clot?—It would make it more difficult to clot.

18978. When you say ultimate recovery—recovery from what?—The animal is none the worse for the citric acid.

18979. Then, with regard to the standardisation of drugs, you spoke about the necessity of using animals for experiment, and I see in this pamphlet that you have handed to us, which you put it in I think to show the varying nature of the strength of ergot?—Yes.

18980. This caught my eye: "For my part I unhesitatingly express the belief that many hundreds of patients die annually from digitalis and its allies not possessing the virtues which are required of them."—Yes.

18981. That is your belief?—Yes.

18982. If you could get a perfect standardisation for digitalis, for instance, then a doctor administering it would know exactly the strength that he was giving?—Yes.

18983. At present I suppose in many cases he has to give it in absolute ignorance of the strength of the drug he is administering?—In absolute ignorance.

18984. Intending to give perhaps a strong dose of digitalis, and giving a quantity which in a proper preparation would be strong, in effect he gives next to nothing to the patient?—Yes, he gives none.

18985. And the patient dies perhaps?—Yes.

18986. Is that true of what you call the allied drugs which I think you said were strophanthus and squills?—Yes.

18987. When you have arrived at a perfect knowledge of what the dose ought to be, and the strength ought to be, would it be possible to cause the sale of the standardised drug to be universal?—I think so, certainly. If a drug is not sufficiently active, it ought not to be used, and if it is too active it can be diluted to a certain definite strength.

18988. I do not think I made my question quite plain. I am supposing that you have got a quantity of a drug of exactly what you consider the right strength—that you have satisfied yourself of the actual strength by tests on animals?—Yes.

18989. That quantity is sold out to different chemists?—Yes.

18990. Does it retain its standardised strength?—That is the point the Pharmaceutical Society specially asked us to investigate, and we have done it in a number of cases now; it retains its strength for eighteen months, and it is not necessary to go longer.

18991. It would be possible then for any chemist in the country or anywhere else to supply himself, if he went to a proper place, with an amount of this drug or any other similar drug of what was known to be the proper strength?—Yes.

18992. You spoke of taking curare yourself accidentally by the mouth. How is curare generally administered to an animal?—Intravenously. It has no action, of course, when given by the mouth.

18993. You proved that to your own satisfaction?—Yes.

18994. I should just like to ask you one thing before you leave the standardisation of drugs. Is there any pain caused to the animal that is experimented upon, beyond the mere prick of a needle?—I do not think so. Of course we standardise digitalis on frogs which have very little brain.

18995. I ought to have asked you that before, what animals are generally used for standardising drugs?—In the case of digitalis, stropanthus, and squill we use frogs; in the case of ergot we use cats, that is under licence alone and anæsthetised the whole time; and with cannabis indica we use dogs. The drug is injected, the dog becomes intoxicated, it recovers completely, and it is none the worse for it; indeed cannabis indica intoxication is rather pleasant, there is nothing objectionable in it at all. It is greatly used in India as a pleasure.

18996. Then, in point of fact, it comes to this, that in all this standardisation of drugs either the animal is under anæsthetics, or there is no suffering at all?—Yes.

18997. (*Dr. Gaskill.*) I think perhaps some members of the Commission did not quite understand what you meant when you spoke of anæsthetics causing the sensory cells in the spinal cord to be put out of function, while the motor ones still remain, so that reflex actions are still possible?—What I mean is, that you can get a stage in chloroform anæthesia when you can show definitely (this is not a question of opinion at all, it is a question of absolute fact—it is in all the text books of repute) that in the spinal cord the sensory cells are paralysed and the motors are still active.

18998. What do you mean by the sensory cells in the spinal cord. That is what I think the Commission did not quite understand?—There are two sets of cells in the spinal cord, the cells which receive and the cells which give out impulses, and we use from analogy the word sensory to mean receptor—receiving.

18999. Is it possible for reflex action to take place without those sensory cells being involved?—Yes.

19000. So that really the sensory cells, which would be the communication between the cord and the higher centres, would be put out of action; while a simple reflex would still be possible because the motor cells would not be put out of action. That is what you mean?—Yes.

19001. I see that you say in your *précis* that a dose of morphine such as two grains administered to man would lead to deep sleep, from which he is aroused with difficulty, while larger doses are followed by coma and complete unconsciousness?—Yes.

19002. So that you would consider that two grains to a man leads to complete absence of pain?—Yes.

19003. And deep sleep?—Yes.

19004. What I want to know is, does the dose of morphia required bear a distinct relation to the body weight of the animal used?—No, it bears relationship to the amount of brain substance.

19005. And that bears a relation to the body weight?—Yes.

19006. What I mean is that a small dog and a large dog would not require the same dose to produce the same effect?—Certainly not.

19007. Could you draw any relationship between the dose for a man and the dose for a dog, considering the difference in size of the man and the dog?—I should not like to draw any relationship.

19008. But I meant rather if two grains is a dose for a man to produce very deep sleep, would you consider that two grains, or more than two grains, or less than two grains would be required for a dog very much smaller in size than a man?—One grain in a dog, of course, produces a very marked effect. You want much less relatively to the two animals.

19009. Would that be partly on account of the size of the animal and partly on account of the development of brain substance?—Yes.

19010. Both those things would combine in the same relation?—Yes.

19011. And cause a lesser amount of morphia to produce the same effect in a dog as in man.

19012. Then I see that the Hon. Stephen Coleridge included your name in one of the five cases, I think it was, that he prominently brought before us as having infringed the Act—he referred to your paper by Dixon and Brodie in the "Journal of Physiology," Vol. XXIX., No. 2, page 144?—Is that all that was mentioned? I am afraid that I have not read it.

19013. That is what he refers to in his evidence here. The point of it is this, if I might just say what he accuses you of. At No. 10371 he says: "On page 144 these two vivisectors make the following statement: 'In studying these reflexes we have found it of the utmost importance to avoid the use of chloroform or ether as the anæsthetic. The experiments must therefore either be performed upon unanæsthetised animals, upon animals anæsthetised with morphia, or upon decerebrate animals. Our experiments were usually conducted under one of the two latter conditions, but in a few instances were repeated upon animals lightly anæsthetised with chloroform.'" This point is that, as you have said, they must be performed upon unanæsthetised animals, or other, when you speak of light anæsthesia, you really mean unanæsthetised animals. That is what I understand.

(*Dr. Wilson.*) I should say incomplete anæsthesia.

(*Witness.*) I think he ought to have referred in the same paper to the beginning, to page 102 of the same research, which I have here, under the heading of "Animals, Anæsthesia, etc." In that heading it says: "The animals mainly used were cats. With the exception of a few experiments, in which the animals were killed by pithing, all were anæsthetised, usually with ether, but at times with chloroform, or with the ACE mixture. In experiments on dogs we generally administered morphia as well. In a few cases we anæsthetised first with ether and then injected urethane, either intravenously or intraperitoneally." That is the first thing, at the beginning of the paper where one always describes the anæsthetic effect. I know, of course, Mr. Coleridge's Society reported the statement on page 144 to the Home Office, and they replied by pointing out to him this page 102 where we definitely state that all the animals were anæsthetised. If I may just refer to page 144, the words which are quoted are perfectly correct, but in order to understand exactly what they mean, the context is really necessary. These experiments were in connection with asthma, and they were to show that when, by any cause, you excite a certain portion of the septum of the nose, you get constriction of the bronchioles. You never get that constriction of the bronchioles if there is much volatile anæsethetic in the lungs, because a volatile anæsthetic in the lungs paralyses the nerve endings. Accordingly, we state: "The experiments must therefore either be performed upon unanæsthetised animals," which is, of course, quite impossible, first of all because it is not allowed, but also because you cannot do an experiment on an unanæsthetised animal where the whole point is absolute stillness. You must not even let a man walk across the laboratory when an experiment of this kind is going on. The movement of a hair would spoil the whole thing, so we say, "Upon animals anæsthetised with morphia or upon decerebrate animals. Our experiments were usually conducted under one of the two latter conditions"; that is to say, the animals were decerebrate or they will have had chloroform first and then morphin, and then subsequently when everything was quite still, no more chloroform being given, the excitation of the nose was done. The least little bit of sensation of any kind would, of course, immediately have produced a big effect on the animal absolutely vitiating the experiment. That was the experiment. Then we say at the end that the experiments "in a few cases were repeated upon animals lightly anæsthetised with chloroform." There is really nothing to comment upon on that. They were anæsthetised. An ordinary surgeon, when he does an operation, only deeply anæsthetises his patient when he wants to relax the muscles, when he wants all the muscles, say, of the abdomen to be relaxed so that he can feel a tumour at the back of the peritoneal cavity. Then he puts the patient under deep anæsthesia. But in ordinary cases he uses light anæsthesia, and, personally, I think the lighter the anæsthesia the better.

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19014. (*Dr. Gaskell.*) Mr. Coleridge's comment is: "They cover up the anæthetisation, in my opinion, by speaking of them as 'lightly anæsthetised with chloroform,' because if they were anæsthetised properly with chloroform, according to their own description, the experiments would be valueless"?—Of course, if a man says that the animals are not anæsthetised, it is his business to bring evidence to show it. All I can say is that if there were the very slightest movement or were the sensory nerve centres acting in any way, the animal would move, and one movement of any kind would vitiate the whole experiment. May I pause to show you the delicacy of the tracing referred to? It would be impossible to take a thing like that with the slightest movement (*handing in a tracing*).

19015. Then, so far as I understand it, your remark, "The experiments must therefore either be performed upon unanæsthetised animals," etc., did not in the smallest degree imply that you yourself ever performed experiments on unanæsthetised animals?—Certainly not. That is stated clearly at the beginning of the paper. The pages are given on the outside of the book. There are two pages—144 and 102.

19016. Now I think you would like to tell us some few points with respect to the administration of the Act as at present. You have not referred to anything of that sort, although I see it is down in your *précis*. We should like to know what, in your opinion, are the difficulties and the annoyances which the present Act causes to workers?—At Cambridge we encourage the men when they are qualified to come and spend six months in the laboratory learning to observe, doing a piece of useful research work which shall enable them to take their M.D. in research. Those men come, and I suggest some useful object for them to work upon in their six months; but often they cannot start; they are kept delayed sometimes as long as two months waiting about until they get their licence to begin. They lose heart; it is difficult to give them anything to do, and altogether it is a most discouraging thing to the men.

19017. (*Dr. Wilson.*) They have the M.B.; they are qualified?—Yes, they are qualified medical men, but we require a man to show something of his own capacity before we give him the M.D. Then I have had a good deal of correspondence with the Home Office, because I wrote up and asked for certificates for painless experiments, and they replied, "If you want to do painless inoculations you do not want any certificate at all; you can do those as much as you like—they do not come under the Act." But they added, "If you do them you must do them on your own responsibility," which means that one can be prosecuted at any time for doing these experiments, and as one does not quite know how the Act is going to be interpreted, one has, of course, to say that they are not painless, but painful, and you get your certificates. Then at the end of the year, when you send in your report, you have to state whether the experiments performed were painless or painful.

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19018. What sort of experiments are you referring to?—Inoculation experiments.

19019. (*Sir Mackenzie Chalmers.*) The results may be painful?—The results that I am speaking of in these cases were painless.

19020. And you knew they would be painless?—You have moral certainty. For instance, the injection of caffein must be painless; it cannot be otherwise. Yet you must have a licence, and you have to put in at the end that the experiment was after all painless. It seems to me rather absurd to have to go through all that. Of course, the Act at the present time says, "Experiments calculated to give pain." In my opinion most experiments are not calculated to give pain; in fact, in ordinary vivisection none of it is calculated to give pain. I have a perfect right, according to that, to perform experiments without a licence at all, because they are not caluclated to give pain. If they were calculated to give pain you would find very few people who ever performed experiments. Then, as a second point, what I recommend in order to avoid this trouble to be rid of all these complications and make the thing as simple as possible, would be to have one licence and to give a comprehensive licence to the head of a laboratory or to any other responsible person.

19021. (*Dr. Gaskell.*) Do you mean by that to abolish all certificates?—Yes.

19022. But upon the licence you would state the experiment that was intended to be done?—No, I should not. I should give a complete comprehensive licence to responsible persons, heads of laboratories; and to workers, to people who were not so responsible or who were not heads of laboratories, and so on, I would give an endorsed licence, only allowing them to do certain experiments, and indeed only doing those experiments under the control or supervision of someone who had had more experience in research than they had.

19023. Again without any special certificates?—Yes.

19024. In fact, you favour the abolition of the certificates altogether?—Altogether.

19025. Then, I suppose, you would require, were such licence to be given, that a report of the whole experiment should be sent into the Home Office at the completion of the experiment?—Certainly, of everything that was done. I think that would be an excellent provision. At the present time, supposing you start a research, it does not matter what it is, it never leads to where you think it is going. As soon as you get into it you find that there are branches in all directions. You have to follow one branch; you have to use different animals from those you put in your certificate. There is delay of a fortnight, three weeks, or a month, it may be, before you get that certificate back. You may have to perform inoculations interperitoneally instead of subcutaneously, and there is more delay again in getting fresh certificates. At the present time I have had, I think, several certificates A, each fresh one allowing me to do a different thing. I have had a number of Certificates A—I do not remember the exact number.

19026. With respect to these senior students, would you restrict this licence limited by endorsements to students who had already taken a degree, or would you allow it to students who were in *statu pupillari*?—If the experiments are done under the control and supervision of a responsible person, I do not see why any intelligent and educated man should not be a fit and proper person to do them, whether he happened to have a degree or not.

19027. It is hardly necessary to ask you would you consider it for the benefit of the men themselves, who are sending in these theses for M.D., this form of research is valuable and good for humanity at large?—I think it is one of the most valuable parts of their education. I am quite sure that the men who have done six months' research work look upon the subject in an entirely different fashion. They have learned in that six months to observe; they see things in the simplest experiments that they never observed before.

19028. And you would rather encourage an increase in the number of advanced students doing the research work of this kind than discourage it?—Certainly, I would thoroughly encourage it.

19029. (*Sir Mackenzie Chalmers.*) I suppose that if advanced students were allowed to do research work you would impose this condition, that it should be done under the immediate supervision of the head of the laboratory, and that the anæsthetic should be given by some person who understands anæsthetics?—Yes, I think that is most important. With regard to anæsthetics, anæsthesia at the present time is a very simple process. We do not use chloroform; it is altogether too much trouble; you have to be continually looking at your animal and its reflex the whole time, adding a little more and stopping it.

19030. You prefer urethane?—I use urethane.

19031. Because the worst that can happen is that the animal if kept too long under the anæsthetic may die?—Yes.

19032. Have you ever had to perform a painful experiment on an animal without any anæsthetic?—Never. I have never seen a painful experiment on an animal.

19033. Without anæsthetics?—No, I have never seen a painful experiment on an animal.

19034. (*Mr. Ram.*) Would you advocate students being allowed to vivisect animals under a lethal dose of an anæsthetic in order to acquire manual dexterity?—No, I would not under licence.

19035. You think that manual dexterity can be sufficiently acquired without using animals?—Do you mean for surgical operations?

19036. Yes?—It is a very large question, I think. It is used at the present time largely in America. The man has to go out into practice. Where does he get his practice now? He gets it by practising on man. A country practitioner who is qualified, and gets his M.D. at Cambridge or London, may have had no experience, perhaps he has been house surgeon; but wherever he goes he has to learn to do his surgery on human beings.

19037. That is why I asked the question. Do you think it would be well to allow the use of animals under a lethal dose of anæsthetic to enable students who are perhaps qualified medical men to gain manual dexterity in the performance of operations on human creatures?—Yes. I think it would be a wise provision to make for the sake of humanity.

19038. Do you think that experimenting on the bodies of animals would give such manual dexterity? Is the formation of an animal's body so similar to that of a human body that a man by experimenting on an animal could gain manual dexterity when he came to operate on human beings?—I am quite sure that he could. I am quite certain on that point.

19039. (*Mr. Tomkinson.*) You claim that morphin and similar narcotics are an absolutely satisfactory preventive of sensation if properly administered?—Yes.

19040. Their action is first upon the sensory nerves?—Yes, the sensory cells.

19041. And afterwards upon the motor nerves?—Are you referring to morphin now?

19042. Yes?—In the case of morphin you want very large doses to affect the motor cells. I made a distinct difference between morphin and the alcohol anæsthetics. I said that there are two groups, the morphin group and the alcohol group.

19043. Is the first symptom something approaching stupefaction?—Yes.

19044. Stupefaction or very deep sleep?—Yes.

19045. Then you claim that the sensory nerves are rendered insensitive to pain?—The sensory cells become quite inactive.

19046. Although there might still be motion?—Although you can still get reflexes. The motor cells are intact.

19047. Then before the discovery of chloroform was it used?—Yes, it was to a limited extent.

19048. To any general extent?—I do not think it was used to any general extent.

19049. Was not the discovery of chloroform hailed as one of the most beneficent discoveries, and the first real alleviation of the horrors of the hospital?—Certainly, quite commonly operations were done without any anæsthetic previously.

19050. And it was generally accepted that an operation was a terrible ordeal; that amputations of limbs

and other operations were so terrible that people could not survive them or live under them?—Yes.

19051. Morphia was not recognised then certainly as anything at all in the same class?—The point about morphia, you see, is that you dare not give it beyond a certain dose, because the respiratory centre gives out. In animals it does not matter if it does give out, because you can do artificial respiration.

19052. (*Dr. Gaskell.*) In those days was it not given by the mouth as laudanum? Did the hypodermic syringe exist in those days?—No; and then the morphin took a huge time to act. It was not morphin; it was opium.

19053. (*Mr. Tomkinson.*) The idea of instilling it through a vein or the skin was not known?—No.

19054. And that was the great difference?—That was one great difference.

19055. Then curare is an anæsthetic in a large dose. I understand you to say that to be effective it must not be given through the mouth in the way that you accidentally took it, but instilled by inoculation?—Yes.

19056. Then it becomes immediately effective?—Then it becomes immediately effective.

19057. I did not quite grasp what was the first step in the experiments in regard to wood poisoning by which animals were utilised. The men were suffering from a certain complaint?—Yes.

19058. Was a drop of their blood taken?—No, the wood was sent to us, the shuttle wood, in the form of sawdust, and the alkaloids were extracted from the wood; then we tried the action of the alkaloids on animals, and the effect on animals was exactly similar to the effects that were being produced on men.

19059. By inoculation?—Yes.

19060. That was not a painful experiment, nor did the animals suffer much?—No.

19061. But they showed the same symptoms that the men suffered from?—Yes.

19062. In regard to the causing of death by blowing air into the vein, would that be a very painful death to an animal not under anæsthesia? It stops the action of the heart, I understand?—Yes, immediately. It would be just as if you were shot, I take it. It is an immediate cessation of the heart's action. There might be a moment's pain. I could not say.

19063. Is that the common way of destroying animals before they recover consciousness?—I think it is the quickest and easiest way by which you can kill them. Sometimes I kill them before the class with chloroform to show them exactly what happens to the heart and respiration. I might sometimes kill them with morphin.

19064. When you speak of the brain being paralysed, does that mean insensibility to pain?—Yes.

19065. That is the form that the anæsthetic takes?—Yes.

19066. It paralyses the brain and arrests its receptivity?—Yes, it cannot receive any impulses from outside, and so the animal or man becomes unconscious.

19067. With regard to cocaine as a local anæsthetic, that is introduced through the skin?—Yes, subcutaneously.

19068. At the part which it is desired to anæsthetise?—Yes, it is injected under the skin.

19069. For a local operation?—Yes.

19070. How deep will it extend?—The anæsthesia extends to the muscles. The muscles are not very sensitive, of course. It would not extend far beyond the skin.

19071. We have had it stated here by one witness on the subject of veterinary operations on a farm, that he sometimes uses it in the case of castration, in your opinion would it be a tolerably effective anæsthetic in that case?—I think it would, because there is such a lot of cellular tissue there; there are no muscles, and it would very readily get at all the nerves.

19072. Is it introduced by injection?—Simply by injecting it into the scrotum.

19073. You spoke of injecting into the spinal cord. That would be rather an important operation, would it not?—No, it is quite painless. You simply, if it is a man, make him sit up and bend forward, and you take the needle and put it in; it has to go in quite a long way, a surprisingly long way, until you feel the bone or front part of the spinal cord, then you withdraw it a little, and you get the spinal fluid coming out. Then you fix your syringe on to the needle and squirt as much of the local anæsthetic as you require into the fluid of the spinal cord.

19074. (*Mr. Ram.*) Have you done that on man without anæsthetics?—Yes.

19075. And was there any pain?—No, no pain at all.

19076. Just the prick of a needle?—Just the prick of a needle.

19077. (*Mr. Tomkinson.*) You strongly deny and protest against the idea of demoralisation being caused by familiarity with vivisection experiments?—Certainly. Familiarity with pain would be quite a different matter in my opinion.

19078. (*Mr. Tomkinson.*) Have you any knowledge of the system carried on in Paris, I believe without anæsthetics?—No, I have no knowledge of it.

19079. Have you never heard of the Veterinary Surgical Establishment in Paris?—Are you referring to the Pasteur Institute?

19080. At Alfort, where experiments were carried out upon living horses in the Paris veterinary schools?—I am afraid I know nothing about it.

19081. With regard to the question of demoralisation resulting from familiarity with vivisection experiments, it has been stated by an English veterinary surgeon, who had gone over there to study, that the demoralisation of the French students and their apparent entire indifference to the torture of animals, was something too terrible for him to even stand?—I do not know the fact; I have not heard of it, but I can quite believe it. But, of course, there is no comparison. No student in England has ever seen pain in an animal experiment.

19082. Then your position is that you totally deny that under the system, as carried out in England, any pain is inflicted?—Certainly. My students, practically all of them, are graduates of the University, and most of them have taken an honours school previously. Surely one would not show a man of that stamp experiments involving pain.

19083. Not if they could help it?—They would be the first to protest if such a thing were possible even.

19084. You are aware, I suppose, that there is on the part of the public, rightly or wrongly, considerable doubt and scepticism on the point of there being no pain inflicted in vivisection experiments?—Of course.

19085. Would it, in your opinion, be satisfactory if there were to be a freer right of entry or permission of entry, to witness these experiments by independent, properly accredited witnesses, in order to reassure the public, if possible, on that point. We all know that certain people are prepared to observe what they wish to observe?—It is a perfectly common trait in all forms of life. If the people were responsible people, that is to say, if they had had a medical education, I should not object at all.

19086. I purposely said, I think, properly accredited people, not violent partisans who went there in order to make a case?—Precisely. Then I should be the last person to object; whenever I do an experiment before a class, I have perhaps 40 or 50 witnesses.

19087. (*Sir Mackenzie Chalmers.*) Do you have any of your advanced students at your research experiments?—Yes.

19088. Besides yourself, when you are doing a research experiment, there are a certain advanced students allowed to be present?—I have a considerable number of advanced students working in the laboratory doing these experiments.

19089. Under licences?—Yes, six or seven, I should think.

19090. They are licensees and qualified men?—Yes.

19091. What I meant was, when you are doing research experiments, are they always done absolutely in the presence only of yourself and a laboratory assistant, or are advanced students allowed to be present and see what goes on?—I never do them alone.

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When I do an experiment, it is such an educational advantage to other people to come in and see it that I should be the last person to prevent their coming in. Any one who wishes can always come in.

19092. (*Dr. Gaskell.*) The question is, do they?—Yes, they do, certainly.

19093. (*Sir Mackenzie Chalmers.*) They do, in fact, advanced students do come in and see the experiments?—Certainly.

19094. Then we have not only your word for it (I do not say it in any offensive sense), that these animals are properly anæsthetised, but we have got certain other qualified persons there who are also able to judge whether the animal is properly anæsthetised?—Yes, at practically every experiment.

19095. (*Dr. Wilson.*) You would still agree that experiments on animals should be regulated by Act of Parliament in this country?—I certainly think so. You might have all kinds of irresponsible people doing them otherwise.

19096. You also contend that certificates should be abolished?—Yes, they lead to continual complication and bother. A man might quite easily, unless you take the most stringent preventives, do experiments under the present complicated system which he was not entitled to do.

19097. You object to them on account of the delay which is sometimes entailed on application to the Home Office?—The delay and the complication.

19098. Now, as the General Medical Council is responsible for the education of medical students throughout the kingdom, would you be prepared to advocate that that Council should be held responsible for the issuing of licences rather than the Home Office? The Home Office has to see that the Act is carried out, but in difficult cases the inspector is consulted as to whether it is advisable to permit experiments or not?—Yes, I think it would be a very good plan that some responsible body of that description should have the power of issuing licences. I had not thought of it before.

19099. Are all the experiments carried on in your class room on animals painless experiments?—Yes, all painless.

19100. And it is perfectly understood by your class, I suppose, that they all must be painless experiments?—Yes, we always discuss it. I discuss with my class the anæsthetic which the animal is under and the men all come up and examine it; and they learn from that fact alone. And then we go on with the experiment. I think it is a most instructive thing.

19101. Would you object, then, to a notice being put up in the class room to the effect that "all experiments carried out on animals in this class room shall be painless," because I suppose a good many students do not know much about the Act?—I suppose that a good number of them do not, naturally.

19102. Would you object to such a notice?—I should have no inherent objection. I do not know whether I should approve of it.

19103. Would you be prepared to certify that, to the best of your belief and endeavour, for example, all experiments which you have been carrying out have been carried out painlessly, in reporting to the Home Office?—Yes.

19104. Would you be prepared to append a certificate to that effect?—I always do append a certificate to that effect. I think in two or three cases I put down that they had experienced "slight pain," or something of that kind. After a subcutaneous injection of some drug an animal feels ill after it, and you have to say that it feels pain.

19105. Now with regard to the experiments carried on for the standardisation of drugs, those experiments, I suppose, are carried out for the Pharmaceutical Society?—They have been. We have done them simply to try these special drugs. This standardisation of these drugs was a new thing, and we did it in order to show how variable these drugs were naturally.

19106. But certain manufacturing chemists, I suppose, employ experts to carry out experiments for them?—Yes, I believe they do.

19107. There are only, I suppose, a very few drugs about which there is any real doubt as to their physiological action?—As to their strength. The more important ones, far and away more important than any others, are, of course, digitalis and ergot and stropanthus.

19108. And so long as these drugs are used you contend so long must experiments be carried out on animals to standardise them?—If a time arose when we understood sufficiently of the chemistry to estimate the active bodies chemically, then the need for animal experiment would disappear at once.

19109. But until that time arrives you must continue to experiment on animals?—Yes, it is the only way.

19110. In experimenting upon man, ergot is generally given by the mouth, is it not?—Yes.

19111. Always?—No not always, it is sometimes injected. In fact, if you want to be sure of its action in these days, and you want a quick action you inject ernutin or ergotoxin; you then get its action quickly and certainly.

19112. And are these preparations as reliable as ergot itself?—They are standardised.

19113. With regard to those experiments on African boxwood, of course, the alkaloid was absorbed by the workmen during the process of manufacture by inhalation?—By inhalation.

19114. You did not try any experiments by using the sawdust on animals in that way?—I made beds of sawdust for the animals and they did not show any ill effects at all. But then these people work in a cloud of dust—you cannot see across the room I am told; it is an absolute cloud of dust; one could not be continually shaking up an animal's bed.

19115. But with the perspiration there might be some absorption through the skin?—I do not think so. Drugs are not absorbed through the skin except under very rare circumstances.

19116. (*Sir William Collins.*) I understand you to suggest the abandonment of certificates under the Act, but that you think it would be well to retain the practice of licensing under the Act?—Yes.

19117. May I ask whether that is in the interests of the public or of the animals or of those who practise vivisection?—I should say that it was on behalf of the public and those who practise vivisection. I do not think the animals come into it. I do not think they are affected. They would be the same in either case.

19118. The advantage then would be on behalf of the public and those who practise vivisection?—Certainly.

19119. What advantage do you think accrues to the public?—That you are able to do researches quickly and able to do them more efficiently, and therefore, as all the results are for the good of the public, they must benefit.

19120. But why are you able to do researches more quickly if you require licensing than if you do not?—The trouble at the present time is with the complication of certificates.

19121. You did not understand me. I am agreeing for the moment, for the sake of argument, with your suggestion that there should be no certificate. I understand that you suggest that there should be a licence under the Act?—Yes.

19122. What advantage is it to the public that there should be a licence under the Act as against having no licence at all?—As against having no licence at all, I beg your pardon, I misunderstood your question from the beginning.

19123. I thought so. Perhaps I had better put it from the beginning again?—Thank you.

19124. I understand that you suggest the abandonment of certificates, but that you think it well to retain the practice of licensing under Act of Parliament?—Yes.

19125. Is such licence in your opinion to the advantage of the public, or of the animals, or of those who practise vivisection?—It is to the advantage, I think, of everybody.

19126. Will you tell me in what way you think it advantageous to the animals?—Because you might have irresponsible persons—anyone could do it—making experiments. I do not think that irresponsible persons should be allowed to do that, and I do not think that experiments should be made except in proper places.

19127. Do you think that there were irresponsible persons making these experiments in this country before the Act of 1876?—I think there is not the slightest doubt about it.

19128. (*Sir William Church.*) Before 1876?—Yes, before the present Act. I should say there is not the slightest doubt about it.

19129. (*Sir William Collins.*) It has been stated before the Commission that the Act was superfluous because there was no occasion for legislation?—It is a question that I have not looked up. I am not prepared to give you facts and figures, but I think so far as I remember my reading that there is no doubt that unnecessary experiments were done before that time.

19130. Do you think that the effect of the Act has been to reduce the amount of vivisection in this country?—Undoubtedly; especially directly after the Act; it absolutely stopped a long series of experiments that were going on. Very many people were unable to continue their experiments entirely owing to the difficulty of the Act, and researches were allowed to lapse for a time.

19131. Do you think that there has been less vivisection this year than there was in the last year before the Act of 1876—that is, in the year 1875—in this country?—No, much more. But you take into consideration the leaps and bounds that medicine has been making in this time. It is just the time that medicine has been making those great advances.

19132. Do you think that if the Act were repealed now that there would be danger of animals being unnecessarily employed for purposes of vivisection?—I think the medical profession would be strongly against the Act being repealed now, because, as I say, any medical student, any irresponsible person, could make all kinds of experiments if he wished to do so at his private house.

19133. We have had one physiologist here present who said, and I think he said his opinion is shared by others, that the Act had operated altogether antagonistically to the advance of physiology. Is that your opinion?—I do not agree with that.

19134. Then you think it would be unsafe to repeal the Act for fear of unnecessary and irresponsible vivisection?—I think so, certainly.

19135. Would you tell me what advantage you think the Act secures to the public?—I think it does, or at all events ought to, give them confidence that animals are not unnecessarily subjected to pain.

19136. And what advantage do you think it secures to those who practise vivisection?—To those who legitimately practise it, those responsible persons, I do not think it makes any difference, but they would be subjected to less abuse if it was known that there was an Act in existence than otherwise, of course.

19137. You think that the Act affords protection and security to those who practise vivisection?—Undoubtedly. We are not liable to be wantonly prosecuted; there is no doubt about it; it forms, of course, a very great protection.

19138. Then you would not agree with those medical witnesses who think that it would be a matter of indifference to those who practise vivisection if the Act were abolished altogether?—No. I should like to see some kind of Act still continue.

19139. You do not think it should be described as an Act that the vivisectionists are ashamed of?—No, certainly not.

19140-1. (*Mr. Ram.*) You spoke just now of certain experiments which were in course of being made when the present Act passed, and which had to be suspended or stopped in consequence of that Act. Can you tell us how did those experiments militate against the Act, so that the Act caused their suspension?—I have not followed the experiments. I was referring to Sir Lauder Brunton's experiments for one, on snake poisoning, and then there were some other experiments.

19142. Did they involve such suffering to animals as the Act forbade?—No, but as I understand it, there was very much more difficulty in obtaining certificates in those times than there has been in more recent times.

19143. Then the difficulty, I understand, so far as your recollection goes, is not that they were experiments which would now be illegal?—No.

19144. But they were experiments which, because the machinery had not got into operation, were delayed in consequence of the failure to obtain the necessary permission?—That is what I believe.

19145. (*Dr. Gaskell.*) Do you consider that before 1876 there were irresponsible persons who practised vivisection for mere curiosity, or because they took delight in cruelty? You speak of irresponsible persons who, you think, did vivisection before the Act was passed, but I am curious to know what class of person you think they were?—I should say they did it from curiosity.

19146. Do you think they were connected with the medical profession?—No, I think they were not necessarily connected with the medical profession. It is the medical profession who want the Act, in order to protect themselves.

19147. But is this remark, may I ask, that you have just made about the possibility of such persons existing simply a pious opinion of yours, or have you anything to go upon?—It is absolutely nothing that I know; it is simply a possibility.

19148. (*Sir William Collins.*) Would not that class of irresponsible persons that you have in your mind be amenable to the ordinary Act for the Prevention of Cruelty to Animals, if this Act had not been passed?—Yes, I think they would.

19149. So that for the prevention of that form of irresponsible vivisection, the ordinary law would have sufficed?—Yes, that is so.

19150. (*Sir William Church.*) I should like just to ask very much the same question that Dr. Gaskell has asked you. You meant that there was a possibility before the Act for irresponsible persons practising vivisection?—Yes.

19151. I understood you at first to say that you were of opinion that superfluous experiments were done before the Act?—No, I meant that there was a possibility; of course, there was no control.

19152. That is a very different thing, I might say I should like also to ask you a question in relation to what Dr. Wilson asked you. He asked you if you were in favour of the General Medical Council, rather than the Home Office, being the body which should grant licences and certificates, and you said that you would be in favour of a body. But he, I think, referred to the existing General Medical Council. You would not consider that, as now constituted, with the duties that it has thrown upon it by Act of Parliament, it would be a good body to refer to?—Not as it exists at the present time, because, of course, there are not necessarily the right people members of it, but there should be a reference to some responsible body, which should be composed, of course, of people who were, at least some of them, experts.

19153. Do you think that the Home Office is likely to get assistance from the Association for the Advance of Medicine by Research? You know that all applications for licences go before that body? Yes, certainly, because that body practically consists of the highest scientific opinions in the country.

19154. On the other hand, witnesses have stated before us here, that they are the worst possible body, because they are vivisectors, and so strongly in favour of vivisection themselves?—Of course, in a case of that kind, it is absolutely necessary that those giving the advice should be the people who know.

19155. That leads me on to what I want to ask you. Therefore you feel sure that it would cause very great dissatisfaction among all physiologists and researchers into all forms of experimental investigation by means of animals, if the question of granting a licence for special work was not referred to a body of what I may call experts in that subject?—To a body who are acquainted with the needs of the subject, certainly.

19156. (*Sir William Collins.*) Reverting for one moment to the question I asked you before, and putting aside the irresponsible persons with whom you have dealt, I should like to understand from you what

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advantage you think it is to the public to retain the licensing under the Act, in dealing with responsible vivisectors; do you think that the present inspection and machinery under the Act give any real security to the public?—Certainly.

19157. Have you known of instances in which inspectors have detected irregularities?—No, because I do not see how irregularities could occur in a licensed laboratory. If a man has got his licence, I do not see what he can do that is seriously wrong if he does not exceed his licence and certificate.

19158. Then what is the nature of the security that the public receive as the result of such inspection and machinery under the Act?—In what way do you mean?

19159. Do you think that the inspection, as carried out, of vivisection by responsible persons gives any security to the public?—Yes, it gives them a sense of security, I admit.

19160. Can you tell me of any case of irregularity which has been detected thereby?—No, there have been none in my laboratory.

19161. Do you think, as we have been told by other witnesses, that we might reasonably trust to the responsible vivisectors under the Act without inspection?—Certainly, I think so; but at the same time there is no objection to any number of inspectors. Practically whenever one does an experiment, although there are a great number of unofficial inspectors, it is perfectly true that there is not an official inspector. It would make no difference whether the inspector happened to have an official mark on him or not.

19162. Then what is the value of the official inspection to anybody?—I do not know that there is any special value as regards his inspections. I am referring entirely to the vivisection as I explained in the last answer.

19163. (*Sir William Church.*) When the inspector visits your laboratory he does a great deal more than merely walk into the laboratory, does he not?—Yes.

19164. Would you just mention in what the visit of the inspector generally consist?—The inspector, as it happens, has come in several times just when I have been doing an experiment. He has come in twice to my knowledge when I have been giving a demonstration—in the middle of a demonstration to the men. He always examines the animal with interest, and he waits until the demonstration is over, observes the result of the demonstration, and then examines the animal, and sees it killed. Those are exceptional occasions, because he does not usually hit off the time when there is an experiment on. Under ordinary conditions he comes and perhaps asks to see the laboratory book, and asks to see the experiments going on. He sees every worker in the laboratory if he is about. He inquires after the animals, sees every animal under experiment, and sees the condition of the cages in which they are kept; and he would, I presume, make objection if they were not properly housed, and so on.

19165. Therefore he is a security to the public that these animals are kept in a condition in which they suffer no pain before they are experimented upon, and that they are kept in such a condition as to be as healthy as they can be, apparently, under the circumstances after they are experimented on?—Yes, I suppose he is a security to the public in that way. It would make absolutely no difference to any conscientious man whether there was inspection or not.

19166. That is an important part of the inspector's duty, is it not, that he should inspect the condition of the laboratory and of the premises connected with it?—Yes, to see that the animals' cages are suitable ones and that kind of thing.

19167. (*Sir William Collins.*) After that explanation may I again repeat my question: What is the value of the official inspection to anybody?—I do not quite know what answer you require. The value of inspection is to give the public a sense of security that everything is being done according to the Act.

19168. But I understand you to add in one of your answers to Sir William Church that no one would make any difference if he had not an inspector?—No, naturally not, because the people who vivisect under the Act are not criminals nor have they criminal instincts.

19169. So that what is the value of the official inspection?—To give the public a sense of security.

19170. Is it superfluous so far as the vivisectors are concerned?—I think so. Of course, it is conceivable (anything is conceivable) that you might get an unscrupulous and brutal vivisector, as you might get an unscrupulous and brutal man in any profession.

19171. But you think, that the Act gives valuable protection and security to those who practise vivisection?—Yes, I think that.

19172. Is that, do you think, one of the most important reasons for retaining it?—It is a reason for retaining it. And I gave my other reasons before, that some sort of Act ought, I certainly think, to be in existence, as I said, to prevent irresponsible persons, unsuitable persons, from obtaining a licence. There are lots of men to whom we would not give a licence.

19173. Who have applied for them?—Before they apply they would come to me and I should regard them as unsuitable men, and I would not have them in the laboratory, as not being the proper kind of men to do the work.

19174. Have many unsuitable persons applied to you for assistance to obtain a licence under the Act?—I would not say many.

19175. Have any unsuitable persons applied to you for assistance to obtain a licence under the Act?—They have approached me.

19176. How many?—I am not prepared to give any number.

19177. (*Dr. Wilson.*) Is it on account of their inhumanity, may I ask?—No, because the only men who do this kind of work are really what I call the very best men. We use the very best men.

19178. (*Sir William Collins.*) I should have liked to have known if you could have told me whether one, two, or many unsuitable persons had applied to you for assistance to obtain a licence?—People come and talk to you and say, I should like to do a piece of work on such and such a subject, and you do not say, "I am not going to have you in the laboratory"; you are not so foolish as that. You are tactful and suggest they should do something else.

19179. You would not like to tell me how many of those persons there are?—I could not tell you. I do not suppose I could remember.

19180. (*Mr. Ram.*) With respect to inspection you, as I understand, think that with regard to all the persons, and all the licensees you know, their conduct is such that they are not restrained from misconduct by the presence of the inspector?—That is my point.

19181. On the other hand, very many members of the public think that there is either danger that they might act illegally, or, at any rate, it is desirable that exactly what they do do should be known.?—Yes.

19182. In other words, that exactly what does take place should be known?—Yes.

19183. Does the presence of the inspector in your opinion give such security to the public?—Yes, I think it does. That was my point.

19184. Therefore, although in your opinion such security may not be necessary, for the sake of those who do not share your view, it is desirable as you think to retain inspection in order that such persons may receive the security which inspection may give them?—Certainly, that is my view.

19185. (*Sir Mackenzie Chalmers.*) Take, for instance, the inspection of factories. A well-conducted factory is inspected just as much as any other factory?—Yes.

19186. And it is desirable that all factories should be inspected, and, therefore, is it not desirable that all laboratories should be inspected?—Yes, I think it is for the same reason.

19187. (*Sir William Collins.*) Have you heard of any ill-conducted laboratory in this country?—I have not heard of any.

19188. (*Dr. Wilson.*) But, after all, in spite of any amount of inspection, the question of painless experi-

ment depends entirely on the experimenter, does it not?—Entirely.

19189. That is to say, in some experiments, when artificial respiration is being carried on, the inspector really could not tell?—You mean with curare?

19190. No, with any anæsthetic, except on your own assurance that the animal was thoroughly under anæsthesia during the whole experiment?—Yes, certainly he could. The animal would have no reflexes; exactly the same as with man, you can tell if a man is under an anæsthetic.

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19191. But with curare it would be difficult?—With curare he would simply know that you had administered the anæsthetic, and he would be able to tell from the blood pressure. Those are the two points.

FORTY-SECOND DAY.

Tuesday, 10th December 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. CHARLES R. J. A. SWAN, M.B., M.R.C.S., L.R.C.P., called in and Examined.

Mr. C. R. J. A. Swan, M.B., M.R.C.S., L.R.C.P. 10 Dec. 1907.

19192. (*Chairman.*) You live at 3, Chester Place, Hyde Park?—Yes.

19192A. Do you practise in London?—I have practised for the last fifteen or sixteen years.

19193. As a physician or surgeon?—I do not do any surgery; I know nothing about it, I am afraid.

19194. You are a Bachelor of Medicine, and also of Surgery of Oxford?—Yes.

19195. And you have various other qualifications?—I believe I was examined by one of the Commissioners, Sir William Church.

19196. You are a member of the Royal College of Surgeons, and a Licentiate of the Royal College of Physicians?—Yes.

19197. And Consulting Physician to the Consumption Home, Gloucester Place?—Yes, it is only a small hospital.

19198. You are appearing here at the request of the Canine Defence League?—Yes, they wrote to me and asked me if I would come.

19199. You have come to give us evidence in favour of the opinion that dogs ought not to be subjected to vivisection, whatever may be the case with other animals?—Yes. I do not want it to go out to the world in general that I am one of those people who say that vivisection must not be. I think it is a painful necessity, and we cannot blink that fact.

19200. You think that experiments upon live animals properly conducted are essential to the medical profession?—Yes, they are.

19201. I will bring you at once then to the point that you are going to speak on, viz., Why the dog should be exceptionally treated. The two main points appear to be, first, the question of ethics or sentiment, or whatever you like to call it, in the case of an animal so much the companion of man?—Yes, that one gets so attached to.

19202. And the other is as to whether or not the examination of the living dog is of special importance, having regard to its resemblance in its construction in some respects and its want of resemblance in others, to the construction of man?—Yes.

19203. As regards the first point, I understand from what you have said about your love of dogs that that influences you?—Yes. I admit it is to a great extent a sentimental feeling, and, if I may so, what I am anxious to know is, whether the Commission could not find some means of using other animals and obtaining the same beneficial results to mankind, letting the dogs alone.

19204. That comes rather to the other head that I was speaking of as to the special qualifications or usefulness of dogs for experiments. As regards your feeling about it, I do not think we need ask you more; we quite understand that view, and it has been put before us at considerable length very often. You have that feeling yourself very strongly?—I have.

19205. As regards the usefulness of dogs for the purpose of experiment, that depends a good deal, no doubt, upon how far the dog resembles man in its structure?—Yes.

19206. We have had a good many eminent witnesses before us who have pointed out that in certain parts of their structure dogs do resemble man very nearly, and that in others they differ from him, and therefore are not specially valuable in respect of certain experiments; would you give us your view about that?—Going back to one's early days, I have seen the old muscle and nerve experiments, what we call the classical muscle and nerve preparation experiment. We did them first of all on frogs, which was perfectly simple. You isolated your sciatic and stimulated it and made the whole experiment work. Then I remember at one time it was tried on a dog at Oxford and it was a very difficult matter to see; there was a mass of blood about, and if I had not known my procedure and learnt it from books and diagrams first, I do not think I should have been much the wiser. I remember then it struck me how much simpler it was to take a lowly-organised animal like a frog to do your simple experiment. I thought, when I get qualified one day, if ever I do, I will stick to that way and not use dogs for demonstration purposes before students if one can avoid it.

19207. You never have been licensed yourself?—No.

19208. Nor have you performed experiments of this kind?—Only on a frog. The laboratory arrangements at Oxford were that we had all our frogs, and pithed them or not, as we liked, and made muscle and nerve preparations with Burdon Sanderson.

19209. It was not what you would call a very serious training in physiological experiment?—It was all we had at the beginning.

19210. (*Sir Mackenzie Chalmers.*) How long ago are you speaking of; ten years ago?—In 1883. I began my scientific work at Oxford the same day that Burdon Sanderson came into the laboratory for his first lecture. Then there was another time when we were doing a pressure experiment, a manometer experiment on the jugular, that was done, I believe, on a dog. I know it was a warm-blooded animal, and I frankly confess that I may have been stupid or dense, but it was impossible to see the mercury rising or falling properly in the manometer.

19211. How many were present?—I should think about ten.

19212. And only the physiologist himself who was performing the experiment could see it. It may be difficult sometimes for the pupils to see?—That is a thing I could not be sure of. If I may state my own

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conviction about it, it is that errors in manipulation are so manifold and easy, that you are very likely to get a wrong result; then you repeat your experiment, and go on till you get your results to agree with what they ought to be.

19213. (*Mr. Ram.*) Have not things improved very much since those days?—I am not in a position to say, and I do not want to dogmatise on what I am not qualified to dogmatise upon.

19214. (*Chairman.*) That experiment that you were last speaking of, on the blood pressure, is an experiment for which the dog is said to be particularly fitted?—Yes, but my own feeling is that one now understands the whole thing, and I am perfectly willing to accept the fact that that experiment has been done—the vagus stimulated and the blood pressure made to fall—and for the purpose of teaching other students I should be anxious that it should be taught by the aid of diagrams.

19215. Of course, demonstration before students is a branch of the question only?—Yes.

19216. But do you consider now with your present experience that experimenting on dogs with reference to blood pressure, always under anaesthetics, of course is a desirable thing?—If it is done under anaesthetics I do not think one ought to be inclined to veto it.

19217. And that the results of such experiments on dogs are more useful, or more possible one ought to say perhaps, than results by examining rabbits and guinea-pigs?—I cannot see why rabbits should not be as useful. I have thought it over anatomically, and I do not see why the distribution of your vagus should not be equally effective in a rabbit. I may be wrong in my histology, my minute anatomy; but that is my idea. You can get at the jugular of the rabbit easily enough.

19218. Has not size something to do with it. The dog that would be used for this purpose would be larger than a rabbit?—You mean on account of a clot of blood spoiling the tube?

19219. I am not myself a medical man, and I would not like to express it in detail, but I am giving you the views which were given by some witnesses that we have had before us?—I cannot honestly say that I do not think you could do it as well with a rabbit. I should think that you could do it with a big rabbit. I have never seen it tried.

19220. What you say then is that the dog is an animal in which the blood pressure can be watched and tested as satisfactorily as in any other animal, but whether it could not be just as well done on a rabbit you are not prepared to say?—That is so. I am not trying to hedge in my evidence. I want you to understand exactly what I mean.

19221. That is just why I have summed up what I understood you to say, to see whether you assent to it. Then, as regards questions relating to the digestive and nutritive organs, is the dog suitable for those?—I should think so, but without wanting to be conceited about it, I have managed to estimate the condition of my own and other people's stomachs with perfect comfort without making a gastric fistula.

19222. Without making it on an animal, do you mean?—Yes. I estimate the condition of my own stomach for experimental purposes often.

19223. I was not aware that it was the practice of the profession to make gastric fistulæ for the purpose of examining what the state of a patient's stomach is?—I did not mean that. My idea is that there are cases which are quoted in lectures by those who wish to state what is going on in the stomach. There was an old experiment, as the member of my profession who is present will remember, in which you made a gastric fistula of a dog, and held a piece of fried bacon to the dog's nose to see the gastric juice spread to the fistula, and it struck me that you could just as well do that by syphoning. I have made a great hobby of the treatment of gastric disturbances, and I constantly sample the contents of my own stomach to see how it is going on.

19224. That is hardly what you would call a painful experiment?—No, but my idea is that you can arrive at the benefit on yourself and your friends just as easily without experimenting on a dog with a gastric fistula.

19225. Do you mean by that for the purpose of physiological study and research?—With a view to treating mankind. I thought that all research was really with the object of treating mankind in the end.

19226. I suppose that is the pathological side of it, but then there is the purely physiological side, is there not?—I only look at it from the chemical side.

19227. But not necessarily with a view to the immediate result of a particular experiment which shall give you a particular remedy, but that by greater knowledge of the working of the human frame and of its diseases, you can get knowledge which ultimately will prove useful. It does not follow that the physiologist, as I understand from his own account of it, is necessarily performing the experiment for the express and immediate purpose of the discovery of a remedy for some known disease, but because it is very important to know how the machine works. Is not that so?—My view is that you can ascertain just as well how the machine is working by syphoning the stomach at different intervals.

19228. (*Sir William Collins.*) Is your point that any light which may have been thrown upon the human digestion by experiment on dogs by way of gastric fistula could equally have been obtained by syphonage on man?—That is my view, whether right or wrong, and it is backed by my having experimented a great deal on myself, and my friends occasionally, by means of which I have been able to treat cases.

19229. (*Chairman.*) When you say that you investigate the state of the stomach by syphonage, does that include every stage of the condition of the stomach? You do not acquire all knowledge that is useful about the stomach from that, do you?—I do not see why you should not acquire all the knowledge that you would get from a fistula.

19230. You think that you can add nothing by experimentation on animals to the knowledge which you can obtain by use of the syphon?—I think that you would get as good results by the syphon.

19231. (*Sir William Collins.*) Is that limited to the stomach, or would your remarks go to the digestive process in the pyloric end of the stomach?—That is where the difficulty comes in, I know. You mean with regard to absorption?

19232. I mean with regard to absorption and secretion beyond the stomach?—Yes, I think you would have to make your fistula, but there, again, one would be anxious to take some animal which ate a mixed diet other than the dog.

19233. Such as——?—A pig is an excellent animal.

19234. (*Chairman.*) *Apropos* of that, is your reason for preferring a pig that you think it is not so intelligent as a dog, or so friendly?—Partly. I have no affection for a pig, and if you want to trace anything in a pig's liver, the member of my own profession who is here will allow that there is nothing so beautiful to examine under the microscope as a piece of pig's liver. Is not that so?

19235. (*Sir William Collins.*) I am afraid it is the privilege of a Commissioner to ask questions?—I beg your pardon; I am sorry. That is my view of the matter.

19236. (*Chairman.*) I gather from your *précis* of evidence that you consider that a dog has anticipation of pain?—I know that it has.

19237. What exactly do you mean by that; under what circumstances has it anticipation of pain?—I will give you an instance of a dog of mine which had cancer in its ears. We decided that we must curette them. We curetted one ear first of all without an anæsthetic.

19238. Will you explain to us what curetting is?—Scraping the diseased tissue away with a half sharp instrument, to get a clean space; it is much worse than cutting.

19239. Did you do that under an anæsthetic?—I could not, the dog was discharging so very much.

19240. This was not an experiment?—No, it was for the benefit of the dog; the dog was so bad. We did one ear, and then it had had enough of it. It was perfectly intelligent, and never tried to bite. Then, when the veterinary surgeon and I, about two days afterwards, tried to do the other ear, the dog simply shook, and I thought it was anticipating pain.

19241. That is when you came there in order to scrape the ear?—Yes.

19242. Did you scrape it?—No, we could not; we

had to give it an anæsthetic. We got it down, and the dog shook and whined.

19243. And you had the knife ready for the purpose?—Yes.

19244. That I can quite understand. That is what you mean by being sure that a dog anticipates pain?—Yes.

19245. (*Sir William Collins.*) Based upon a reminiscence of suffering?—Yes.

19246. (*Chairman.*) Is experimentation on the kidneys of a dog a thing which you consider useful?—That is a thing which I should, perhaps, not have put in my *précis*, because what I have to say on the other side is purely clinical.

19247. I do not quite know what that means. Do you mean that clinical observation will teach you everything that can be learnt about the kidneys and their action?—I should think that you could learn two-thirds of it, and that you might do as well with the kidneys of other animals as with those of dogs—of the cat, for example.

19248. Do you think that in the sphere of clinical observation, which was the method of observation for, I suppose we may say, thousands of years, there is as much to be added to your clinical knowledge as there can be to your experimental knowledge?—I think that you must have experimental knowledge too, but I would exempt the dog if possible.

19249. You have not quite answered my question. Do you think that there is as large a field for new knowledge to be obtained by clinical observation as there is for new knowledge to be obtained by experiment?—There is a field in both.

19250. Is there as large a field for new clinical knowledge, considering that our clinical knowledge has been of such long standing, as there is for experimental knowledge?—Yes, but our clinical knowledge now starts on a very much higher level since we have had the knowledge obtained from experimentation to help us.

19251. Then one of the results of knowledge obtained by experimentation is that it enlarges your clinical observation?—Yes.

19252. And you know where to look?—Yes.

19253. And what to expect to find?—Yes, one cannot deny the benefits of experiment.

19254. Have you any suggestion to make as regards the present law; would you have it altered in any way?—I would urge, if possible, that all dogs should have their experiments conducted under anæsthesia, and if, for the sake of arriving at a conclusion, you want to prolong the animal's life after the experiment, you should keep it under morphia.

19255. So as to dull the pain?—Yes, and give it freedom from pain.

19256. Is that always practicable?—I should think it was a thing that might be tried, and that you should use other animals where it is not practicable at present, and then, of course, if it is found that mankind is suffering, or the knowledge of the world is suffering, the question must be reconsidered.

19257. I am speaking in entire ignorance, and I ask the question for my own information. After a dog had recovered from an operation would observation while it was under morphia be as useful as observation would be without morphia?—That would depend upon the organ upon which you were operating. If you were operating upon the kidneys it would be equally useful, but if you were going to do one of those operations where morphia would alter your results, you should try first and see whether you cannot use some other animal. One has to speak temperately on this subject because it is such a very important one.

19258. I rather gather from what you say that assuming that animals are not brought out of anæsthesia for future observation except in cases where it is strictly necessary, and if possible not using a dog, you do not object to such observation after operations?—On animals, no; but I would strain every nerve to leave the dog out.

19259. (*Sir William Collins.*) Was the handbook of Burden Sanderson, Klein, and Brunton used in the laboratory at Oxford when you were there?—Yes; but it was not a handbook; it was not bound; it was loose sheets.

19260. Was the matter contained in that book used before students in the laboratory?—At Oxford, I think it was what is called the handbook now; they were loose unbound sheets that Sanderson always issued and Gotch used to take round.

19261. Was that the material upon which the ordinary student or the advanced student worked?—That was what we all of us worked by—the thing that we saw. You had the experiment for the day, and the apparatus required, you know.

19262. If experiments on dogs for scientific purposes were always conducted under anæsthetics would all your objection to using dogs be met?—Yes.

19263. You have no objection to what has sometimes been called exploiting dogs, using them for that purpose provided that it is painless?—No, for this reason, that if you are going to benefit your race I do not think you have any right then to let your sentiment come in the way. I would say always, if possible, try to use some other animal. If you can assure me that you have a right to know more about these matters than any of us who are engaged in other work, and you say to me that you know that the dog is the only animal that will answer the purpose satisfactorily, and if you also say that the dog shall not suffer afterwards, then I say I am sorry that it has got to be so, but mankind must come first, and you must take it.

19264. Would you exempt the monkey as well as the dog?—I have not got the same affection for the monkey. I only had one.

19265. (*Mr. Ram.*) And you were glad to be rid of it?—It bit everybody; there was no affection about that animal.

19266. (*Sir William Collins.*) Your objection then is based upon affection and not upon the proximity to man by way of evolution?—That I do not know. I have some relatives who like monkeys, but one has not the same sentiment about a monkey that one has about one's dog.

19267. It is based then on affection, or sentiment?—Yes, I admitted that.

19268. You spoke of experiments on the kidneys and you suggested that clinical experience was rather against their value, I understand?—I did not mean against their value, but I was thinking of a case where we had cut down—at least I did not, but the veterinary surgeon did—upon the kidney, and we expected to find stone, whereas we found the whole kidney instead of having stone in the hilum was simply riddled. The case was a woman, and from watching how the kidney was working we took the trouble to catheterise through the bladder or ureter separately to see how much the other kidney was helping it. That seemed to me indirectly valuable information It was done for the patient's benefit, of course.

19269. In your *précis* you state: "Experiments on the kidneys objected to. Careful clinical work preferred"?—I do not mean altogether to say preferred; I mean what I said just now, that you can supplement your experimental work.

19270. Has the treatment of kidney disease by providing the capsule been the outcome of experiments upon animals?—Yes, it has.

19271. Is that a valuable mode of treatment?—A most valuable; one cannot get away from those facts.

19272. Do you find the views that you hold prevalent amongst the profession?—I was surprised to find how prevalent they were.

19273. They were or they are?—They are. There are many men, of course, who think the other way, and I feel great diffidence in coming here and opposing them, because so many of the men who think otherwise than I do are my intimate friends, and men for whom I have the highest regard. One of my dearest friends, for instance, is Victor Horsley, so that I feel a certain amount of diffidence over this; but my object in coming was to try and get that one animal exempted wherever it was possible—not to let it suffer.

19274. (*Sir Mackenzie Chalmers.*) Have you seen any experiments yourself lately—any research experiments at all?—I have not, and I explained that to the Canine Defence League.

19275. The last experiments that you have seen were lecture experiments at Oxford?—Yes.

19276. About twenty years ago?—In 1883 or 1884.

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19277. Rather more than twenty years ago?—Yes. My experiments were finished at the time that I had a talk with Sir William Church.

19278. Who were your examiners?—Sir William Church was one of them, Dr. Bristow the other.

19279. We should all like, of course, if we could, to restrict experiments on dogs, therefore I want to ask you one or two questions to see how far you go. You agree that superfluous dogs have to be destroyed, I suppose?—Certainly.

19280. And you are aware, as a matter of fact, that about 30,000 dogs a year have to be destroyed in London in the lethal chamber?—Yes.

19281. Have you any doubt yourself that a dog can be properly chloroformed and anæsthetised?—I have not the slightest doubt that a dog can be chloroformed, and I have had some little difficulty in persuading the gentlemen of the Canine Defence League to that effect. I will tell you why I think so. Any of you gentlemen who probably are used to dogs will know that taking out a dog's dew claws is most intense agony. Every dog that I have in the country—I dare not do it in London—has to part with its dew claws, because it would be a misery to itself and everyone else. I have tried everything, cocaine and ukaine, but a local anæsthetic does not do. When I chloroform a dog it will go absolutely to sleep. I took both dew claws off an Irish terrier; we did it in the morning, it did not have its food in the morning, it went dead asleep under the chloroform, and I know that by one o'clock it was growling over a bone, perfectly happy, and it never evinced any pain.

19282. Having ascertained your opinion on this point, I was going to ask you a further point. Seeing that a great many dogs have to be destroyed, if the animal is put under anæsthetics till death do you feel any objection to a dog being experimented upon during the process of dying?—No, I think you are justified.

19283. Therefore, experiments under a licence alone without a certificate you have no objection to?—I do not quite follow you.

19284. When a gentleman holds a licence under the Act, but does not hold any certificate, the condition is that every animal experimented upon must be put under anæsthetics, and he may perform what experiments he likes provided that the animal never recovers from the anæsthetic, but is killed during the time of anæsthesia. You would not object to that?—No, I do not think it would be right to say that you shall absolutely forbid it provided that the animal is not going to suffer.

19285. (*Mr. Ram.*) Provided that it dies under a lethal dose?—Yes. You should treat it kindly first. Do not knock it down if it is going to be a benefit to mankind. If it has to die, there is no reason why it should not be made use of in that way, provided that it will not know anything about it.

19286. (*Sir Mackenzie Chalmers.*) Have you any reason to believe that any of these animals are ill treated in any physiological laboratory; have you any knowledge of it?—No, of recent years I have not had any means of knowing.

19287. Can you quote to us any experiment that you know anything about in recent years which has been in your opinion objectionably conducted?—No, I am not in a position to state that during the last few years.

19288. Have you followed the course of experiment lately. Do you carefully read up the physiological experiments?—To a certain extent; but it is very hard; I read it by fits and starts. A man in full practice does not find it very easy.

19289. Coming to the first certificate, certificate A authorises the experimenter to experiment on an animal without anæsthetics, but does not authorise anything more than a skin-puncture, for instance, the prick of a hypodermic needle; but, of course, that involves the inoculation of the disease. What do you say about that?—I would exempt the dog from it until it can be shown that the dog and the dog alone will be of benefit to humanity.

19290. But if that can be shown, then you think that the dog must give way to humanity?—If that can be shown, then I do not see that one has any right to neglect to take means to save human life.

19291. Now I will come to the next point. Take the case of distemper. You know what a dog suffers from distemper, and what valuable dogs are lost through it?—Yes.

19292. Do you think it is justifiable, for the sake of discovering a remedy for distemper, to use experiments on dogs?—You mean to let one dog suffer in order that many dogs may escape?

19293. Yes?—Of two evils I should choose the less.

19294. Now, may I ask you which is the less. Do you think it is justifiable to experiment with dogs in the hope of curing or mitigating distemper some day?—It is in the dogs' interest, therefore you must do it.

19295. It is not in the interest of the particular dog that is experimented on?—No, but it is in the interest of dog-land.

19296. It is in the interest of the canine race?—Yes. It is no more than we do to human beings.

19297. You think that the particular dog must suffer for the rest?—Yes.

19298. Does not that argument carry you this far, that if we can do anything for the human race by sacrificing dogs, we are bound to do it?—I come back to the other point, if you can satisfy me that the dog and the dog alone can answer the purpose; but I should require to be satisfied on that point first.

19299. Have you read Professor Starling's evidence on that point?—No, I have not. I meant to have done so.

19300. I am rather sorry that you have not, because he gave us certain reasons why he said that it would be almost impossible for research to go on if dogs were excluded: that there were certain cases where the dog was the only appropriate animal. I should have liked your opinion on that?—I may say that I only had notice that I was to give evidence three days ago, and I have not had time to read it.

19301. Therefore you cannot give us any opinion upon it?—I cannot.

19302. Now I want to come to Certificate B. Under Certificate B, if any operation is to be performed on an animal, the animal must be kept under anæsthesia throughout the whole operation, including, of course, all the subsequent stages, but the animal may be allowed to recover. There, as I understand you, is where you would draw the line with dogs?—Yes.

19303. Is that because you think that there must always be subsequent pain?—I think there is always some subsequent pain.

19304. We have been told that in many cases there is nothing more than small discomfort?—I know, but I have my own personal experience the other way. I have had various things done to me when I have been damaged at boxing and fencing, and have had little bits of me removed, and I have always had pain afterwards.

19305. That is personal experience?—Yes, I had a fatty tumour removed from my neck and the surgeon told me afterwards that I had no pain. I told the surgeon that it was my neck not his, but he said I had had no pain at all.

19306. Then Certificate B is really the part of the Act to which you object?—It is.

19307. On the ground that during the period while it is necessary to watch the animal you think that in most cases there must be not only discomfort but a certain amount of pain?—There is pain.

19308. And that the experiment would be frustrated if the animal was killed as soon as bad symptoms of pain appeared?—Yes.

19309. But you admit that you have not watched the particular experiments?—I have not, and therefore I am not in a position to dogmatise, and that is why I felt that I was almost taking up the time of the Commission under false pretences.

19310. (*Mr. Ram.*) I have very little to trouble you with. Everything turns, I take it, in your opinion, with regard to the use of dogs, upon the presence or absence of suffering of the dogs either mental or physical?—Yes.

19311. If you were sure that no pain was caused either mental or physical, in the interest of science you would not preclude the use of dogs?—No.

19312. With regard to physical pain Sir Mackenzie Chalmers has asked you some questions. With re-

gard to mental pain you gave us the instance of that dog whose ear you curetted, and who, next time you endeavoured to do it, showed great signs of fear. Did that dog show signs of fear on the first occasion?—It became nervous when I came to touch the tender part.

19313. But before you touched the tender part, when you produced the knife did it show any signs of fear?—No.

19314. Its terror on the second occasion then would be due to memory?—Yes, recollection.

19315. I want to put a question on your *précis* with regard to a statement that I am not clear about. Do you think that a dog can be said to have a greater anticipation of pain than any other animal if you eliminate all idea of memory?—No more than a child or a man. Human beings have no sensation of pain the first time. If you try to take a tooth out for the first time from a child it does not mind.

19316. Apart from memory, then, would you say that there is any anticipation of pain?—Perhaps I used the wrong word when I said anticipation of pain; my *précis* was drawn up in a hurry.

19317. There is another sentence in your *précis* that I want to put to you. You say with regard to the treatment of gastric troubles that you are entirely with the vivisector or anybody else—that he may vivisect himself or any of his friends or any dog if that is the only way by which he can arrive at the state of the stomach at a given moment, by a gastric fistula. That is your view?—Yes, because I believe with regard to the stomach that you can do anything you like with a tube.

19318. In that sentence, therefore, do you mean to go the length of allowing a fistula to be made in a dog to be kept perfectly open and the dog to recover from the anæsthesia?—No, I was putting a *reductio ad absurdum* knowing that you would not say that that was the only method or taking it for granted that you would not.

19319. Then I am not quite sure that I follow your view about this. Does it mean that if it be essential that a dog should be used and no other animal, then under anæsthesia it should have a fistulous opening, and be allowed to recover, the wound being treated aseptically?—No, I meant it the other way. I am so sure that you can get the same results by intubation of the stomach that if anybody could prove that it was absolutely essential to make a gastric fistula I should say, do it, but I am absolutely certain that you can do it by syphonage.

19320. I am glad I asked you the question, because that makes it perfectly clear. Your attitude of mind, as I understand, would be that if it could be proved that it was absolutely necessary——?—I say that the world would come to an end; it is absurd.

19321. You do not think it is absolutely necessary?—I am sure that it is not.

19322. In the mode that you adopted of examining the stomach you evacuated the stomach?—I took a little out of it. I often test my own stomach. I have tested the digestibility of different foods on myself. I wait a certain time, pass the tube down, and draw off what I want, and test it for acidity and parapetones and peptones.

19323. But you do not get pure gastric juice?—I could get pure gastric juice if I chose to wash the stomach down first.

19324. But do you by that method get pure gastric juice?—I never tried for pure gastric juice, but I should think I have.

19325. What you have tested would have been gastric juice, plus other contents of the stomach?—Yes. I do it to test the process of digestion.

19326. I think the summing up of your evidence is this, is it not, that you would like to narrow as much as possible any operation on dogs?—Yes.

19327. That you have no objection to an operation on a dog if it be a desirable animal, provided that it dies under the anæsthetic?—Yes.

19328. And that if it could be proved to the satisfaction of competent persons that it was necessary that dogs should be allowed to recover, if such proof were forthcoming, you would not object even to that?—Allowed to recover and suffer?

19329. Yes?—I could not go so far as that.

19330. I put it to you only in the event of such persons as the President of the Royal Society, the President of the Royal Society of Edinburgh, the Presidents of the Royal Colleges of Surgeons and Physicians, and such persons being satisfied that for the advantage of mankind it is necessary that a dog should be allowed to recover, being effectually treated?—I should want to know all the details of it.

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19331. (*Sir Mackznzie Chalmers.*) That would be *carte blanche*?—To give leave to do that would be a large order.

19332. (*Mr. Ram.*) There is no question of *carte blanche*. The person who certifies has to know what the operation is that is to be done, and he has to satisfy himself that the experiment will be necessarily frustrated unless it is performed on an animal similar in constitution or habits to a cat or dog, and no other animal is available for such experiment?—Then take a cat.

19333. You would take a cat in preference to a dog?—Yes.

19334. Supposing that there were no cat available?—You can always find a cat.

19335. You would sacrifice the cat to the dog?—Yes.

19336. Does your view depend upon the amount of suffering that is subsequently caused? Supposing that the amount of suffering upon recovery is very slight indeed, should you object even to that?—It would be a dangerous precedent.

19337. You have said that you would substitute every other animal for a dog, and you suggested a pig. You know that experiments can only be made in licensed places?—Yes.

19338. And that those licensed places are almost always laboratories attached to hospitals, medical schools, and so on?—Yes.

19339. A dog or cat can be kept without discomfort in such places, but a pig could hardly be kept under those circumstances, if at all. You realise, therefore, the difficulty of using pigs?—There were some experiments I believe on pigs in the Oxford Laboratory.

19340. (*Dr. Gaskell.*) I understand you to say that you have not seen any experiments under Certificate B which allows the animal to be kept alive under the experiment and to recover?—No, I have not.

19341. Therefore you are not competent in the slightest degree to consider really what the condition of the animal is afterwards?—No, I am not.

19342. And you have made no inquiries into the matter?—It has always struck me that inquiries are biassed on one side or the other. Unless you can see the operations you cannot judge.

19343. That is what I mean. You have never seen an operation yourself—it is very remarkable to see?—I stated at the beginning that all my experiments are 20 years old.

19344. Do not you think from your knowledge of physiological laboratories that the vivisectors themselves would agree with practically all you have told Mr. Ram just now?—I believe they would.

19345. Do not you think that they do as a matter of fact choose other animals rather than dogs if they can?—Yes, I think so, and I am very glad of it.

19346. And do you not consider that that is partly because they have the same feeling with respect to dogs as you have?—I am sure they have. I stated at the beginning that that is my difficulty in giving evidence, because so many of these men are my personal friends.

19347. In addition to that is it not the case that dogs are more expensive than other animals, so that the very lowest of motives, the motive of the pocket alone, would cause them to prefer other animals?—You can steal a dog anywhere.

19348. You do not mean to imply, do you, that the dogs used in physiological laboratories are stolen, and not paid for?—I have stolen dogs in my early days.

19349. Do you think that they are not paid for?—I thought honestly that the dogs are taken from the Dogs' Home for nothing. I was told so, and I said that I would accept the statement with salt.

(*Sir Mackenzie Chalmers.*) It is forbidden by the Statute.

19350. (*Dr. Gaskell.*) I do not know how much you

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are acquainted with other laboratories than that of Oxford?—That is the only one I know.

19351. Was it not a fact that all the dogs were paid for there?—I should think so.

19352. And that they were more expensive than other animals, so far as that goes. I am not saying that that is the sole motive?—Yes.

19353. (*Sir John McFadyean.*) I suppose you have sufficient acquaintance with dogs to know that they are the subjects of certain diseases quite peculiar to the canine species?—Yes, they are.

19354. And I daresay you also know that there is a great deal of ignorance with regard to some of those diseases?—Yes, there is.

19355. Therefore you will agree that it is advisable that they should be further investigated?—Certainly.

19356. You would also admit that if they have to be investigated by experimental methods only dogs would suffice since the disease is not transmissible to other animals?—Yes, but if you are going to carry that further you can only experiment on man if you want to improve the state of man.

19357. I did not mean that. In the case of a disease which is only transmissible to man you cannot experiment at all?—No.

19358. You have assented to the statement that certain diseases are peculiar to the dog?—Yes.

19359. I ask you further to assent to the view that if those diseases have to be experimentally investigated you must use the dog?—Yes.

19360. Now take such a disease as distemper. You are aware that it causes a tremendous amount of pain and suffering to dogs?—Yes.

19361. Do you think it is desirable that it should be experimentally investigated if there appears to be a reasonable prospect that you might thereby get a knowledge that would save future suffering to dogs?—My practical experience is that I have had many dogs with distemper, and if you keep them warm, and give them an occasional aperient you will not lose a dog from distemper; it will get well comfortably.

19362. Will you assume, please, that that is an exceptional experience, and that you can have an immense amount of evidence to show that it is a very deadly disease in special outbreaks, and that the dogs are undoubtedly very ill and uncomfortable?—I may have been lucky in my experience.

19363. You think it is desirable that it should be experimentally investigated if there is a reasonable prospect that you could get a remedy?—That is what I should prefer.

Mr. R. J. Cowen, L.R.C.P.I., L.R.C.S.I., called in; and Examined.

Mr R. J. Cowen, L.R.C.P.I., L.R.C.S.I.

19364. (*Chairman.*) You live at 15, Half Moon Street?—Yes.

19365. Do you practice there?—Yes.

19366. As a surgeon?—As a surgeon.

19367. I believe you are a Licentiate of the Royal College of Physicians, Ireland, and of the Royal College of Surgeons, Ireland?—Yes.

19368. Are you a Fellow of the Gynecological Society?—Yes, and several other societies.

19369. You attend to give evidence at the request of the Canine Defence League?—Yes.

19370. Are you a member of that Society?—No.

19371. I believe you desire to give evidence on the anatomical differences between dogs and men, with a view to suggesting that there is no utility in examining dogs in order to study their diseases, and how the remedies for those diseases act on the body of man?—That is so.

19372. Would you put it so high as to say that dogs are so different in their structure that no such information can be gained by examining their bodies with regard to man?—No. I would put it that information could be gained more readily, and the conclusions would be less liable to be erroneous, if the experiments were made upon animals more similar to human beings, mixed feeders, for instance, such as pigs, and especially animals that perspire.

19373. You look upon the pig as more allied to man?—I look upon the pig as having a closer resemblance to man, and being on that account more suitable for experiment.

19374. You say that the pig is a mixed feeder?—Yes.

19375. Is not the dog a mixed feeder?—The dog was not intended to be a mixed feeder. The teeth of the dog show plainly that he was intended to be a carnivorous animal entirely.

19376. What was man intended to be?—I always hold man to be a mixed feeder.

19377. I suppose that would depend upon where he lived, and how he could get his food?—Certainly; but his teeth vary much from the teeth of a dog and think we have considerable evidence in favour of the fact of his being intended always to be, and his being, a mixed feeder.

19378. A feeder on one substance in one place, and another in another, do you mean?—Or a feeder on two or three different substances in the same place.

19379. But supposing that a man was living as a naked savage on a barren plain, where there is but little vegetable food, would he not be then an animal feeder entirely?—The difference, I think, is that man can support life upon the fruits of the earth; a dog cannot. If a dog was only given uncooked vegetable food it would die; it could not live on it.

19380. You think that is the crucial difference?—It is an important difference.

19381. And one that you think makes experiments on dogs valueless?—Undoubtedly it does to a large extent.

19382. You go as far as that?—I go so far as to say that conclusions drawn from experiments made on the digestive organs of dogs must necessarily be erroneous.

19383. I do not know whether you have read the evidence that has been given before us?—I have read some of it.

19384. Then you are aware, probably, that a large number of very eminent physiologists, some of the most eminent in the kingdom, have given evidence that they consider experiments on dogs to be really valuable in many cases of research?—Yes, I am aware of that. I have gone very carefully over the experiments performed abroad and here where dogs are used, and I have not yet succeeded in finding one single experiment which could not have been performed quite as well, if not better—I hold better—upon some other animal.

19385. In books?—Both in books and personally in laboratories abroad and from reading the researches of others.

19386. Have you held a licence yourself?—No, I have not.

19387. Where have you studied—in what laboratories?—In Germany, in Bonn, in Leipsic, and in Paris with Pasteur. I cannot say that I have studied there, but I have spent time there, and gone over with them and gone into the work.

19388. Did you belong to the class of Dr. Pasteur?—No, I did not belong to the class.

19389. Or at Leipsic?—No, I was only a visitor.

19390. As such did you frequent the laboratories or did you visit them once or twice?—I suppose I visited each of them a dozen times or more.

19391. Is that the extent of your experience in operations on live animals?—That is the extent.

19392. Would you tell us what you consider to be the special points of difference between men and dogs? In the first place the teeth. We do not find in a dog the same shaped molars that you nearly always find in mixed feeders, or in vegetable feeders. The position of the canines also, the over-lapping canines, is a very important distinction.

19393. That is a distinction in structure; but it is not a distinction which bears much, or at all, upon the question of experiments upon dogs?—That is so; the canine teeth do not bear upon that because they are intended almost entirely as weapons of defence, but the shape of the molars is very distinctive.

19394. What I rather meant by internal structures are those examined in experiments. You do not examine a dog's teeth by vivisection?—I was going down from that. The next difference is the digestive

tract in the dog, the dog bolts its food and as a consequence the stomach has to do the greater part of the digestion—the stomach and the intestines. As a result of this, relatively speaking, the digestive tract is very much shorter and the digestive juices are a great deal more concentrated and more powerful; they will digest more powerfully than the similar juices of the human being.

19395. Have you observed in the evidence of the witnesses before the Commission that that distinction has been pointed out to us by some of the gentlemen who still think that for many purposes experiments on dogs are extremely important?—No, I did not notice that. I have not had time to go over the whole of the evidence.

19396. What is the next distinction that you find?—The position of the heart and the character of its beat is a very important distinction. The intermittent pulsation of a dog's heart is very different from the steady regular beat of the human heart.

19397. Do you consider that an experiment to ascertain the blood pressure on a dog, and the action of different drugs upon it is not a proper subject of research?—I cannot conceive how it is possible to draw conclusions as to human blood pressure from experiments on an intermittently pulsating dog's heart.

19398. Have you read the evidence which has been given on that point as regards experiments on dogs?—Yes, I have, and some of it I cannot agree with at all; with much of it I cannot agree.

19399. You differ from those gentlemen who have said that for purposes of blood pressure, experiments on dogs are valuable?—I differ very much.

19400. Exceptionally valuable?—I object to the term "exceptionally."

19401. You do not object to the term "valuable"?—It is just possible to conceive a condition under which an intermittent pulsating heart might bear close resemblance to a deceased human heart and consequently might have some value in that way, but otherwise it would be impossible.

19402. Is there any other point to which you wish to call attention as regards the difference between a dog and a man?—The brain is a curious thing. A careful analysis of the chemical composition of the brains of different animals and the relative amounts of phosphorus which they contain shows that the larger the proportion of phosphorus in a general way the greater are the signs of intelligence shown by the animal that owns the brain. In a dog's brain there is a very considerably larger proportion of phosphorus than in that of any other animal—next to it comes the horse—but the dog has a very much larger relative proportion of phosphorus in its brain. I think that is important.

19403. I see you say that one of the reasons why dogs are so often made the subject of vivisection is that they can be cheaply and readily procured?—That is so; it has always been so.

19404. I suppose that rabbits and guinea pigs can be more cheaply procured?—Not quite so easily, and perhaps not so cheaply.

19405. Have you ever bought guinea pigs or dogs for the purposes of experiment?—No, I have not, but I have gone into their prices more or less. I have ascertained what they cost, what they could be bought for, and how they could be obtained.

19406. But you yourself have not bought any?—No.

19407. We have been told that dogs are more expensive to experiment on than guinea-pigs or rabbits by those who do use them. I do not know whether you are prepared to dispute that evidence?—I can only dispute it on hearsay.

19408. What animals do you suggest should be used instead of dogs?—Almost any other animal that is a mixed feeder would be more suitable for experiments, if experiments are necessary.

19409. A cat, for example?—A cat is undoubtedly more suitable, although the cat is a domestic animal, and can hardly be called a mixed feeder.

19410. You have given us the reasons why you think that a dog is less suitable. Do you say that it is less suitable than a rabbit or a guinea-pig?—Undoubtedly. I think that is proved by the action of different drugs. If we compare the action of different drugs upon a dog and upon a human being, it is very different. I have noted down two or three here. For instance, camphor, while undoubtedly causing sleep in most animals, has no effect whatever upon dogs. Take, again, citric acid, the acid of lemons, which is a deadly poison to most animals.

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19411. Do you think that a skilled physiologist, if he wanted to experiment with camphor or citric acid, would choose a dog for the purpose?—I am taking only a few out of a very large number of drugs.

19412. You are stating that there are a good many substances which do not affect a dog?—Yes.

19413. Do not you think that a skilled physiologist would be aware of those substances and would not use a dog for the purpose?—Yes, but I am taking the case of a new drug. Supposing that he is experimenting upon the action of quite a new drug, about which he knows nothing, he makes the experiment on a dog, and finds a definite result; he finds, for instance, in one case *cocculus indicus*, or say another drug such as woody night-shade, which is deadly poison to a dog, he finds that it kills the dog, and comes to the conclusion that it is a deadly poison necessarily to man, whereas it is quite harmless.

19414. Do you think that mistakes of that kind have been made? Have you ever seen any accidents occurring through such mistakes?—I have seen a very serious mistake made and carried out in many medical journals that calomel has no effect in increasing the bile flow in a dog, and, therefore, it must be so in a human being, whereas the medical profession for almost centuries have known the value of calomel. It was found that a mistake had been made.

19415. How was that discovery made—by experiment?—By experiment in the first instance that it would not produce bile, and it was then published that calomel was really of no good to cause an extra flow of bile. Gaurana is another drug. Gaurana intoxicates a dog, but is absolutely harmless to a human being.

19416. Do you consider that dogs can be fully anæsthetised and yet kept alive for the purpose of examination?—On account of the intermittent pulsation of a dog's heart, of which I have spoken, I think it is practically impossible to keep a dog fully anæsthetised for any length of time.

19417. I think you said that you yourself had not operated upon a dog?—No, I have not.

19418. Or anæsthetised dogs during an operation?—No, but I have watched it very carefully. I am wrong in saying that I have not anæsthetised a dog. I did it once in order to remove a little tumour from a pet dog of my own. I used every possible care in the case, and I am perfectly aware that the dog felt and could not help feeling it. It was a pet dog, and I did my best to avoid giving it pain. There will be an anæsthetic I think in the immediate future that can be used with animals to produce a more perfect anæsthesia.

19419. Do you consider that any struggle in an animal or man while under an anæsthetic is an indication that they must be suffering pain?—I do. In the case of a human being, when your patient is being put on the table he passes through the usual stages of delirium, excitement, and so on, and then sleep. So long as he is sleeping really perfectly anæsthetised and there is no movement he is absolutely painless, but then there comes a period when he is just recovering, a period which I have noticed you have had described in the evidence before you, when if you put an extra stitch in, the patient will give a cry and pull away from you. When the patient awakes in most cases he will say that he felt no pain whatever, but having forgotten the pain is no indication that it did not exist.

19420. Then do you draw a distinction between a person who comes to and is not conscious of having been hurt at all and a person being hurt who has actually felt pain; do you consider that there is any difference between the two?—I think that wherever there is any purposive movement, where there are any of those reflexes that one sees when the skin is touched with a knife, that animal, human or other, feels a pain, but in many cases of anæsthesia apparently they have forgotten the pain when they awake, but the pain was there.

19421. Do you attach importance to that?—Yes

19422. Do you consider that a person who has an operation and struggles, and whom you think is under pain, but when he comes to and is asked about it when the operation is over says that he has felt noth-

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ing, has suffered shock of any kind more than he otherwise would?—Undoubtedly. We find in the case of surgical operations which were performed before the introduction of chloroform, and even since then——

19423. I am speaking of since the introduction of chloroform. Supposing that a person is operated upon under apparently complete anæsthesia, and when he comes to, feels and remembers nothing, is he in any worse condition if he has shown some signs of struggling during the operation from a man who goes through exactly the same thing and has not struggled?—I think he is in a worse condition from two points of view, because pain increases the risk of surgical shock, and he has also felt the pain whether he has forgotten it or not.

19424. Can you point to any case in which a patient has suffered severely from shock in consequence of having felt pain during what was apparently complete anæsthesia?—If it was apparently complete anæsthesia, they would not struggle or give any evidences of pain whatever.

19425. The question is whether it is an evidence of pain?—I think the purposeless movements that one sees very often, reflex movements, and excitement, have nothing to do with pain, but I have many times seen persons under apparently complete anæsthesia, when you can touch the eyes and prick the skin and they will not be affected, but there is a certain very painful part of the body, and the moment that you touch this one part with a knife it is a sure test, because if the anæsthesia is incomplete the patient tries to struggle away; and with a little deeper anæsthesia these movements will be abolished, and the patient will lie perfectly still.

19426. You would exclude dogs from the operation of the Act?—I would exclude them from all experiments.

19427. And you think that science would not suffer at all in that case?—No. I cannot see why in the later Act stray dogs should be given privileges which kept dogs have not got. Stray dogs are expressly forbidden to be used for vivisection purposes.

FORTY-THIRD DAY.

Wednesday, 11th December 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. G. WILSON, M.D., LL.D.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. L. E. SHORE, M.D., called in; and Examined.

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19428. (*Chairman.*) You are, I believe, a Fellow of St. John's College, Cambridge, and Doctor of Medicine?—Yes.

19429. And University Lecturer on Physiology?—Yes.

19430. Do you remember some experiments that were made by Professor Gaskell and yourself (you were assisting, I believe, in them) with a view to ascertaining what the method of action of chloroform on the heart was?—Yes, I remember them perfectly.

19431. Were there a series of experiments by you and Dr. Gaskell?—Yes, there was a series by Dr. Gaskell and myself which were carried out in the years 1890 and 1891.

19432. Do you remember Colonel Lawrie coming to Cambridge to see some experiments of the same kind?—Yes.

19433. Which, I believe, were experiments made for the purpose of showing them to him?—Yes.

19434. In what year was that?—That was in July, 1894.

19435. There were two experiments made in the presence of Colonel Lawrie, I understand?—Yes.

19436. Were those of the same nature as the experiments that you had conducted previously in 1891?—Yes, of the same nature with a slight improvement in the method.

19437. Will you just tell us what the experiment was; was it what you call an experiment by the method of cross circulation?—Yes.

19438. Will you just explain very shortly what it was?—The object of the experiment was to decide whether chloroform causes a fall of blood pressure by an action on the heart or an action on the vaso-motor centre in the medulla, which could be absolutely decided by taking an animal and so ligaturing the arteries of the brain that its own blood could not reach its own brain—the brain of the animal being supplied with blood from another animal. When that was done one could send chloroform either to the brain or to the heart of the first animal.

19439. Then two dogs were operated upon side by side at the same time?—Yes.

19440. And then the vessels of one which would lead to the brain, and bring blood to the brain, were fastened up, tied up with ligatures?—They were tied with ligatures and in some cases divided between the ligatures. The animal with the vessels divided to separate its blood from its brain we call the fed animal; the other animal we call the feeder; it delivers blood to the brain of the fed but to the brain of the fed only, and the blood is returned from the brain of the fed back to the feeder.

19441. What is the process by which you ascertain what the effect of chloroform on the heart directly is without the possibility of its coming through the brain? A record of the blood pressure of the fed animal is taken and a fall of that pressure can only be brought about by a change in the heart when chloroform is administered to it, because the chloroform administered to it cannot reach its brain, because the blood vessels leading to the brain of the fed animal are completely ligatured.

19442. The object being by giving chloroform to the fed dog and at the same time cutting off the supply of blood to the brain of the fed dog, to test what the direct action is of chloroform upon the heart without the possibility of the brain affecting it?—Yes.

19443. You carried out that operation, or practically the same operation, I understand, in 1891 and 1892?—1891.

19444. Were those operations successful?—Yes.

19445. Were they carried out successfully in the sense of ascertaining what you wanted to ascertain?—Yes, they satisfied us that we had definitely proved the point that chloroform does act directly upon the heart and that a fall of the blood pressure is due in the main to that.

19446. Then in 1894 you carried out practically the same experiment twice in the presence of Colonel Lawrie you told us?—Yes.

19447. Were those two operations both successfully carried out or was there any failure in either of them?—The first experiment was quite successful.

19448. Proving, to your satisfaction at least, what?—Proving the point that we wished to demonstrate to Colonel Lawrie.

19449. That there was a direct action on the heart?—Yes, but he criticised our experiment and thought that some chloroform was reaching the brain of the fed; so we offered after the death of the dog to inject some coloured solution, a blue solution was used, to see whether our ligatures were all perfectly tight and that no chloroform was reaching the brain. We found that a small amount of our blue colour did reach the brain.

19450. You forced it, I suppose?—It was forced by a

syringe. There is no proof from that that any chloroform got up by the same path as the blue colour did because the conditions of the pressure in the brain were totally different during life and very probably the blood did not leak up past those ligatures, but even if it did it did not vitiate the experiment in the slightest degree.

19451. And the first experiment was not vitiated in any degree?—It was not to our mind. But we offered to do the experiment again in four days' time before Colonel Lawrie once more, taking particular care, and in that case we divided the blood vessels between the ligatures and took the greatest care that nothing whatever could leak up.

19452. Then Professor Gaskell, I understand, communicated what you had ascertained about the possible vitiation to a certain extent of the first experiment to Colonel Lawrie?—Yes, we offered to do that straight away and the result of it was that we offered to repeat it again?—But afterwards there was a letter written. Then when the first experiment had taken place, Professor Gaskell wrote and explained what had happened?—Yes.

19453-4. That is a letter that has been communicated to us by Colonel Lawrie?—Yes.

19455. And then this second experiment was held four days afterwards, and it went all right?—It was a perfectly satisfactory one; there was no leakage whatever in that case of any sort.

19456. That is the full account of what is referred to the Question 16972 and the following questions in the evidence?—Yes.

19457. Then there is another matter—I think it is probably an accidental error of Colonel Lawrie's. At Question 16869 he is asked this question: "You also say that you saw another physiologist perform prolonged vivisection experiments on dogs under a ten-drop dose of morphin solution, this dose being equal to one-twelfth of a grain of solid morphin. Had they no other anæsthetic of any sort or kind?" and he answers, "I believe not, until the observations began on the action of chloroform." Is it correct that this was one-twelfth of a grain?—No. I suppose Colonel Lawrie is referring to these two experiments that we did?

19458. It would look so?—In each of those two experiments we had two dogs. Of the two dogs, one dog received ten cubic centimetres of a 2 per cent. solution of hydrochloride of morphia, and the other dog six cubic centimetres of the same solution.

19459. Will you just explain what the difference is between that and the statement of Colonel Lawrie?—A 2 per cent. solution of morphia contains 30 grains in the 100. As the dog had received 10 cubic centimetres, it therefore had 3 grains of morphia, not one-twelfth.

19460. The dose was really 36 times stronger than what appears from Colonel Lawrie's statement in his *précis*?—Yes, and the other one which, as I have said received only six cubic centimetres of the solution, had received 1·8 grains.

19461. That may be accounted for, I suppose, by his having thought that it was a 10-drop dose of a different solution from that actually used?—Perhaps he made some mistake of that kind.

19462. However, it is inaccurate to the extent of 36 times?—Yes.

19463. And that dose that was given was a completely full and satisfactory dose?—Yes. I see in the statement that Colonel Lawrie implies that no chloroform was given until the test was purposely made with the chloroform. That is not the case. In addition to the morphia, chloroform was given to the animals before the skin was cut, and during the cutting operation the A C E mixture, just as it was required.

19464. Were the animals kept in a state of anæsthesia before the cutting began and up to the time of death?—Absolutely.

19465. (*Sir William Church.*) Others were present at these experiments besides Colonel Lawrie, yourself, and Dr. Gaskell?—Yes, at the first experiment there were two of Colonel Lawrie's students from India. I suppose they had been helping him in his own experiments in India, and they were present. And at the second experiment Sir Thomas Clifford Allbutt was present, and Dr. Anderson, as well as ourselves.

19466. And during the experiments the various steps in the operations were recorded as they took place?—They were written, nearly all of them, automatically, by the tracing of the animal and by clocks and other automatic arrangements.

19467. And the record, such as it was, was gone over by all those present at the end of the experiments and signed by them, was it not?—Colonel Lawrie did not actually sign the experiment. He was present at the time, and confessed that the experiment was a success, and that air was properly entering the animal, and other details that we asked him.

19468. Did the others who were present sign?—No. They did not actually write their names on the record.

19469. (*Dr. Gaskell.*) Surely they did?—No, we wrote down the statement that they were present on the record, but they did not actually sign it.

19470. My impression is that they did?—No.

19471. (*Sir William Church.*) Do you know what became of that record?—I have it in my possession.

19472. Was a copy of it sent to Colonel Lawrie, such as it was?—The tracing was analysed and a report of it was sent out to Colonel Lawrie to India, as was agreed, for him to sign there, and then for it to be published.

19473. (*Dr. Gaskell.*) That report that was sent out to him was signed by the other people present, if I remember rightly?—I do not remember.

19474. You signed it, and I signed it, and Sir Thomas Clifford Allbutt, and Dr. Anderson all signed that report if I remember rightly?—I do not remember their signing it. I signed it, and I think you got their signatures afterwards.

19475. My impression is that they all signed when it was sent out to Colonel Lawrie?—Yes, I know that we agreed to do that.

19476. Have you ever had that report back?—Colonel Lawrie has never sent that report back to us for publication.

19477. (*Sir William Church.*) Did he send any reply at all to the communication?—None whatever.

19478. (*Dr. Gaskell.*) There is another matter, but I think that the Commission will be interested in it. You have had chloroform as an anæsthetic yourself?—Yes, on two occasions.

19479. Could you tell the Commission whether on those two occasions you endeavoured to test the length of time after the anæsthetic had been administered before unconsciousness supervened?—On the two occasions that I had to undergo an operation, and was given an anæsthetic, I decided to see whether I could tell the length of time that intervened from the commencement of giving the drug to the period of unconsciousness, and I decided to do it by carefully counting my regular respirations. On the first occasion I was able to count ten respirations, and then I became absolutely unconscious. On the second occasion I could count twelve. That makes considerably less than a minute before unconsciousness supervened. But one ought to remember that in that case I was receiving as an anæsthetic nitrous oxide and ether, which are known to be fairly rapid. I have no experience as to whether chloroform alone is equally rapid, but we know that chloroform is a stronger anæsthetic, but its full effect may not come on so soon. The second point which those personal experiences led to is also interesting. In the middle of the operation I heard the surgeon make a certain definite remark about the operation, but I could feel no pain whatever. When the operation was completely over, and I recovered consciousness, some four hours later, I inquired of the surgeon, and he told me that the remark had been made when the operation had been in progress about half an hour. That experience, I think, points out that the sensation of pain is more easily abolished than such a sensation as hearing, and it is interesting to note that in local or peripheral anæsthesia, which is now very much used, we have the sensation of pain much more readily abolished than other sensations. Many years ago I made some observations with cocaine on the tongue, when I found that the different sensations of the tongue disappear in definite and precise order according to the dose of cocaine or the duration of the cocaine action; and the first of all sensations to disappear is the sensation of pain, then of sweet, and then bitter, then salt, and then acid, and last of all the sense of touch. It is quite striking to notice that

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when the tongue is touched with the needle you feel the touch, but although the needle is thrust into the tongue you feel no pain. So we see that also in local or peripheral anæsthesia painful sensation is the first, I think, to be abolished.

19480. May I ask was the operation in which you heard distinctly the surgeon make a remark, and correctly heard it, of the nature of a fairly severe operation. I mean was the anæsthesia that you had supposed to be full surgical anæsthesia?—The operation was certainly severe. I was put under the drug at 9 o'clock in the morning, and I did not recover consciousness till 1 o'clock in the middle of the day.

19481. Would a surgeon, do you think, with your experience, say that that anæsthesia was rightly described as light anæsthesia, that they were attempting to put you under, at all events? We have had a great deal of discussion here at this table about the meaning of light anæsthesia and deep anæsthesia, and it has been said that light anæsthesia is never given in the case of operations. Therefore I presume you were under comparatively deep anæsthesia; you were supposed to be fully anæsthetised?—I was supposed to be fully anæsthetised, but at the time when I heard the remark, no doubt the anæsthesia had been allowed to go light.

19482. But yet there was not the slightest sensation of pain?—Absolutely none.

19483. (*Dr. Wilson.*) You say that these animals that were experimented on were absolutely unconscious of pain during the whole time of the experiment?—Absolutely.

Mr. A. Guillum Scott and Sir Frederick Banbury, Bart., M.P., called in; and Examined.

Mr. A. G. Scott and Sir F. Banbury, Bart., M.P.

19484. (*Chairman.*) (*To Mr. Scott.*) You are a Member, and, I believe, Chairman of the Royal Society for the Prevention of Cruelty to Animals?—I am.

19485. They have drawn up a *précis* of the evidence to be given before the Royal Commission on Vivisection, which they have printed, and sent to us?—That is so.

19486. I do not quite understand, seeing that there are two witnesses before me, whose *précis* that is. Is it a *précis* of the evidence which you propose to give, or which Sir Frederick Banbury proposes to give, or both?—It is evidence of the Society. It was decided upon at a meeting of the council that the representatives should give this evidence on behalf of the council and the Society.

19487. Then, perhaps, instead of calling it a *précis* of the evidence to be given, we had better call it a statement of the views which the Society have resolved to be their views?—Yes, that is so.

19488. And, therefore, to begin with, and probably to shorten matters, we will put it upon our notes as such?—If your Lordship pleases:—"The Royal Society for the Prevention of Cruelty to Animals was founded to protect animals generally, and not for the suppression of any kind of cruelty in particular. The members of the society are drawn from all classes, and there is a considerable difference of opinion amongst some of them upon various subjects, although on others they are practically agreed. On the question of vivisection, for instance, there is a wide diversity of opinion, many persons objecting to any kind of vivisection; while others object only to painful experiments; others again consider that vivisection is necessary for the advancement of science in the interests of mankind. The attitude taken up by the society, almost since its foundation, has been that it deprecates all experiments on animals which cause pain, but as regards experiments which cause no pain there is no ground for interference by the society, because the question of cruelty does not arise. The society, therefore, is of the opinion that all severely painful experiments should be carried out while the animal in question is completely under the influence of an anæsthetic, and that it should be destroyed before the effect of the anæsthetic has been removed. The society has, ever since the passing of the Cruelty to Animals Act, 1876, repeatedly pointed out that the supervision of experiments carried on under licences granted under that Act is totally inadequate. It may not be possible that every experiment should be performed in the presence of an inspector, but all those should be which cause pain, either in their initial stages or afterwards. All severely painful experiments should only be performed in a particular place or places, so as to facilitate inspection; and all painful experiments should be prohibited except in the presence of an inspector. The number of experiments has increased since 1876 from between three and four hundred in that year to over 46,000 in 1906, and the number of inspectors (originally two and now three) is entirely out of proportion to the increase in the number of experiments. The number of Inspectors is absolutely inadequate if a proper supervision is to be exercised over the licensees. The Inspectors occasionally witness experiments, but very few. The inspectors' reports are drawn up from the information culled from the reports made to them by the licensees. It is impossible for the Inspectors to exercise proper supervision while experiments are going on every day in all parts of the country. The society is strongly of opinion that additional inspectors, who shall not necessarily consist of medical men, should be appointed, and that such inspectors should visit all licensed places frequently, at various times, unknown to the licensees, and that all holders of licences should be required to declare that no experiments, except those duly reported in accordance with the terms of the licence, have been performed, and the licensees should be required to make a return of all animals acquired by them, and how obtained. With a view of securing and maintaining the confidence of the public, the necessity of obtaining the consent of the Home Secretary for the prosecution of a licensee for an alleged offence should be abolished, and it should be left to the discretion of the magistrates, who are empowered to refuse a process if they consider an application is vexatious. The period for taking proceedings should be extended, so as to be within six months of the publication, or the giving information of that which is impugned. The society, soon after the Act of 1876 came into force, successfully instituted a prosecution under Section 6. The society has not been interested in or promoted any other prosecutions."

19489. In the case of the Royal Society, where something of the same kind was done, we thought it necessary to call the President of the Royal Society, so that any questions might be asked which occurred to any members of the Commissions arising upon their views, and we are following the same course in calling you and, as the society wishes it, another well-known member of the society. I do not know for myself that I have any questions to ask you, except whether there is anything that you wish to add to this statement of the society?—I do not know whether I might say that we are as anxious as the most extreme persons to abolish unnecessary pain, and we differ from them only in not attempting impossible legislation, while trying to improve legislation on this subject. That is our effort. The anti-vivisection societies, of course, occupy a special position, and carry out a special work, which could not be undertaken by an institution organised to conduct general operations by various modes of action, jeopardising other objects of a kindred nature on a far more extensive scale. For instance, drinking fountains have a society, and the Dogs' Home another of a special character.

19490. Your society has a very large number of members?—A very large number of members.

19491. Can you say how many?—I cannot really tell you that, but I can tell you how many officers we have. We have about 160 inspectors all over the country.

19492. And you spend a considerable income?—We spend in every way about £60,000 a year.

19493. When was your society established?—In 1824 I think it was.

19494. And it has been in existence ever since?—Yes.

19495. And in full operation?—Yes, and it has been growing and growing in the most extraordinary manner. I may, perhaps, say that I am what is called a man of leisure and, therefore, give up a great deal of my time as Chairman to the society. There is not a day passes when I have not some communication. I mention that to show that there is a great deal of work.

19496. A man of leisure means an unpaid busy man?—It seems so, and in my case I think I may say it is so.

19497. I think, perhaps, the best plan would be that we should hear Sir Frederick Banbury, and then if any of the members of the Commission wish, as I dare say some of them will, to ask questions first of you and then of Sir Frederick Banbury it will be better; but I think it is better to have first the general views of you both?—If you please.

19498. (*To Sir Frederick Banbury.*) Will you tell us what view you wish to speak to?—I, of course, only come here to represent the view of the society and not my own particular views upon the question of vivisection, but I think I can say that, speaking broadly, the view of the society is that while vivisection is permitted by law and animals are called upon to undergo, not for their own benefit, but for the benefit of man, operations which in their nature involve not merely the sacrifice of the life of the animal but the possibility of considerable suffering, every precaution should be taken to prevent pain to the animal creation. They are called upon to bear this burden for the general benefit of men; that is the idea, I believe.

19499. Of men and animals?—Men and animals; and it would seem to be an obvious principle of humanity and justice that man should concede to these dependent creatures all those safeguards against pain that he naturally demands for himself if he is undergoing a similar operation for his own benefit. Therefore, I think I may say (and Mr. Scott, I think, will agree with me) that the opinion of the society is that there are not a sufficient number of inspectors in order to see what actually is going on. Next, I think that, in the opinion of the society, it is not necessary that the Inspector should always be a medical man. I would not for a moment suggest that he should not be a medical man, but that that should not be an essential qualification, but that if there are other qualifications they should be considered perhaps before the qualification of being a medical man. Further, I think that another important point is that the Inspectors should give their whole time to the duties of their office and not hold any other appointment. In the Report of the Commission of 1876 I believe there is an observation that "publicity is the antidote of suspicion," and I am rather afraid that that observation of that Commission has not been carried out by the law, because I think there is a strong opinion amongst members of the society that the Inspectors obtain their reports very often, not from what they have seen themselves, but from what they are told by the people conducting the operations.

19500. Was that phrase, "publicity is the antidote of suspicion," used with reference to operations in laboratories, or was it used in the Report with reference to reporting cases brought into Court by the society?—I am afraid I cannot answer that. I have not referred myself to the Report; it has been given to me on, I think, good authority, that there is that statement in the Report, but whether it referred to experiments in laboratories or whether it referred to the case you have mentioned I cannot say.

19501. (*Sir William Collins.*) I have the passage here: "Publicity is the antidote of suspicion, and we look to the reasonable superintendence of constituted authority as affording the means of reconciling in the public mind the sentiment of humanity with the desire for scientific knowledge."

19502. (*Chairman.*) That looks as if it referred to laboratories?—Yes, I was not quite sure. I do not want to say anything that I am not absolutely certain about. I think that practically that is all I have to say. I do not know whether Mr. Scott bears me out.

(*Mr. Scott.*) Yes, I do. I think the inspection of returns should be the inspection of experiments; that is to say we want more inspectors. They seem to be able to inspect the returns, but they do not, I believe (in fact they could not) inspect all the experiments.

19503. You say that there were 46,000 experiments in 1906?—Yes, that is so.

19504. You are aware that the vast majority of those are inoculations?—Yes, quite painless.

19505. You do not mean that every one of those should be inspected?—No.

19506. Would you confine the inspection to any particular class of experiment. Some, of course, which are cutting operations are extremely slight?—Yes.

19507. And some are very serious?—Yes. What we say in our statement is that "All severely painful experiments should be carried out while the animal in question is completely under the influence of an anæsthetic."

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19508. That answers my question?—Yes.

(*Sir Frederick Banbury.*) Might I say one word? I did not quite finish. I ought to have said that with a view of facilitating the work of inspectors, the society is of opinion that the number of registered places should be to some extent limited.

(*Mr. Scott.*) Yes, that is so. The words of the Statute are, "Cruelly ill-treating, abusing, or torturing animals." Where no cruelty and certainly where no pain is caused in an experiment interference is not called for any more than when animals are destroyed for food.

19509. You are referring to the original Cruelty to Animals Act?—Yes, the original 1849 Act.

19510. Not the 1876 Act?—No.

19511. That is your most important Act?—Yes, the improvement on Martin's Act.

19512. (*Sir William Church.*) In your statement you say, "The society, therefore, is of the opinion that all severely painful experiments should be carried out while the animal in question is completely under the influence of an anæsthetic." Might I ask you what you consider a painful experiment?—It is rather difficult to define. I cannot give you anything in the way of a professional opinion. I am sorry to say I cannot do that, but there are operations which are, of course, notoriously painful, and such a one, we think, should be under an anæsthetic and the animal should be destroyed before the effects of the anæsthetic had been removed.

19513. Is not that the present law?—Is that so? We have been reading in the papers about the brown dog at Battersea—I have seen something about that. That is a case in point.

19514. As you have referred to that, might I ask you what the circumstances are with regard to the brown dog at Battersea?—I do not know them. I only know that I read in the papers that the students, I am not surprised, were annoyed at some inscription.

19515. Before you advance that as an instance should you not know something of the circumstances?—The dog, I understand, was twice operated upon. I thought it was understood that it was brought round (I do not know it of my own knowledge, but I understand that it was the case) and operated upon again and suffered a great deal of pain; the forceps were in the dog.

19516. Might I ask for a little more particulars about the dog, as you have instanced the brown dog of Battersea. Do you know when it was first operated upon?—No, I do not know, I am sorry to say, but I understood that that was the case.

19517. Do you know what the character of the operation was?—No, I do not. I believe it was a painful one.

19518. Was the dog under anæsthetics at the time the operation was done?—I understand so.

19519. Do you know when the second operation was done upon it?—No.

19520. At what interval of time after the first?—Very soon I am told.

19521. You are not aware that between two and three months elapsed?—Is that so? It was about the third day, was it not?

19522. Are you aware of the condition the brown dog was in during the two or three months that elapsed between the first operation and the second?—No, I am not. I think there were three operations altogether. (*Sir Frederick Banbury.*) If I might answer the question, because I happen to have read the evidence at the trial, the evidence at the trial to the best of my recollection (I have not refreshed my memory with it to-day) was that there were three operations upon the brown dog: that the first operation took place some two or three weeks, or a month, before the second, I could not say which, but that at the end of the second operation the dog was handed over to another operator, the operator who performed the first operation quitted the building, leaving the dog in the hands of the operator for the third operation. That, to the best of my recollection, was not denied in Court.

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19523. Was the dog allowed to recover from the anæsthetic between the second and the third operations?—Is it possible for a dog to be under an anæsthetic for so many hours as that dog was without recovering consciousness?

19524. I suppose you will not take it from me that you can keep a dog under anæsthetics as long as you like almost, that is to say, up to six, eight, ten or twelve hours?—I am not an expert, but I am informed by experts that it is almost impossible.

19525. You have no knowledge of your own on the subject?—No, I am not an expert.

19526. Would you accept it from me that the dog was under anæsthetics at the end of the second operation, and was not allowed to recover from that anæsthesia until after the third operation was performed?—My desire is to accept everything that you say, but in this particular case I do not think I can.

19527. (*To Mr. Scott.*) I should like to go back to what is considered by your society a painful operation, because you say that you have no objection to inoculation experiments?—No, we have none, I think. They are classified, certainly, in the return to the Home Office under different heads.

19528. With regard to inoculation experiments you say apparently that your society does not regard them as painful?—We believe them not to be painful.

19529. But we have had a good many witnesses here who have objected to inoculation experiments very strongly, saying that they are extremely painful in some cases; not the inoculation itself, but the result of the inoculation. What is the view of your society about that?—That all painful experiments should be prohibited except in the presence of an inspector.

19530. That is just my point. I am speaking of these inoculation experiments. You say that they are not painful, but we have had a good many witnesses who have objected to them on the ground that their results are painful?—If the Inspector is present and pronounces an experiment not to be painful I suppose we should accept it.

19531. (*Chairman.*) I understood you to say when you spoke of 46,000 operations that you were aware that a very large majority of those were merely in the nature of inoculations, and that you never thought of there being any necessity for inspectors to be present at those?—No, I do not consider, speaking for myself, that those would be painful, and I think that is the view of the society, unless they produce pain, which we do not think they do.

19532. (*Sir William Church.*) But my question was this. I wished to know whether your society objected to inoculation experiments on the same grounds as many witnesses have, that the results may be painful?—Yes, in that case we should.

19533. But you cannot tell what your results will be?—We say, "All painful experiments should be prohibited except in the presence of an inspector"—he must be the judge.

19534. Do you know what these inoculation experiments are chiefly done for; are you not aware that the large bulk of them take place in laboratories connected with public health for the purpose of diagnosis of disease and preventing its spread?—Yes, I think that would be a very noble and proper thing.

19535. But that is what the large bulk of these inoculation experiments are done for?—Yes.

19536. And, of course, you cannot tell whether the result of an inoculation may cause discomfort to an animal or not until after the proper time has elapsed to see what has been the result of the inoculation?—Our view, as we have expressed it, is that it is not possible that every experiment should be performed in the presence of an inspector, but that all those should be which cause pain either in their initial stages or afterwards.

19537. Then am I to take it that your society does object to inoculation experiments for the purposes of diagnosis?—Anything that causes pain to the animal afterwards we should object to.

19538. The Inspector would have to be present then and give his consent or not according as he thought that the result of an injection experiment might or might not be painful. Is that what I am to take from you?—I think so.

19539. That, of course, would put a stop to the use of inoculation experiments, possibly for the purposes of public health?—I am sorry to say that not being a professional man I am not able to follow some of these questions.

19540. Might I put it in rather a different way. Would your society prosecute a man who overrode or overdrove a horse when going to the assistance of another man?—I think very likely if we had all the facts of the case before us we probably should not.

19541. But yet you would not allow an inoculation experiment which might or might not have the result of saving a great many human lives, I mean by preventing the spread of an epidemic or anything of that kind, unless the Inspector was assured that the result of the experiment could not be painful?—That is the line we take. We want to defend the animal as much as we can—that is our object—what the society exists for. I only give the general views. I am not giving my own opinions at all.

19542. I want to get the opinion of your society?—Yes, I am trying to give it you.

19543. I think you will admit that it would be very hard for the Inspector, unless he had some fixed rule whenever any material was going to be injected that might give rise to pain or discomfort in the animal, to say that it was not to be allowed; he would have great difficulty in knowing what injections he should permit and what he should refuse permission to?—We say, "All painful experiments should be prohibited except in the presence of an inspector."

19544. Might I ask what you would consider the qualifications for an inspector?—I should imagine that the man should know something of anatomy and surgery generally, and I suppose that really he would have to have some knowledge of hospitals and something of that sort. I suppose that would be necessary before the man could give an opinion.

19545. You think that they should not all be medical men?—Not necessarily medical men. There seems to be an idea that they might be men of some experience who were not medical men.

19546. Still, I should like to have some more definite idea what you think should be the qualifications for an inspector?—We have no objection, so far as I know, to the qualifications that exist now, only we say that there are too few of them.

19547. (*Mr. Ram.*) Too few inspectors?—Yes.

19548. (*Sir William Church.*) Hitherto all the inspectors have been, I think, medical men?—That I am not sure of. They are appointed by the Home Office. I should think they most likely were, but there are very few of them.

19549. (*Chairman.*) Do you understand that these inspectors are to be persons who should have authority to say to a professor of physiology in a university, who is going to perform an experiment under anæsthetics with a certificate from the College of Physicians, that the animals should be allowed to recover, and therefore feel pain, "In my opinion this will cause pain to the animal, and it must be stopped." Surely all he has to do is to report any breach of the law?—I should say yes.

19550. He would not have any preliminary authority to stop experiments which appeared to be in conformity with the Act, although they caused pain?—No. If a license has been granted for a painful experiment we say that it ought to be performed in the presence of an inspector.

19551. (*Sir William Church.*) You cannot, therefore, give the Commission any assistance with regard to what the qualifications of these inspectors should be?—I am afraid very little.

19552. Has Mr. Coleridge placed before your society his amended Bill for experimentation on animals?—I do not know whether he has or not. I rather think that he has very much taken up the lines of the Bill that we ourselves proposed.

19553. Have you sent us a copy of your proposed Bill for the prevention of cruelty to animals?—No, we have not. Nor have we one.

19554. (*Sir Mackenzie Chalmers.*) Have you not got one?—No. (*Sir Frederick Banbury.*) I introduced one in the House some two or three years ago.

19555. (*Sir William Church.*) But not by your society?—Well, with our approval. It was not drawn up by the society. It was really Mr. Stephen

Coleridge's Bill amended by myself—his Bill of that date.

19556. (To *Mr. Scott.*) You are aware that in his amended Bill he proposes that before a man receives a licence for being allowed to experiment on animals he should get a certificate of humanity?—This is what you refer to, I think: "A certificate recommending that the application be granted on account of the applicant's reputation for humanity, which must be signed by two or more of the following persons, that is to say, a Justice of the Peace, a minister of any religious denomination in the United Kingdom."

19557. Yes, that is what I refer to. Is that approved by your society?—No, I cannot say that it is. We have not had it before us.

19558. Do you think that that certificate would be of any value?—I do not think it would be, really.

19559. Then you would also approve of its being in the power of anyone, without the consent of the Home Office, to institute a prosecution against anyone who is stated to be guilty of cruelty in experimentation?—Our view is, "With a view of securing, and maintaining, the confidence of the public, the necessity of obtaining the consent of the Home Secretary for the prosecution of a licensee for an alleged offence should be abolished, and it should be left to the discretion of the magistrates who are empowered to refuse a process if they consider an application is vexatious." That is what we prefer.

19560. At the present moment anyone can prosecute a person guilty of cruelty to animals under the Cruelty to Animals Act?—Yes.

19561. And your proposal would amount to a similar method of prosecution in the case of those who hold licenses for experimentation. That is the meaning of it?—We mean that it should go before a magistrate in the same way as anything else, and should not be left to the Home Secretary, which, of course, causes necessarily a great deal of delay, and very often no proceedings can be taken, because the period has passed when we get the information of anything like an abuse of the Act.

19562. You do not think that that might be a very vexatious clause to those who are engaged in this work in laboratories?—It did not occur to me that it would be so at all.

19563. Considering the statements that you must be aware are made about the experimentation that goes on in laboratories, do not you think that very often cases would be brought forward before magistrates who could not judge at all of the rights of the case, which would be vexatious?—No, I think the ordinary magistrate would be a man of common sense, who would grant a summons or not, as he thought right, and the process would not go on.

19564. In judging whether an experiment is cruel or not, is not there a considerable amount of what is generally considered physiological knowledge required?—I should say certainly.

19565. Is that possessed by the public and the majority of magistrates?—I should be afraid not.

19566. Therefore is it not only possible but probable that in perfectly innocent ignorance, a great many prosecutions might arise?—So far as the society is concerned every possible care is taken never to prosecute unless it is absolutely necessary. We meet and discuss these matters. No prosecution can take place without the consent of the parent body.

19567. You are speaking now of your society; I am speaking of the general public?—I beg your pardon; I did not follow you. It did not occur to me in that way. So fas as we are concerned, I am quite sure that we should not prosecute unless we had a very good case.

19568. Have your society any evidence whatever that unnecessary cruelty is practised in laboratories?—We have never had but one prosecution, and that was for advertising, under the Act of 1876. That is the only one we have had, and it is very curious that it should be in that connection. I have it here, if I may just read it to you. This is from our report for 1876: "First conviction under the Vivisection Act.—The Cruelty to Animals Act, 39 and 40 Vict., c. 77, came into operation on the 15th of August last. Three days later Dr. Abrath, of Sunderland, issued a large placard headed, 'The Balham Mystery,' announcing his intention to deliver a lecture at Sunderland on 'Antimony,' when he would perform experiments on animals to show the effects of poisons, and to demonstrate his theory that Mr. Bravo was not killed by that drug. The branch of the Royal Society for the Prevention of Cruelty to Animals at Sunderland immediately reported the matter to the secretary in London, and prompt measures were taken to prevent the learned gentleman from performing his experiments. According to the 6th Section of the Statute named above, an offence had already been committed by the announcement of a public exhibition of experiments on animals, and Dr. Abrath, having spoken contemptuously of the new Statute at his lecture, and provoked the derision of a portion of his audience against it, instead of apologising for his projected defiance of the Act, was summoned to appear yesterday before the Sunderland Borough Bench, at the instance of the R.S.P.C.A., when he was fined for publishing the illegal placard alluded to."

19569. Might I ask who Dr. Abrath was? Was he the holder of a licence at the time?—No.

19570. Then that case did not come under the Act?—Yes, it came under it in this way. This is the section: "Any exhibition to the general public, whether admitted on payment of money or gratuitously, of experiments on living animals calculated to give pain, shall be illegal."

19571. That is, illegal to the holder of the licence?—"And any person publishing any notice of any such intended exhibition by advertisement in a newspaper, placard, or otherwise shall be liable to a penalty not exceeding one pound." This man's offence was under that Act, of asking the public to come and see this experiment, and for advertising that he was going to perform it.

19572. But the gentleman in question was not the holder of a licence?—No.

19573. He did not propose that these experiments that he wished to show to the public should be done under the Act?—His offence against the Act was that he proposed that there should be a public exhibition; that is where the offence came in.

19574. That had nothing to do with the Act regulating experimentation on animals. In that Act there is a special clause that an experiment should not be performed in public?—That is just what I was saying. You were asking me about these matters. It is the only conviction we have ever had, and that was simply for advertising.

19575. He was not a licensee?—I am not able to say that, but his offence was advertising the performance of the operation.

19576. Then your society, which has, I understand, £60,000 annually at its disposal, has never been able to bring forward instances of unnecessary or wanton cruelties having been performed in laboratories?—No, I have no case.

19577. Might I not infer from that that the probability is that wanton and unnecessary cruelty does not exist in laboratories?—That is a difficult question for me to answer. We say that there are not enough inspectors, and that the Inspector inspects the returns but does not inspect the experiments. Only three inspectors cannot possibly do that.

(*Sir Frederick Banbury.*) If you give us leave to be present at an inspection we will answer the question.

19578. (*To Mr. Scott.*) Do you think it possible for an inspector to be present at every operation done in a laboratory?—I think at a great many they might be. At present it is not possible.

19579. Have you any idea of the number of laboratories that there are in this country?—I have a list of them here. I have not counted them.

19580. Do not you think it would be a great drawback to science if a man's work had to be stopped in order that a communication might be made with the Inspector that he was to be present at such and such a time, and then the Inspector said he could not be present at such and such a time?—Then I think they should arrange between them that the Inspector should be present, as it is the law that he should.

19581. How many inspectors do you propose there should be for the Kingdom?—It is impossible to say.

19582. What sort of remuneration would you give these inspectors?—Whatever they are satisfied with

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now I daresay others would be satisfied with. I daresay it would be very costly, but it would be money well expended, would it not?

19583. What grounds have you for thinking that an inspector would be more likely to tell the truth or to stop what should not go on than the gentlemen who at present are in charge of these laboratories and are responsible for what goes on in them?—I have the greatest admiration for your profession, I assure you——

19584. I beg your pardon, you are under a misapprehension. A large number of the heads of these laboratories in which experimentation on animals goes on are not members of my profession, or not necessarily members of my profession?—I thought you would hardly throw that back at me.

19585. But as a matter of fact some of the most distinguished experimenters on animals are not members of the medical profession, but men of science?—Yes, men of science certainly.

19586. What reason is there for thinking that the Inspector would be able to keep better—I will say order—in the laboratories than these gentlemen themselves?—It is the law that they should be inspected, and therefore I think that it should be carried out; that is all.

19587. Besides the experiments in laboratories, are you aware what other points the Inspectors direct their attention to?—No; I am not.

19588. You do not know that it is part of the duties of the Inspectors, which they always do, to examine into the condition of the laboratories?—I understood that they were licensed after being examined.

19589. The manner in which the animals are kept, both before and after experimentation?—Yes.

19590. You were aware of that?—Yes; I knew it.

19591. Although visits to laboratories are not very frequent now, do you consider that they are sufficiently frequent to enable the Inspector to form an idea whether every proper care is taken of the animals or not?—I should think not. I do not think that there are enough inspectors to be able to do that.

19592. How frequently do you think visits to a laboratory should be made by an inspector to see that the animals are properly taken care of?—I should think almost weekly—every day almost.

19593. Daily and weekly?—Yes; something of that sort. I do not see how a man can say that a thing is properly done unless he goes frequently to see that it is so.

19594. You do not have your dairies daily or weekly inspected?—No; but I inspected my own stable every day. I did not trust them, however good the men might be.

19595. Therefore it would be almost necessary to have an inspector for each laboratory?—I do not think so. I should think they would have districts, could they not? A man could pay a surprise visit on Monday to one, and on Wednesday to another, and so on. We do not think there are sufficient inspectors. If there are to be inspectors there should be a certain number of them.

19596. I wished to gather from you some idea of what number you thought would be necessary?—A sufficient number to look after these laboratories and other places and keep them up to the mark.

19597. If a laboratory has to be visited daily a man could only visit one or two if they happened to be near to one another on the same day?—More than that I should have thought.

19598. How many do you think?—I should imagine that if they are close to one another it would not be a difficult thing for a man to visit many in a day to see that the laboratory is properly conducted, and that the animals are being well looked after.

19599. And to be present at every operation?—Yes, he would have to make those arrangements to fill up his time. I mean to say he would have to arrange his time.

19600. Therefore, taking such a small place as London no operations could be done on several days in the week in some of the laboratories because he would be visiting others?—But "a small place like London" the laboratories, I should assume, and not the size of the place.

19601. I mean small as regards its area for travelling?—Yes.

19602. (*Sir Williiam Collins.*) Your society gave evidence before the last Royal Commission in 1876, did it not?—Yes.

19603. Through Mr. Colam?—Yes, the secretary.

19604. And they put in a letter addressed by command of her late Majesty the Queen to the president of the society?—Yes, to Lord Harrowby.

19605. And there was a good deal of documentary evidence put in by Mr. Colam purporting to show the existence of painful experiments?—I believe so. I was not there then.

19606. In the report of the Royal Commission of 1876 there is an allusion to your society. They state "We were reminded by the secretary that the Royal Society for the Prevention of Cruelty to Animals is not the society established for the total abolition of experiments. The Royal Society for the Prevention of Cruelty to Animals has prepared its Bill upon the supposition that experiments of a nature to cause pain are justifiable if they are performed when the animal has first been rendered wholly insensible to pain and is destroyed before the effect of the anæsthetic ceases." Is that still the position of your society?—Yes.

19607. Then another allusion is made to your society in the report of the previous Commission. They say, "The secretary of the Society for the Prevention of Cruelty to Animals when asked whether the general tendency of the scientific world in this country is at variance with humanity, says he believes it to be very different indeed from that of foreign physiologists; and while giving it as the opinion of the society that experiments are performed which are in their nature beyond any legitimate province of science, and that the pain which they inflict is pain which it is not justifiable to inflict even for the scientific object in view, he readily acknowledges that he does not know a single case of wanton cruelty, and that in general the English physiologists have used anæsthetics where they think they can do so with safety to the experiment." Is the experience of your society still in that direction?—Personally, I say so certainly.

19608. You have told us of one prosecution instituted by your society successfully?—Yes.

19609. Is that the only prosecution that they have instituted?—That is the only one we have ever had.

19610. Has your society any reason to think that there is considerable suffering to animals under the operation of the existing Act?—It has been suggested to us that there is.

19611. The Commission was told by Dr. Starling in answer to Question 3451: "Though I have been engaged in the experimental pursuit of physiology for the last 17 years, on no occasion have I ever seen pain inflicted in any experiment on a dog or cat, or, I might add, a rabbit, in a physiological laboratory in this country, and my testimony would be borne out by that any one engaged in experimental work in this country." Have you any remark to make with regard to that?—I should think that is very strong evidence in favour of what you have read of Mr. Colam's evidence; it is corroborated very much.

19612. We were told by Dr. Thane, the Inspector under the Act, in answer to Question 457: "On the other hand it is certain that in some cases of this group, that is, experiments performed under Certificate A, the infection or injection is followed by great pain and suffering. I may mention the injection of tetanus toxin and the infection with plague; also the insertion of certain drugs." Have you any remark to make in regard to that?—No, that is the experience of a surgeon, is it?

19613. It is the Inspector?—I beg your pardon.

19614. What I want to gather is what would be the wish of your society in regard to such cases in which after injection or infection there is great pain and suffering.

(*Sir Frederick Banbury.*) We think the animal ought to be destroyed.

19615. Then a question has been put as to the difficulty of determining in which cases of injection or means so many inspectors according to the number of infection considerable suffering may result. Do I correctly understand your suggestion to be that in all

cases in which such result may happen the animal should be kept constantly under the eye of the Inspector?—If there is any risk of serious suffering, yes, I think so.

19616. That would almost necessitate the residence of an Inspector on licensed premises, would it not?—No, I do not think so, because the Inspector would know that a certain operation was going to take place and the operator would know when the result would begin to show itself. When the result began to show itself then I think the Inspector should see, if there is pain, that the animal should be destroyed.

19617. But I am afraid that in the case of some of these injections or infections the date at which the operation of the particular injection begins to take effect may be doubtful even in the mind of the experimenter. What I am anxious to know is what suggestion the society would make with a view to secure as far as possible that no case of suffering should result in animals subjected to injection or infection?—I do not think that the society could take upon itself to suggest a law which should never make mistakes. It is one of those things that we should have to accept as inevitable. I think the opinion of the society is that if vivisection takes place pain must sometimes result. The wish of the society is to limit that pain as far as possible.

19618. I think I gathered that you stated that the society would wish the number of licensed places to be as few as possible?—I will not say as few as possible.

19619. Or reduced?—Yes, and grouped together more.

19620. Was that with a view to secure more frequent inspection?—Yes, certainly.

19621. Do you think that valuable results have attended the operations of the Inspectors in the past as regards preventing or detecting breaches of the Act?—I do not think that any result has happened at all. We cannot give an opinion upon that because so far as we know many of the Inspectors do not really ask what has taken place; they take the opinion of the operator. It may be all right, but it may not be. You have already instanced an Inspector who says that pain has taken place. Previously to that you instanced one eminent man of science (I believe not a member of the medical profession) who says that no pain ever does take place. I cannot say which is right. I have my own opinion.

19622. We have had evidence from witnesses in both directions, some setting great store upon inspection as being valuable, and others regarding the present inspections as superfluous. I was rather anxious to hear from either of you gentlemen what view you take of the present inspection so far as it goes. Is it superfluous or is it valuable?—I think it is better than nothing.

19623. Then I should like to know, if possible, what opinion the society has in regard to a question that has also been raised here by witnesses as to the effect of repealing the Act of 1876. Do you think that animals would be in a better position *qua* scientific experiment if the Act were repealed and they were left to the operation of the ordinary law, than if the present Act were maintained or modified or amended?—That is a legal question. I do not think I could answer it as to what power a society like our own would have under the ordinary Prevention of Cruelty to Animals Act. (*Mr. Scott.*) I am sorry to say that I cannot answer it.

19624. (To *Sir Frederick Banbury.*) I understand that you introduced a Bill into the House which had the approval of the society?—Yes.

19625. In what direction did that Bill go?—Unfortunately I live in the country, and have not got the Bill with me, but it more or less was Mr. Stephen Coleridge's Bill altered in a way that I thought would be more acceptable to the House of Commons. My recollection goes that it left out the clause with regard to the certificate for humanity, because I do not see how that is practicable; or the object of the Bill was to prevent experiments being made for the purposes of illustrating a lecture or to demonstrate facts which were already well known; experiments could only be made in the presence of the Inspector and in order to ascertain something which had not already been ascertained. That was, roughly speaking, the object of the Bill.

19626. Did it prohibit experiment on animals which would be painful?—No. All that it did was to say that they must be under anæsthetics and the animals must be destroyed, but it did not prohibit experiment, provided that the animal was under anæsthetics and was destroyed before it recovered, and provided that it was shown that it was in the interest of humanity to discover something which had not previously been known.

19627. Would it have prohibited an injection experiment which might have resulted in painful disease?—I do not think it would; but the result would have been that when a painful result ensued, the animal would have been destroyed.

19628. (*Sir John McFadyean.*) (To *Mr. Scott.*) You have already explained that this statement of evidence has been drawn up by, or has been before, your council?—It was drawn up by the council.

19629. Does it contain anything that you are not prepared to subscribe to yourself?—I am only here as representing the council, you know.

19630. Does it contain anything that you are not prepared to subscribe to yourself?—I do not think that it does. (*Sir Frederick Banbury.*) I think I subscribe to everything.

19631. (To *Mr. Scott.*) I thought, very likely, you did subscribe to it. Your society was founded for the Prevention of Cruelty to Animals. Would you mind telling us what is the society's definition of cruelty?—"Cruelly ill-treating, abusing or torturing animals."

19632. Take cruelty by itself without the torturing. What do you understand by cruelty?—I see a good many forms of it. I think cruelty would be to over-bit a horse, if you want an extreme case.

19633. I can get what I want better in this way. The term "cruelty" is not used as if it were synonymous with the expression "pain," is it—for preventing pain to animals?—It would be certainly for preventing pain.

19634. Do you think you would quite express the views of your society if you were to substitute "pain" for "cruelty?"—I think so. I think to starve an animal would be painful. There are so many forms of cruelty that I do not wish to commit myself to the fact that it must necessarily cause pain. We call ourselves the Society for the Prevention of Cruelty to Animals, and I have no wish to alter the word at all.

19635. But the point is really this: Do you think that your society recognise that it is ever justifiable to cause pain to an animal for some other purpose than that individual animal's own benefit?—In all painful experiments we consider that the animal ought to be protected by being under anæsthetics.

19636. That is to say, it should be prevented from suffering any pain?—Yes.

19637. That seems to be an affirmative answer to my question?—Yes, pain should be prevented.

19638. Then your society denies the right of man to cause pain to an animal except for that individual animal's own good?—Oh, no! I do not say that, under an anæsthetic, we say.

19639. But that would not cause pain?—It might.

19640. You mean that it must be an efficient administration of anæsthetics?—Yes.

19641. In which case there would be no pain?—No. (*Sir Frederick Banbury.*) It depends upon the amount of the pain.

19642. If you do not mind, I will deal with your Chairman in the first instance. (To *Mr. Scott.*) Do you think it would perhaps better express the objects of your society if, instead of substituting the word pain for cruelty, that is to say, "for the prevention of pain to animals," you were to put in "for the prevention of unnecessary pain." Is that what you mean by cruelty—unnecessary pain?—That would answer the purpose very well. It is unnecessary pain that we desire to prevent. A man spurs a horse unnecessarily very often to get it over a bit of timber, and so on.

19643. In its operations does your society find it necessary to discriminate between necessary and unnecessary pain?—No, but we do discriminate between a man ignorant very often of what he is doing, and a man who is not ignorant, but does it of set purpose and knows what he is doing. You may have an ignorant man inflicting pain from ignorance.

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19644. But all pain inflicted by an individual who was conscious that what he did would cause pain you would regard as cruelty?—Yes.

19645. But I thought you had already told us that your society would not proceed against a man who rode a horse to death on the chance of saving a human life?—It would depend altogether upon the case. We should consider that matter. If the circumstances were such it would be a matter of very grave consideration whether we should prosecute in such a case. There might be a difference of opinion.

19646. But if the case came before your council, you think it would really cause them some difficulty to decide whether a man who had ridden furiously to save a human life, and had saved it, but had caused the horse's death, ought to be prosecuted?—It is a question whether the same object might not have been attained without causing the pain.

19647. I will ask you to assume, please, that there was no evidence to show that the human life could have been saved in any other way?—That means to say that the horse could not be galloped fast enough without spurs, do you mean?

19648. No, I mean that if this particular horse had not been ridden to death the human life would have been lost?—I say that if we knew that the man knew that he was going to kill that horse we should prosecute him.

19649. Supposing that there were a possibility that he might, by riding the horse furiously, save a human life without causing the horse's death, you will admit that he would cause the horse very great distress and pain?—Yes.

19650. Would your society prosecute then?—If it was unnecessary pain—for instance, if the man rode the horse with spurs and could get the same pace out of the horse without spurs.

19651. It is not a question of spurs at all; it is a question of causing pain?—But if the man could have attained the object without killing or ill-treating the horse we should then prosecute him, because the same object could have been attained without cruelty.

19652. But I ask you to assume that the object could not have been attained in any other way?—Then I should put it before the council.

19653. You think possibly that the council might consider his action justifiable?—Possibly.

19654. On the principle that the life of a man is more valuable than the life of a horse?—Yes, I agree "of more value than many sparrows." I believe that certainly.

19655. Can you tell me how you discriminate between a man's action in that case and the action of a person engaged in research who causes a certain amount of pain to an animal in the confident belief that he can thereby gain knowledge that will afterwards be useful for the saving of human life and alleviating human suffering, and, also, perhaps, alleviating suffering among animals too. The case is this: you justify the sacrificing of the horse's life to save a human being in the particular circumstances that I put to you?—I did not quite agree to that.

19656. Then I will put the question again. I asked how do you discriminate between that and another case in which an experimenter would sacrifice a horse's life to extend knowledge which might be useful, and which he believed would be serviceable for saving human life. I asked you to discriminate between the two, and you said that possibly your council would not proceed against the individual who rode a horse to death to save a human life, but I understand from you that they would object to any pain being caused to a horse to obtain knowledge that would be useful for saving human life?—That is so.

19657. What is the principle that enables you to discriminate between the two cases?—One is certainly a chance and the other is a fact. We say: "The society if of the opinion that all severely painful experiments should be carried out while the animal in question is completely under the influence of an anæsthetic"; and if in the case in point the animal is under an anæsthetic we say that we should agree to it, but that it should be destroyed before the effect of the anæsthetic has been removed.

19657A. That is no answer to my question at all. The hypothetical case that I put to you was one in which the animal would be caused a certain amount of pain in an experiment?—We object to that.

19658. When you advise us that there ought to be inspection in all cases in which the experiment is of a severely painful character, does that cover also those cases in which the licensee has to have the animal thoroughly under the influence of an anæsthetic all the time. Do you call that a severely painful experiment?—I should imagine it would be.

19659. Although the animal is under an anæsthetic all the time?—Yes. That is the protection which we think the animal ought to have.

19660. But you only mean that it would be severely painful if the animal were not anæsthetised?—Yes, that is what I understood you to mean.

19661. What is the purpose of having an inspector present; is it to judge as to whether the anæsthesia is efficient or not?—I assume that that is one of the reasons.

19662. What other reason would there be for his presence?—Because by law he has to be there to see that the thing is properly carried out.

19663. You think that under the existing law the Inspector must be present?—Yes, that I believe to be the law.

19664. Have you read the Act?—No.

19665. Do you think you could find any passage in it which lays that down?—We look upon it in this light. I have already said that we think the Inspector refers to the returns and not to the experiments—that the Inspector guarantees the returns, but that he has never seen the experiments.

19666. I do not think we have ever had it suggested that the Act lays it down that the Inspector is to be present at every experiment or even at every painful experiment?—We consider that all painful experiments should be prohibited except in the presence of an inspector.

19667. Does that mean that you think that that is the state of the existing law, or that the law should be amended in that way?—If it is not so we think it ought to be so.

19668. I ask you your view?—You see how we refer to that: "It may not be possible that every experiment should be performed in the presence of an inspector, but all those should be which cause pain, either in their initial stages or afterwards." That is the question you asked me, I think?

19669. No. I asked you to give us the reasons why you thought it necessary to have an inspector present. Is there any reason beyond his being there to judge of the efficiency of the anæsthesia?—That is what we mean.

19670. Was there any other reason?—Yes, to see that the experiment was properly carried out, and that it was under the particular certificate that is granted.

19671. You have expressed already a high admiration for medical men in general?—Yes, I have, and have reason to believe so.

19672. In spite of Sir William Church's partial disclaimer I am afraid that we must admit that the great majority of those who experiment are members of the medical profession?—I thought so.

19673. Have you any reason to think that at the present time any one of the licensed experimenters would willingly violate the law. For instance, do you think that there is any licensed experimenter who would deliberately or even carelessly neglect to administer an anæsthetic in sufficient quantity in those cases in which the certificate prescribes the use of anæsthetics?—I do not know—it is quite possible.

19674. I did not ask you whether it is possible; I asked you whether you think it is at all likely?—I repeat what I have already said. I believe the medical profession to be very highly honourable men and men who from a high sense of honour would do the best they could to carry out what they undertook to do. But we know that in all professions there are men who are not what we wish them to be. We cannot help that.

19675. Have you read the Act?—No, I have not.

19676. Are you aware that the application for a licence has to be accompanied by a recommendation?—Yes, I know that. I have it here.

19677. Does not that appear to you to be a sufficient guarantee that the individual will be at least an average member of the profession?—Yes, certainly, I put him a bit above that.

19678. Then unless you have some very strong misgiving as to whether the Act is violated or not in such a way as to cause cruelty to animals, does it appear worth while to go to the great expense that will be involved in having every experiment witnessed?—We classify them here by saying that all "severely painful experiments should only be performed in a particular place or places so as to facilitate inspection; and all painful experiments should be prohibited except in the presence of an inspector." That is our view.

19679. I suggest, though you may not have calculated it out, that that would be very expensive?—Yes.

19680. And I further suggest that unless you have a very strong misgiving as to the way in which experimentation is carried on at the present time it does not appear to be worth while to incur that expense?—That has always been our line, I think, ever since the Act passed. I think we have always taken that view that there were not enough inspectors.

19681. (*Sir Mackenzie Chalmers.*) You told us that your society had had one prosecution?—Yes.

19682. That was about 1877?—I think it was in 1876, soon after the Act came in.

19683. And it was perfectly effective?—Yes.

19684. It stopped once and for all any attempt at a public demonstration?—I think it did; we have never had another case.

19685. That was a successful piece of work?—Yes.

19686. I take it that the object of your society generally is to prevent cruelty and not pain—cruelty to animals, that is to say, pain which is not justifiably caused?—Yes.

19687. You do not disapprove of sport, do you?—Personally, I do not.

19688. I mean that necessarily there is a great amount of pain caused to animals in sport which might be saved; there are a great number of wounded animals?—Yes, I am afraid it must be so always.

19689. But that is not cruelty; the pain is not caused intentionally?—No.

19690. The intention is to kill; therefore it is outside the scope of your society?—Yes. If a man overrode a horse out hunting that would be cruelty; or sometimes we have had cases of cruelty brought to our notice with carted deer.

19691. But it would not be cruelty if a man overrode a fox?—I wish we could take the fellow up for it, but you cannot.

19692. (*Chairman.*) You have prosecuted for overspurring in a race?—Yes, we have.

19693. (*Sir Mackenzie Chalmers.*) And for overflogging in a race?—Yes, for over-flogging too.

19694. I suppose you have never considered whether the amount of pain that is caused in the whole of the laboratories in this Kingdom would be equal to the pain caused on one day's rabbit shooting?—No, I have never considered it in that light.

19695. Your society again would not, I imagine, touch commercial operations, would it? For instance, every horse, as we known in London, is gelded?—That we have never taken up, of course.

19696. That is for commercial purposes?—Yes, it has never been taken up. (*Sir Frederick Banbury.*) We have taken up docking and we should take up gelding if it was done by an unskilful person and done without the necessary precautions.

19697. (To *Mr. Scott.*) As a matter of fact it is very seldom done under anæsthetics, is it. Perhaps you have not gone into it?—I have never had one done.

19698. It would be outside the scope of the society?—Yes. Of course it is mutilation in a sense, but it is considered a necessity.

19699. Under the Vivisection Act no such operation would be allowed to be done without anæsthetics and under the aseptic condition?—I believe as a rule it is done very young with a horse, and in later life when it has been done I believe it is done under chloroform.

19700. We have had some evidence on the subject, but I need not trouble you with it if you have not considered it?—I once had a stallion I used as a hack, but it is very rarely this is done.

19701. I want to get at your ideas about inspection. I quite understand that the public would feel more confidence if there was more inspection?—That is our view.

19702. But I want rather to try and see what would satisfy the bulk of your society, because there are some difficulties to be considered. Last year 20,795 inoculations were made on mice for the Cancer Research Fund, and I want you to take that as an example. The object of the inoculation is to produce cancer for the purpose of watching it and seeing whether any alleviation can be obtained for a most horrible human malady. Do you suggest that the Inspector should be constantly watching those mice as the tumour may develop?—There are difficulties of detail like that, I confess. I see what your point is, of course.

19703. 20,000 experiments are half the experiments performed in the whole year. I will give you the exact figure: 20,795 inoculations on mice out of 21,082 inoculations made for the Cancer Research Fund and 1,200 on other small animals. You have considered that?—I have not considered that the Inspector should watch every one of those. I do not think it would be reasonable.

19704. Within the principle that you have laid down the initial operation would be painless as being inoculation, but afterwards more or less painful symptoms would develop in those mice. Cancer is sometimes painless and sometimes not?—Yes.

19705. Then there is another point which shows the difficulty. 2,144 experiments were to produce tubercle, and tubercle produced in an animal may or may not give rise to a considerable amount of pain. Would there not be a difficulty there if you laid down your principle?—There would, of course, be considerable difficulty. I quite admit all that.

19706. We all agree, I think, that some extra inspection would give extra confidence?—That is really what we want to secure—the confidence of the public.

19707. But you agree that there are difficulties if you state a proposition in an Act of Parliament which would be unworkable?—Yes, I quite see all these difficulties, and I am very glad that you have pointed them out.

19708. Then 6,000 experiments were performed for Government Departments, County Councils, Municipal Corporations, and other public authorities. Those would be chiefly in testing food. There again there would be somewhat the same difficulty?—No doubt.

19709. Now I should like to ask you about what most people call vivisection—namely, cutting experiments. As I understand your evidence, you have no objection to experiments done under a licence alone. You know the difference between a licence and a certificate?—I think so.

19710. May I put it to you, and then we shall see how far we agree. Under a licence alone, which is granted by the Secretary of State, an animal that is experimented on must be put under an anæsthetic, and any operation may be done upon it, but the animal must never recover from the anæsthetic. The animal must be put under an anæsthetic, and there is no limit to the time, provided that the animal never recovers from the anæsthesia until death puts an end to it. Have you any suggestion to make as to that?—That is just what we say.

19711. That is, under a licence alone?—Yes.

19712. Then you know if any departure is to be made from that a special certificate, signed by two eminent authorities, has to be obtained?—Yes.

19713. Under Certificate B, if any cutting experiment is to be performed more than the prick of a hypodermic needle, the animal has to be under anæsthetics the whole time, but then the animal is allowed to recover, and may be kept alive until the object of the experiment is obtained. Have you any objection to that?—That is where we do object. We say that the animal should be destroyed before the effect of the anæsthetic has been removed.

19714. Then that would render impossible any physiological experiment, would it not? Have you any comment to make upon what the Inspector says in his last report, where he is referring to Certificate

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B: "The operations are required to be performed antiseptically, so that the healing of the wounds shall, as far as possible, take place without pain. If the antiseptic precautions fail, and suppuration occurs, the animal is required to be killed." Does that go far enough to meet you?—(*Sir Frederick Banbury.*) If that is correct it would more or less meet our views.

19715. That is the report for last year?—But everybody makes mistakes, even doctors and inspectors sometimes. Our only desire as a society is that if pain ensues the animal should be destroyed, and we do not for a moment say that it is possible to make a state of affairs under which pain can always be stopped. Accidents will occur we all know; but, so far as possible, pain should be stopped.

19716. I wanted rather to separate cutting operations under Certificate B from mere inoculations under Certificate A, and to ascertain the opinion of the society on these two points?—Certificate A is what we desire, so far as we can have it, but Certificate B we object to unless it is absolutely modified in the direction laid down by us. If what you have read is correct, and there is no pain, then we do not object to it.

19717. I am afraid we are not quite understanding each other. So far as I know, great pain may be caused under Certificate A?—But the animal is under anæsthetics, and is destroyed.

19718. No; the quotation from Professor Thane, that Sir William Collins read, referred to Certificate A, and Certificate A deals with inoculation of disease. For instance, very recently what appeared to be very necessary experiments were carried out with plague—that is to say, animals were inoculated with plague for the purpose of testing the mode of transmission of plague. Do you object to that?—If the animal is allowed, after the inoculation has taken place, to remain alive and suffer pain, if that is the question.

19719. It may be necessary for the disease to develop to a certain extent, or death would come on very quickly. I am asking whether you would object to that?—I think we should object to the animal being kept alive when the operator knew that it was suffering pain.

19720. Even though thousands of animals are dying all round from the same disease?—Yes, I think so.

19721. To take another instance. Supposing that some work is undertaken with reference to finding a cure for distemper, which is a very distressing disease to animals, it may be necessary to inoculate an animal with distemper and to try various modes of treatment in order to find out a successful mode of treatment. Must not that animal necessarily be kept alive until the treatment can be applied, and the treatment may fail?—(*Mr. Scott.*) Yes; in the same way that we keep a dog alive, supposing that it is mad, and watch the results. (*Sir Frederick Banbury.*) I have a considerable knowledge of distemper in dogs, and I should myself have said that distemper is not painful. The animal gets very weak, and I have had, unfortunately, dogs die of distemper, but I do not think there is actually any pain. They have been weak and they will not eat their food, and they fall away. I think those are the chief symptoms.

19722. But when you come to legislate in the sense that you propose you admit that there is a good deal of difficulty?—There is no Act of Parliament that has ever been passed—I think your Chairman will bear me out—through which you cannot drive a coach and four. A perfect Act of Parliament will never be passed.

19723. I was rather putting it the other way—that you would pass an Act which would hit all the cases that you did not mean to hit?—There are means of getting round that, I think.

19724. (*To Mr. Scott.*) I was not quite sure from your evidence whether you wanted this present Act of 1876 amended, or whether you wanted further inspection to enforce it. Do you think the Act requires any amendment?—I think personally what we say in our statement: "The Society has, ever since the passing of the Cruelty to Animals Act, 1876, repeatedly pointed out that the supervision of experiments carried on under licences granted under that Act is totally inadequate"—that is our position. We want more inspectors.

19725. Are you aware that the Inspectors continually see animals under experiment, though not the initial experiment?—I can quite believe it.

19726. Have you ever yourself been over any of these laboratories?—No, I have not. I have studied the matter a little in a very small way. I was once a student at the college at Cirencester, and I read privately with a veterinary surgeon, so that I have some little knowledge, but I should not have presumed to mention it except that I thought you asked me whether I had ever had anything to do with it.

19727. Have you ever asked to see over any of these laboratories?—No, I have never done so.

19728. Do not you think it would be a good thing to do so?—We would go with pleasure if some of you gentlemen would do us the honour to ask us.

19729. We have had several witnesses here from laboratories who have said that if any person with a sense of real responsibility and not from mere curiosity desired it they would be perfectly willing to show them round?—We should think is a great privilege to be allowed to go.

19730. But, as a matter of fact, up to date, you have not yourselves asked to see any of these experiments?—No, neither of us.

19731. And you have no specific cases about which your society knows anything, or enough to bring to the attention of the Commission?—I do not think so.

19732. I do not want to go into the case of the brown dog, but I want to put this question to you. Supposing that an animal was experimented upon, and then was allowed to recover for the purpose of some observation—I am putting a hypothetical case—you know that under the law, as it stands, at the end of the observation that animal must be destroyed painlessly?—Yes.

19733. Since that animal has to be destroyed, do you see any objection, after the anæsthetic has been administered to that animal, to its being again the subject of experiment for demonstration purposes. It has to be destroyed in the case I am putting?—(*Sir Frederick Banbury.*) While it is still under the anæsthetic?

19734. Certainly?—That is a very difficult question to answer, because my view (I do not know whether Mr. Scott agrees with me) is that if one were certain that it is possible to keep an animal under an anæsthetic, and provided that the experiment is carried out at once, our point is met. (*Mr. Scott.*) I should say so. (*Sir Frederick Banbury.*) But then the question arises whether it is possible.

19735. That is outside my question. I am assuming that the animal can be kept under an anæsthetic, and does not recover. Do you see any objection?—I do not think so. Our objection is entirely to pain. (*Mr. Scott.*) And it should be destroyed before the effect of the anæsthetic has been removed.

19736. Even though an incidental effect is that it has two operations; that is to say, that it saves two animals being operated upon, instead of one?—(*Sir Frederick Banbury.*) As long as pain does not ensue is all I can say.

19737. (*Dr. Wilson.*) The question, I understand, is this. The animal is operated on for a certain physiological experiment, and is allowed to recover, say, after taking out a portion of the kidney or the pancreas; then after a time, when the results of the experiment have been sufficiently studied, that animal has to be destroyed; it can be destroyed, of course, painlessly, by administering chloroform. Do you think that that animal should be handed over for another experiment?—No, decidedly not. I did not understand the question. I thought that the case put to me was that of an animal which is brought into the room and put under anæsthetics, an operation takes place, and then another operation takes place immediately. To that I should not object, because I presume that pain does not ensue; but I do object to an animal being operated upon and being allowed to recover, and then being brought forward again and operated upon. I think we both agree as to that. (*Mr. Scott.*) Yes.

19738. (*Sir Mackenzie Chalmers.*) I am not quite sure now whether we have got the point. The animal has been operated upon, and is allowed to recover. That animal has to be killed, and has to be killed painlessly. Do you object to that animal being put under

chloroform before death occurs, and another operation being performed upon it?—Yes, certainly.

19739. You would rather that a fresh animal was sacrificed?—Certainly.

19740. On what ground, if the animal suffers no pain?—On this ground. My contention is that the animal ought to have been killed after the first operation, and that if that sort of thing were allowed as it is difficult always to get animals, the tendency would be to keep one wretched animal for many months in that sort of state, and your argument would always hold good, because you have broken the first contention—that after an operation the animal should be killed, and you might multiply it indefinitely.

19741. Then your objection is not to an animal being experimented upon when it has to be killed, but to the animal being kept alive after the first operation?—I object to the animal being operated upon if pain ensues, except under anæsthetics. But I object to the animal being experimented on and kept alive, and then experimented on again and kept alive again, and experimented on again, and so on.

19742. That is not the question I put to you?—But it might result.

19743. (*Mr. Ram.*) I want to ask you a few questions, if I may, because I am very anxious to get the views of your society, which we value greatly on certain points. First of all, with regard to your definition of cruelty under your Act, I want to clear up that point, if I can. I take it that by cruelty you mean—you will tell me whether I am right—the infliction of unnecessary pain?—Yes.

19744. Not necessarily the infliction of any pain?—No, the infliction of unnecessary pain. (*Sir Frederick Banbury.*) I do not admit that.

19745. Then I must address myself to you gentlemen individually, and I will take Mr. Scott first, if I may. (*To Mr. Scott.*) I put the question, do you by cruelty mean the infliction of any pain in any circumstances on any animal, or do you limit it to unnecessary pain?—The wording is "Cruelly ill-treating, abusing, or torturing animals."

19746. I want to get at a definition of cruelty, if I can. Let me put an instance to you. A cab horse is drawing a cab, it may be a lazy horse, or it may not, at any rate, that horse is every now and again struck by the cabman with a whip, not excessively, but probably every time it is struck the horse feels some pain, and without that pain it would not go at all. Then there is a second case, in which the horse is made to draw an excessive load, or is whipped with barbarity and cruelty to make it do that which it is very painful for it to do. Do you view those two cases as coming exactly under the same definition of cruelty, or do you draw any distinction between them?—I draw a distinction between them.

19747. The first case would not, in your opinion, be a case of cruelty?—No.

19748. The first case is simply whipping the horse, perhaps a lazy horse, enough to make it do a task which is within its power?—Yes.

19749. You would not call that cruelty?—No.

19750. That is, however, the infliction of pain upon the animal?—But it is necessary pain, like when one used to get the cane at school.

19751. Yes, we had necessary and very beneficial pain?—Exactly.

19752. Then I take it that you at any rate will agree with the definition that I suggested of cruelty, namely, the infliction of unnecessary pain?—I think so.

19753. (*To Sir Frederick Banbury.*) Might I ask why you do not agree with that definition?—Because it depends upon the extent of the cruelty.

19754. Shall we say rather the extent of pain?—Yes. I beg your pardon. I should not admit that a cabman had a right if he has a lazy horse to thrash it severely in order to make it go. If you ride a horse at a fence sometimes you may get a horse that will not jump, and I think you are justified in inflicting a certain amount of pain by using spurs, but I do not think that you have a right to have five or six fellows with hunting crops, slashing and hitting at the horse, although the result that you may desire may ensue. I maintain that you must do it by gentle means.

19755. Perhaps I may get you to take my view if I suggest the addition of one word to my definition of cruelty and say that it means the infliction of unnecessary and excessive pain?—I should say excessive pain and leave out the "unnecessary."

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19756. (*To Mr. Scott.*) Has this Act of 1876, which we are dealing with now, ever been pronounced upon as an Act by your society?—We have it in that book of ours as one of the Prevention of Cruelty to Animals Acts. (*Sir Frederick Banbury.*) You mean, have we ever given an opinion upon it?

19757. Yes?—I do not think that we have.

19578. (*To Mr. Scott.*) I understand, I think, that neither of you gentlemen has read the Act?—Only portions of it. I was reading our book only this morning. (*Sir Frederick Banbury.*) I have read the Act, but it is some little time since I read it, and I could not stand a cross-examination upon it if you have the Act there.

19759. I am not going to attempt to administer one. (*To Mr. Scott.*) I think I gathered from your answer to Sir Mackenzie Chalmers that you would not abolish the present Act?—I do not think so.

19760. Your society would not wish to abolish it?—I do not think that they have ever considered it. I do not think they have any reason for wishing it.

19761. You have suggested that the Act should be amended by increasing the number and perhaps making some new regulation with regard to the Inspectors?—Yes.

19762. Can you help the Commission at all by making any other suggestion as to the way in which, in the opinion of your society, the Act might be amended, or may I take it that that is the only one matter in which you think it requires amendment?—That is the only one that occurs to me, at all events.

19763. (*To Sir Frederick Banbury.*) I think you began your evidence by saying that throughout the claim of your society has been that animals should be given by man the same protection as regards the infliction of pain as man asks for himself?—Yes, I said so.

19764. That is to say with regard to anæsthetics and with regard to aseptic treatment in case of recovery from wounds?—Yes.

19765. Do you know that that is the protection which is now given to animals under the Act?—Yes, the Act provides that.

19766. With regard to anæsthetics, the animals have the pull over mankind in this respect, have they not, that whatever the subsequent pain of an operation may be to a man he can only be given such an amount of anæsthesia as implies his subsequent recovery, though it may be accompanied by much suffering?—I suppose so.

19767. In all cases of operations upon men it is intended that they should recover, however much they may suffer subsequently?—Yes.

19768. With regard to experiments on animals in a vast number of cases, at any rate where the experiment is a painful one, the animals are under an anæsthetic from which they never recover. To the extent to which that takes place your society, I gather, has no objection to such experiments?—No.

19769. Again, with regard to the advantage that an animal would have, if an animal is operated on under anæsthetics and is allowed to recover and is treated aseptically or antiseptically, should it be found that the treatment is insufficient or that it does not effect its object—in other words, if the animal suffers, that animal can be, and in the vast majority of cases would be, destroyed at once?—Yes.

19770. That is not so with regard to man. Man, of course, when he recovers from the effects of the anæsthetic must suffer pain, whatever pain it is necessary he should suffer in the course of recovery?—Yes.

19771. Now, with regard to inspection, I am anxious to have your views. So far as the animal is operated upon and never allowed to recover you have already told me that your society would not object, and the presence of the Inspector has no particular effect one way or the other there; it ensures that the animal is under anæsthetics and is destroyed before it recovers?

19772. What I want to put to you as a matter which strikes me as being much more important is this:

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Would it meet the views of your society if, in cases where the animals are allowed to recover, there should be an enactment that those animals should be visited by the Inspector, say, shortly after the operation, and again at a short period, in order to see whether those animals that were allowed to come out from a state of anæsthesia were in a state of suffering or not?—I do not think it would. That is a question to which I would rather not give an answer at such short notice, but I do not think it would. I can only repeat our statement, which I think is clear, that when pain ensues then the animal must be destroyed.

19773. I am sure you recognise that I am anxious to have your assistance so that we may get something workable?—Certainly.

19774. Sir Mackenzie Chalmers has pointed out to you, and I think you both accepted it as I should expect, the difficulty of acting stiffly with regard to the Inspector seeing every experiment, viz., that you will either stop everything or you must have an army of inspectors; therefore, I want to see whether you can help us to make it workable. Would it *pro tanto* meet the views of your society if it were enacted that all animals which by reason of coming out of anæsthesia may be liable to suffering, should be inspected by the Inspector at a certain period shortly after recovery and again after a short interval?—(*Mr. Scott.*) It would be a step decidedly. I should not like to commit the society with regard to that; it is a very important question. It seems a very reasonable suggestion indeed, but, speaking for the council, I do not think I could commit them without further consideration.

19775. I think there is only one other matter that I need trouble you with. I am not going into the case of the brown dog, but are you aware that the brown dog was the subject of the action of *Bayliss v. Coleridge*?—Yes.

19776. Have you read the report of that case?—Yes.

19777. And you know what was stated in that case. It was tried before the Lord Chief Justice and a special jury at great length and the jury found for the plaintiff for £2,000?—Yes.

19778. (*To Sir Frederick Banbury.*) In your statement, you say that all severely painful experiments should only be performed in a particular place or places, so as to facilitate inspection. Are you aware that with regard to all experiments for scientific purposes, whether painful or not, under the Act they can only be performed in a particular place or places?—I think our idea there is that there are too many particular places licensed, and that we would rather restrict the number of places licensed or restrict the area, so that inspection may be easy.

19779. (*Dr. Gaskell.*) You mean that you would restrict it simply for the purpose of making inspection easy?—Yes.

19780. Not because you think there are any signs, in consequence of the increased number of places, of violation of the Act?—No, we do not know anything about that. (*Mr. Scott.*) It is not for that reason.

19781. It is to facilitate inspection?—Yes.

19782. But you would not advocate that if you thought that by so doing you were going to hamper research?—(*Sir Frederick Banbury.*) I do not think that our society takes the line that we are to sacrifice everything to the advance of research. It is a very difficult question for us to answer, but, personally, I do not think that we should hamper science. We might perhaps inflict a little more trouble upon the scientific gentlemen, but we should not object to that. (*Mr. Scott.*) No, we should not.

19783. (*Dr. Gaskell.*) (*To Mr. Scott.*) I think you told us that you looked upon it as a noble and proper thing for experiments to be made by means of inoculation for the purpose of curing diseases; you consider that a thing which ought to be done?—Yes.

19784. And therefore the people who do that, you believe, should be recognised as benefactors to humanity?—Yes, I think so.

19785. Would you say the same if it was a question of inoculation, not for the purpose of curing an actual disease, but for advancing knowledge; that if the experiment was for advancing knowledge it would still be a noble and proper thing to do?—Yes, I think so.

19786. Therefore, you would also say that men who performed such inoculation experiments are actuated by high principles of humanity for the purpose of helping on the human race and the animal race?—I think we may go so far as that. I do not wish to impute anything but proper motives.

19787. The experimenters in physiological laboratories, you will agree with me, are actuated by high motives?—I would say, probably. I would not for a moment suggest that they were not. That would be a reflection upon a body of gentlemen which I have no wish to make.

19788. The object of their experimentation would always be to get the best results, would it not?—I think that is the assumption.

19789. Therefore, if the results were better obtained by the animal being kept quiet under an anæsthetic, the tendency would be to take care that the anæsthetic was thoroughly given, for their own sake and for the sake of the experimentation?—Yes.

19790. And also in the other cases you would agree, would you not, that when the animal is kept alive after the operation it would be for the benefit of the experiment that that animal should be well cared for, should be well looked after, and should be kept free from suppuration?—Yes.

19791. So that the object of the experiment necessitates in the minds of these high-minded people the absence of pain as far as possible?—Yes.

19792. Then what I want to understand is in what way you consider the present inspection deficient. I am not concerned with inoculations here—we have already talked about them—but in a physiological laboratory there are two kinds of experiments. The first is an experiment under a licence, in which the animal is anæsthetised and is killed before it recovers from the anæsthetic. If that is properly administered there would be no pain there?—No.

19793. Therefore, if you have evidence, or if you can believe, that the anæsthetic is properly administered, you would not require a large increase of inspectors for that purpose, would you?—No.

19794. The second class of experiments consists in an operation for the removal of an organ, or the removal of a nerve, say, with the purpose of keeping an animal alive in order to see what happens to its economy in consequence of that removal?—Yes.

19795. Those are operations under Certificate B?—Yes.

19796. The first part of such an operation is perfectly painless, because the animal is anæsthetised. The only place where pain may come in afterwards is when the animal has recovered in the laboratory; there it is possible for some pain to occur?—Yes.

19797. Do I correctly understand that it is for that particular part of the work done in physiological experiments that you would require more inspectors?—Yes, we should.

19798. Then, may I ask, do you know what the Inspector at present does; do you know what the present Inspector does?—No; I know what he is supposed to do. I understand that he has to be present at these experiments, but I do not see how it is possible that he can be.

19799. But the experiment in question, which is the one for which you have just allowed that you want more inspectors, is not a cutting operation, but it is followed possibly by pain in the animal which has recovered from the first operation, but is in, so to speak, the ward of the hospital—that is, it is in the laboratory, and is alive?—Yes.

19800. That animal can be inspected, can it not, by any inspector who comes at any time, because the experiment, if you call it so, is going on all the time, is it not?—Yes.

19801. Then I want to know when the present Inspector comes now into a laboratory, does he not see all those animals which have been operated upon under Certificate B, and which are in the laboratory at the time?—I really do not know.

19802. But the present Inspector has told us that he does; it is one of the main duties of the Inspector. Does he not go round and see the animal house, and see how the animals are kept, and see every animal

that has been operated upon?—That is what I think he should do, but I did not think that there were anything like enough men to do it.

19803. But it only requires one man?—Then I have misunderstood some previous question. It would only require one man for that particular place, but if this experimentation is going on in many places you want many men.

19804. No, pardon me; supposing that an experimenter makes an experiment on a cat to-day, and makes an experiment on another cat to-morrow, and makes an experiment on a third cat the next day, and each of those cats is kept alive, if the Inspector comes on the fourth day, he can see all those three cats?—That is perfectly right if he does.

19805. But he does, does he not?—I understood a Commissioner at the further end of the table to say that if you had inspectors to inspect these matters we should have such an army of them that it would be a very great expense.

19806. I want to draw a distinction, if I can, between animals which are being operated upon and animals which are kept in the laboratory after an operation. All those animals which are kept in a laboratory after an operation can be seen, and very often are, by one inspector at one moment, can they not?—Yes, I should say so.

19807. And he comes at unknown times and sees all these animals?—Yes.

19808. Is there any advantage in having another inspector coming on another day to see those same animals?—No. If that man can see those animals, as you say, that is what we want.

19809. That is exactly what is done now, do you not understand?—I understand what you say, but I understood that it was impossible, that there are not enough inspectors for the work that is done. If you say that there are enough inspectors, that shakes me very much, because I read of so many experiments and I cannot understand how the three Inspectors that there are can look after them all.

19810. I will put the question in this way: A surgeon in a hospital has two duties, has he not; one is to operate upon the patients, and the other is to go into the wards and inspect those patients upon whom he has operated?—Yes, he does that every day.

19811. And he does that, as you say, every day. The Inspector comes into a laboratory where he sees the operation that is being performed; that is one duty?—Yes.

19812. He also goes round the wards and sees all the animals which have been operated upon?—Yes, that is right.

19813. Those are two duties?—Yes.

19814. You yourself say that the latter animals are those in which pain is likely to exist?—Yes.

19815. So that those animals in which pain is most likely to exist he sees most thoroughly whenever he comes to the laboratory, do you understand?—Yes.

19816. He is like a surgeon going into the wards; he sees all the animals in the wards and inspects every one of them with the same care that a surgeon would inspect his patients with?—Yes, I quite follow you.

19817. Is not that inspection in that respect efficient inspection?—I should say that it was very good inspection.

19818. I want to understand from you why it is necessary to have an army of inspectors to do that. If a man comes down at any moment into a laboratory just as a surgeon comes to the ward of a hospital at any moment, and inspects all the patients there, do you think he is likely to find any great difference if he comes another day?—If that is done that is what we mean—if you can say that it is done.

19819. That is exactly the point. I thought you were under a misconception?—You said every day, did you not?

19820. No, I said if he comes down and inspects on one day, is there any reason why he should find anything different on another day. If you were to go into the ward of a hospital——?—Which I have often done.

19821. For the purpose of judging how that hospital is getting on, and you went round the surgical wards one day, would you consider it necessary, or that there would be any point in it, to go round those surgical wards again the next day and the day after?—You are putting to me the case of a surgeon going round.

19822. No, I say supposing you as the Inspector went round?—As you say, I should pay these chance visits, but I cannot see how these men can do it when there are such a number of places and so many experiments.

19823. What I mean is this: in each laboratory there are at any one moment of time a number of animals which have been operated upon, and when the Inspector pays his chance visits he investigates the condition of all these animals?—That is what we wish.

19824. But he does?—If you say so I admit it.

19825. The Inspectors have told us so themselves, as you know if you have read their evidence?—Yes, they have told you so.

19826. What I do not see is how you, or anybody else, would get more advantage than that by increasing the number of Inspectors if the Inspector comes a certain number of times by chance and sees all these animals?—If he comes often enough.

19827. Why do you say that?—Because fresh animals come in and go out.

19828. Why should there be any difference in the arrangements of the animals and the nature of the animals from one time to another?—From Monday to Wednesday, do you mean?

19829. No, I mean at any one moment of time there may be six animals which have been operated upon in the laboratory?—I do not quite follow what you mean. Do you mean that a man might come on Monday and again on Wednesday or on Friday or at an interval of a month?

19830. I mean at any time you like, whenever he chooses, casually. Why should you suppose that there should be any difference in the condition of things in the laboratory from any one day to any other day?—Only that there may be fresh patients.

19831. So there are in a hospital?—Yes, and there, of course, the surgeon visits very frequently. That is what I mean, whether the Inspectors make these visits frequently.

19832. Another point is that the animals should be well looked after?—Yes.

19833. Would you not again consider it sufficient if the Inspector on a chance visit does tell us that the animals are well looked after?—Yes, that would be a very great thing.

19834. That would be sufficient?—It would go a long way towards efficiency.

19835. You see, it is to aid the experimenter that the animals should be well looked after, because otherwise, the experiment would fail?—Yes, I quite see that.

19836. It is his interest to look after that?—Yes.

19837. Then there is one other point I want to put to you. At the present time the law says that the animals must be kept in a licensed place, and those licensed places are, as a rule, in towns. Do you not think it would be better that the wards of hospitals for animals after they have been operated upon should be out in the country so that they should have every opportunity of the best air and the best food?—Yes.

19838. Would it not be better, instead of their being obliged to be kept in licensed laboratories in towns, where they are now, that they should be free to go out into the country?—It seems very reasonable, indeed.

19839. At present the law does not allow that, as you know?—I think it would be a very good thing. It is hardly, perhaps, a proper illustration, but I may say that in the case of our dogs' home we started a country branch, but unfortunately we cannot make it answer at all. People do not seem to care to go to the country. In addition to our home at Battersea we took a place at Hackbridge, because it is much better for the dogs, but it has not been a success. As I say, I do not know whether I ought to have mentioned that, but it seems the same kind of thing. And

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your suggestion would seem a very reasonable and right thing, in the same way as we have convalescent homes.

19840. (*Dr. Wilson.*) I gathered from what Sir Frederick Banbury stated that objection is taken by your society to experiments on animals being carried out for teaching purposes in the presence of students. Do your society take up that position?—Yes, they do very much.

19841. They object to all experiments for teaching purposes?—There is an objection on the part, I think, of a good many members on that ground.

The Hon. STEPHEN COLERIDGE recalled; and further Examined.

The Hon. S. Coleridge.

19842. (*Chairman.*) I understand that you wish to say something about what has taken place in reference to Professor Gotch's evidence?—Yes, but also the night before last I had placed in my hands Sir Victor Horsley's evidence, and with your kind permission I should certainly like to say a few words on the subjects that he has dealt with, not at all with respect to any things which the Commission may think are answers to anything that I said, but I wish to deal with the accusations of a personal nature made against me as a man and a gentleman, which I think certainly need my reply as they will be published.

19843. In that case before we allow that, I think you must tell us precisely what the points of fact are, because we could not hear anything in the nature of a speech in reply to Sir Victor Horsley?—No, of course not.

19844. But if Sir Victor Horsley has made a statement of fact relating to you personally which is untrue, or which requires explanation, that explanation, confining it to the matter of fact, we might think you entitled to give, but it must be confined to that, because this enquiry is a very long one; you have yourself given evidence for three days, and in the course of that evidence you have felt it your duty to attack a good many people?—I never said a word against Sir Victor Horsley.

19845. I am not speaking of that. I am only saying that we could not have everybody who considered that an attack had been made upon him called back again to give evidence, because something must be left to us—when we have heard both sides—to say whether we think it is of importance, and what importance we attach to the statements on each side; and it is only when there is a clear misstatement of fact which is capable of correction relating to yourself that we ought to allow the matter to be gone into. Otherwise many witnesses would say, "I wish to make a reply on certain branches of the case that Sir Victor Horsley touched upon." We must avoid anything of that kind?—Well, my Lord, I have been accused by him of "deception," "constructive misstatement" (Q. 15728), "gross misrepresentation" (Q. 15905), and "dishonesty" (Q. 16052), while my methods of controversy, it is insinuated, are ungentlemanly (Q. 15729, 16011), and have been subjected to "exposure" (Q. 15978). Those I took out as things which he said against me personally. They involve material of this kind. They involve the question of my *bonâ fides* about the publication of the catalogue of instruments for vivisection, upon which I wish to defend myself of course; they involve a statement that I made a perfectly false statement to you about the meeting at the Mansion House for the purpose of instituting a Pasteur Institute in England, a statement on the subject accompanied with considerable personal imputations.

19846. You do not complain of that, Mr. Coleridge, do you? Many witnesses here, yourself included, have not hesitated, when they thought that a thing deserved strong language, to use it?—Certainly.

19847. Therefore, it is a question of fact, and not of a comment on the fact?—Yes. He is wrong in his facts, and I am prepared to show that he is wrong in his facts. Then also he is wrong on the question of muzzling. I can show you that my Society has never said a word against muzzling; on the contrary, I have always regarded the muzzle as the opposite treatment to Pasteurism, and have supported it. I supported Mr. Long in his muzzling order, so that Sir Victor Horsley is utterly wrong, and I can show it.

19848. He speaks of your Society?—Yes, he says that we actually got up meetings against the muzzle. It is absolutely false, we did nothing of the kind. He has mistaken our society for another. I do not attribute anything more than that.

19849. But we had a witness only the other day who spoke very strongly against muzzling?—Yes, from the Canine Defence League.

19850. Who said that his Society originated the opposition to the muzzle?—Yes, that is so. Sir Victor Horsley has mistaken us for the Canine Defence League.

19851. I am only pointing out there that it would be very much shorter for you to say, "This is quite a mistake. Our Society has always been in favour of muzzling, and it is evidently a mistake for the Canine Defence League?—Yes, that is all I want to say. I have got eight headings, and they will not take very long; they are all quite short and all of that nature.

19852. We will deal first with Professor Gotch's evidence, and as to that we could not hear you to discuss the value of the whole of the evidence; it is only if there is something about yourself personally that we can hear you?—That is all I wish. I do not wish to prolong the thing at all. I am very reluctant to come back again. The first matter is at Question 13668. You, my Lord, said: "Some questions were asked as to what Mr. Coleridge knew, and of course he said he knew nothing about it personally, but that these gentlemen had given him this information, and he had no doubt that they were speaking the truth." I do not think that I ever said I had no doubt that they were speaking the truth.

19853. You did not use those words, but that was no attack upon you. I was not in saying that making anything in the nature of an attack upon you at all?—No, I am only raising it because as a matter of fact I never said it.

19854. But you said something rather like it?—No, my Lord, I did not.

19855. You said something to the effect that they were credible persons?—I sent their statement down to Dr. Gotch, that is what I did with it.

19856. I should have thought that that was not a matter which you need have brought forward?—I had some doubt because I sent it down to Dr. Gotch. If I had had no doubt I should not have done that. I did not accept their statement without further explanation. I read out the letters, and the comment upon them begins at Question 11455.

18957. You say at Question 11444 that you received a letter, and you say, "I should like on this to show you the kind of evidence that is constantly brought before me"?—Yes.

19858. Then you go on to say at Question 11446, "I am only showing the sort of evidence that comes before me. I did the obvious thing that a gentleman ought to do. I wrote at once to the head of the laboratory, told him the information that I had received, and asked for his explanation; and he gives the lie to the six gentlemen at once. All I can say is that in a court of justice the six witnesses would go against the one?"—Yes.

19859. That surely is saying that Professor Gotch's evidence is not to be believed as against that of those six gentlemen?—I do not think that.

19860. Surely it was not a very unfair representation for me to put it shortly as I did?—Surely, it is a question of the weight of evidence.

19861. I think we should hardly have recalled you as to that?—Then at Question 13668 you, my Lord, say: "The language probably which he (that is, Mr. Sharpe) did not when he wrote it intend to be read out to the Commission or anybody else"; you are alluding there to Mr. Sharpe's letter to me, in which he says: "Professor Gotch is a liar." When your Lordship drew that inference——

19862. This is really replying. This is not a question of fact. I did not think so, but after that there was

a letter from Mr. Sharpe, who said that he did give you leave, or leave was given to you in a second letter to read it out to the Commission, but he did not think it would go into the Press?—I want to defend myself upon this very strongly, because I sent a letter down to this young gentleman by messenger. I sent him the following letter because I was coming before the Commission in a day or two afterwards: "Dear Sir,—I am forwarding you by special messenger copy of a letter which Mr. Coleridge wrote to Professor Gotch, and his reply. Owing to the nature of his reply, do you think you could send me any fresh information which would explain all the discrepancies between the accounts before Wednesday, as Mr. Coleridge is to give evidence before the Royal Commission on Vivisection there?" So that it was in answer to a deliberate invitation to him to give me more evidence that I might put it before this Commission that he wrote the letter, which I am now vilipended for producing here.

19863. I do not see that you are vilipended. I said what seemed extremely natural from the evidence which I had before me?—Further on I am attacked equally by Dr. Gotch. He says at Question 13700 that he is bound to say he cannot acquit me. Then, further, there is a letter from the Warden of Keble, who actually writes in these terms——

19864. (*Mr. Ram.*) Will you give the reference to it, please?—It is Question 13668, near the bottom: "They" (that is, the undergraduates) "made some attempt to make inquiries, but the attempt was obviously inadequate, and I think that Mr. Coleridge treated them a little badly in accepting and making public the second letter without further investigation on his own part." This young man knew, therefore, that this letter would be read out to the Commission. I asked him for a letter to read here.

19865. (*Chairman.*) Are you complaining of what the Warden wrote?—Yes, I am, very bitterly; most decidedly I am. He cannot have known, I presume, that this young man had an invitation from me to write that letter that I might produce it here or he would not have accused me thereafter of producing it here. What else could I do.

19866. I do not see anything further than that the letter is one which shows that the Warden thought you ought to have made investigations not merely by asking these young men, but by finding out what really happened at the laboratory?—How could I do that? I have no warrant to enter the laboratory.

19867. You had as much power of finding out as they had?—Quite so.

19868. What do you expect us to do? You are now making a speech upon the Warden's letter?—No, my Lord, not a speech.

19869. You are not contradicting any question of fact; you are making observations upon the Warden's letter as not being fair to you?—Quite so; it is not; it is extremely unfair to me. Now I go further, and I will say this: I wish to point out that when these various people announced that these gentlemen had withdrawn and apologised, the fact is that they have never withdrawn and apologised for the only thing that I think is of any value at all in their original letter. Dr. Warren, the Vice-Chancellor, says that he has had placed in his hands "a complete apology by these undergraduates to Professor Gotch, a recognition by them of the correctness of Professor Gotch's statements, an explicit and unqualified withdrawal of the letters and statements sent and made by them to Mr. Coleridge." I say that no such withdrawal has ever been made. They contend, and they have never withdrawn it, that they heard the howls of dogs in agony.

19870. Is not this a speech upon the evidence?—No, my Lord.

19871. Surely it is. Here are the letters before us, and you are saying that those are not an unqualified apology. We have before us the fact; we have what the letters are, and we have the statement which you have just made, that this was an incomplete apology. We must judge of that. We cannot call upon you as counsel to make a speech?—I have got a letter here from Mr. Sharpe, in which he says that he does not apologise.

19872. Has that letter been read here?—No. I have another letter from him to myself.

19873. All I can say is that if Mr. Sharpe writes a letter apologising, and then writes another saying that he has not apologised, we should not pay much attention to anything that he said?—No, he does not say that. He has only withdrawn the statement made to him by the laboratory attendant that such and such things took place in the laboratory. He has not withdrawn that he heard the howls of dogs.

19874. He says that in a letter to Mr. Tomkinson, which was read?—Yes.

19875. If you say that the only thing of importance is whether the dogs howled, although they were not being vivisected——?—That is all I referred to.

19876. We must judge of that?—That is the only evidence that had the slightest weight with me. What the laboratory attendant said to him I do not care.

19877. I venture to think that there are other things in the matter which will have weight with us?—Very well. Now, my Lord, I want to speak about the catalogue.

19878. Which question is that?—This is in the evidence of Sir Victor Horsley at Question 15729, in the middle of the first column, where he says that this catalogue was corrected in 1902. He said that 50,000 copies had been sent out, and then he said: "It was corrected, as I said before, in 1902, and Mr. Coleridge stated to this Commission, at Question 10559, that where he was shown anything which was wrong he liked to correct it. I may say, in parenthesis, that he has never corrected anything which he has been shown repeatedly was incorrect, nor has he ever apologised." My comment upon that is that it is perfectly true that a Dr. Grünbaum, of Liverpool, wrote to me, and said that certain items in the translation of the catalogue were, in his opinion, incorrect, but he said that they were not of any very great moment. I therefore sent his letter on to the gentleman who translated the catalogue for me, and asked him what his view of it was. He wrote back to me that he did not agree with Dr. Grünbaum's suggested corrections. And as the person who translated it for me was appointed by the librarian of the British Museum, at my request, I presumed that he was an impartial man. I had never even seen him, and I did not see why I should correct my catalogue at the instance of a licensed vivisector, but I printed his letter and published it. Therefore I do not see why I should accept that correction. I did what I think anybody would do—I laid the correction before the original translator, who refused to adopt it, and there I left the matter. I do not see how I am blameworthy there at all.

19879. What is the next point?—Then a little further down on this subject, those being the facts, you will find at Question 15978 Sir Victor Horsley speaks of "the exposure of the catalogue of instruments, and Mr. Coleridge's action"; that is the way he speaks of it. All I say is that I did what any gentleman ought to have done, and left undone nothing that a gentleman ought to have done. I do not want to go into the general question, unless you wish it, of the verity of that catalogue, but I can hand in copies of it if anybody likes to see it. I have got 13 copies here.

19880. No; confine yourself, please, to the specific contradiction?—Then the next point is that I stated in one of my opening statements that I prevented a public meeting being held at the Mansion House for founding a Pasteur Institute. With regard to that Sir Victor Horsley, at Question 15594, said: "In the first place, Mr. Coleridge informs the Commission that that meeting was prevented by his society from taking place. It is not so. The meeting took place at the Mansion House," and so forth. Then he says: "It was not called for the purpose of establishing a Pasteur Institute in England; it was called" for other purposes. Therefore he gives me the lie there. My position is this. It was originally intended to be a public meeting, and I and my friends were determined that if it was to be a public meeting we would take care that the opposition was properly represented. Accordingly, I went myself to the Lord Mayor, and told him that if it was a public meeting the Lord Chief Justice of England and Cardinal Manning proposed to speak in opposition to the resolutions, whatever they were, upon which the Lord Mayor said: "It is not a public meeting; it is a private one," and I said, "Very well, if it is a private meeting the Lord Chief Justice and Cardinal Manning can have no part or lot in it. You can have as many

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private persons as you like, and pass any resolutions that you like, and there will be no opposition from anybody." From that moment it was changed from a public meeting to a private meeting. I can prove this by the "Daily News," of July 1st, page 7, which is a perfectly impartial thing. There is the heading—"To-day.",

19881. I am glad to hear of a paper that is impartial upon such questions. I should hardly have classed the "Daily News" as such?—It was only an advertisement of the meeting, and it was 20 years ago, and they were not anti-vivisectionists then.

19882. But does the Lord Mayor take the chair at a private meeting to discuss a question of this sort? I never heard of the Lord Mayor taking the chair at a meeting of this kind, which was private?—That is exactly what it was. That is exactly what we turned it into. We enforced that upon them by my visit to the Lord Mayor.

19883. How was it advertised—as a private meeting?—Yes.

19884. Would you kindly read the advertisement?—This appeared in "The Times": "The Lord Mayor's secretary, replying to a letter from G. Candy and Co." (I think that should be G. Candy, Q.C.), "informs him that at the meeting which is called at the Mansion House on Monday next to discuss the subject of the prevention of hydrophobia, no one will be admitted who has not received an invitation." That proves that it was a ticket meeting.

19885. In that sense I see what you mean. I thought it was not a private meeting?—It was a ticket meeting. I mean that nobody was admitted without a ticket. Then, as I say, in the "Daily News" you have the heading "To-day, Meetings, Proposed London Pasteur Institute—Mansion House, 3."

19886. Is there not a possible explanation? Supposing that somebody gives notice that there will be a very strong hostile party——?—That is what I did.

19887. Which you did. The Lord Mayor would particularly dislike presiding over a meeting at which there were likely to be signs of disturbances?—No doubt.

19888. Therefore nobody would be admitted without a ticket?—That is exactly the history of it. It was turned from a public meeting into a private one, and my statement to you was that there was no public meeting held—that I stopped a public meeting being held, and I did so. Then the second statement that I made was that the meeting was originally intended for the purpose of founding a Pasteur Institute for London, and I say there is the "Daily News," which actually heads the meeting: "Proposed London Pasteur Institute."

19889. You are not going to cite an announcement of a meeting in the "Daily News" on a question so very controversial as being positive proof of what the object was?—No, it is only evidence of my *bonâ fides* in making this statement. And the "Times" had the same.

19890. If I wanted evidence I should not go to a paper that took strong views?—I think it was against us then, but I will not be sure. The "Times" has always been against us.

19891. (*Mr. Ram.*) Is not this really a very minor point?—Yes.

19892. There was a meeting?—Yes.

19893. You have shown that it was a ticket meeting, apparently in consequence of what you said?—Yes.

19894. That was not known to the witness. He thought that there had been a public meeting?—No doubt.

19895. (*Chairman.*) And certain resolutions were passed?—Yes, but Sir Victor Horsley also said that there was no intention of founding a Pasteur Institute. I said that there was originally, until we intervened.

19896. Sir Victor Horsley must be the person who knows best what the object of the meeting was?—I quite agree that when the meeting did assemble they had made up their minds not to push a Pasteur Institute for England, and no such proposal was placed before the meeting.

19897. That is not quite what Sir Victor Horsley said. You had said: "The intention was to hold a meeting the object of which was to start a fund," and so forth; and Sir Victor Horsley said: "The object was not as he states it; it was not called for the purpose of establishing a Pasteur Institute in England, it was called," and then he quotes, "for the purpose of hearing statements from Sir James Paget and other representatives of scientific and medical opinion with regard to the recent increase of rabies in this country, and as to the efficiency of the treatment discovered by M. Pasteur for the prevention of hydrophobia." That is his version of it?—Yes.

19898. Surely it is a very small point whichever way one considers it. You say that it was meant for the purpose of starting a Pasteur Institute?—No, my Lord, that is not my point. I say that the original intention, until the anti-vivisection party intervened, was to have a public meeting to promote a Pasteur Institute for England, and by our intervention it was changed into a private meeting, with a perfectly harmless resolution, saying that Monsieur Pasteur was a wonderful man, and they would like to subscribe and give him a testimonial, to all of which we had not the slightest objection.

19899. (*Sir William Collins.*) At any rate, was the meeting that was held one to which the public had access?—No, they were excluded.

19900. (*Sir Mackenzie Chalmers.*) It was like all those meetings that we have every day from which suffragettes are excluded?—I have not been to any of those, but I thought they always got in.

19901. (*Chairman.*) I quite understand you. It is quite easy for you to point out that it was not a public meeting in the ordinary sense, but one to which admission was by ticket?—Yes, that is all my point. Then Sir Victor Horsley says that we opposed the muzzle.

19902. What question is that?—It is Question 15597. He says: "Immediately this point of universal muzzling was raised the anti-vivisection party, Mr. Coleridge's society in particular, entered upon a virulent campaign against us," and so forth. I say that he has mistaken us altogether for the Canine Defence League. We did nothing of the kind, and he ought to have been more careful in making these accusations, because he holds a very prominent position, and it is rather careless for a man to come and make that kind of statement without any ground whatever. The slightest inquiry would have shown him that he was mistaking my society for some other with which we have no connection at all.

19903. Has your society never opposed the muzzle?—Never.

19904. Has it never put forward the view that rabies was not a disease?—We are against Pasteurism; we regard the muzzle as an excellent antidote to Pasteurism, and we think we have proved it to be successful.

19905. (*Mr. Ram.*) And to rabies?—Yes, it has kept both Pasteurism and rabies out of the country. We are entirely in favour of universal muzzling. It has nothing to do with any cruelty, and is an admirable method of stamping out an abominable disease. Then the next point is what Sir Victor Horsley says about the Anti-Vivisection Hospital, at Question 15592. He says: "I have placed upon my *précis* a very brief reference to the Anti-Vivisection Hospital, because the money for that hospital was originally collected many years ago, by Mr. Coleridge's society in part, on the idea that medical practice could be conducted without any reference to vivisection." There is nothing unkind in that, and I do not in the least find fault with it. I merely want to point out that it was true from February 7th to October 6th only in the year 1897, and on October 6th, I proposed the following resolution with success to the council of my society: "That inasmuch as the object of this society is to maintain a constant fight against existing vivisection and not to erect institutions from which the practice shall be excluded, the council declare that its committee should not actively participate in the project of founding and maintaining an anti-vivisection hospital." So that really almost as soon as I had any authority with the society at all I passed this resolution, saying that we would have nothing to do with the promotion of an anti-vivisection hospital; therefore it is a little unfair to put it upon us in that way, not that I do not feel an extremely friendly interest in the Anti-Vivisection Hospital, but I did not think that it was

a proper thing for our society to have it actively participating in it.

19906. (*Chairman.*) Sir Victor Horsley only says that you did in part collect the money?—Yes.

19907. And for a time you did?—Yes, for a few months only.

19908. It was hardly worth while making that correction?—I only wanted to put that straight. Then at Question 15738 Sir Victor Horsley said: "The Home Office had not given me, to use Mr. Coleridge's words, unrestricted leave to do experiments when I liked, where I liked, and what I liked." I never used those words in my life, or anything like them.

19909. Yes, I remember he did put those as your own words, and it struck me at the time that before us, at any rate, you did not use those words?—I never did.

19910. You did say that he was allowed powers which he ought not to have been allowed?—I only quoted what I found in the yearly report, which was accurate. I found that he had permission to vivisect where he chose. I also deduced that he could not be inspected under the Act in an unregistered place, and, therefore, if he chose to vivisect in an unregistered place he could not be inspected. I think that statement is absolutely correct, and I am sure that Sir Mackenzie Chalmers will say it is so. It was in the yearly report for seven years, I think.

19911. (*Sir Mackenzie Chalmers.*) You accept Sir Victor Horsley's account of what the restrictions were now that you have read it?—Yes; the restrictions put upon his licence to vivisect where he liked have nothing to do with the point. My point was that he was allowed to vivisect where he could not be inspected.

19912. (*Chairman.*) There is power, you know, under the Act, to allow vivisecting in other places than laboratories?—Yes, I did not say that there was not. I did not say that what was done was illegal. I said that I objected to what was done because it placed him outside the possibility of inspection. That was my objection.

19913. Sir Victor in his evidence gives here a specific explanation of why he was given that certificate. The point is that it was absolutely necessary, if the experiment was to be done at all, that it should be done in a hospital, and by the bedside. That is his explanation?—Yes; I am defending myself from an attack upon me; that is my position. I still say that what I said was perfectly true—namely, that he could vivisect where he chose, and that he could not be inspected. And I further go on to say that what I said was perfectly true about his leave being withdrawn from him just at a time which synchronised with the appointment of this Commission.

19914. I think your suggestion was that he was receiving an extraordinary favour, and that the Home Office was going outside its duties, if not its powers, in doing what it did?—I protested against it, and I had protested two years before in vain, but the appointment of this Royal Commission synchronised with the withdrawal of that permission.

19915. I must say that although I do not think you did use the very words before us which Sir Victor Horsley puts into your mouth, his evidence, to my mind, did very much alter the complexion of the matter as stated by you?—I do not see that, my Lord. I stand to every word that I said, and I have said nothing which is not carried out by the published document from the Home Office. If you look at what I said, it is absolutely carried out in the Home Office document, which I have here, and can show you. I decline to admit for one moment that I said anything which I could not stand to in the letter as well as the spirit. I adhere to every word of it.

19916. Is there any fact which Sir Victor Horsley states about that operation that you say is incorrect?—My Lord, he is accusing me of inaccuracy. I am here defending my accuracy. I am not here to attack Sir Victor Horsley.

19917. You do not want to correct any statement of fact except the fact of your using those words?—It is a matter of indifference to me what was done under those extraordinary provisions. I did not attack Sir Victor Horsley. I attacked the Home Office for giving him permission.

19918. (*Sir William Collins.*) Did you ever say that Sir Victor Horsley had been given power to vivisect when he liked, where he liked, and what he liked?—No; I did not.

19919. (*Chairman.*) What is the next thing?—Here is what I consider an extremely offensive answer at Question 16011. This is with regard to Sir Victor Horsley's evidence at the trial of *Bayliss* v. *Coleridge*. Your Lordship asked him, "Was it your evidence he was quoting from?"—that is, I was quoting from, and Sir Victor Horsley says, "It is my evidence he professes to be quoting from, but from my experience of Mr. Coleridge, and especially after what he has admitted to this Commission, I accept nothing that he produces in the way of evidence of what I have said, unless I have the context. . . . Mr. Coleridge's was quite a false rendering of my evidence in the way he presented it to the Commission." I have brought the evidence here to present it to the Commission, that they may see for themselves that I was absolutely right. I should like first of all to know what he means when he says, "especially after what he has admitted to this Commission." What have I admitted derogatory to my accuracy or to my honour to this Commission. I want to know? I think it is a most offensive phrase. Now, as to the question itself. This is the question and answer, Question 834. "(*Mr. Rufus Isaacs.*) Would it be possible to perform the experiment or the demonstration which we know took place if the dog had been conscious?—No, absolutely impossible unless the dog had been fixed up with all manner of apparatus to absolutely fix every bone. You can fix an animal with apparatus of that sort (*pointing to the operating board*)." That is the evidence I quoted. I quoted it absolutely verbatim, and I really fail to see what he means. If Sir Victor Horsley does not like the evidence I cannot help that. Those are his words.

19920. Are you reading from the shorthand notes of the trial?—Here are the actual shorthand notes of the trial. I consider that I am entitled to quote the evidence given at the trial by Sir Victor Horsley without having this sort of innuendo.

19921. Are those words in the shorthand note—"absolutely fix every bone"?—Yes, there is a split infinitive, I cannot help that; the words are "had been fixed up with all manner of apparatus to absolutely fix every bone."

19922. That is what you were reading from the shorthand note?—Yes.

19923. That is what we have in Sir Victor's evidence?—Yes, you will see that I quote it absolutely verbatim.

19924. He goes on to say, "I showed the Lord Chief Justice what I meant by fixing every bone with a rod passing through the bones. Of course, it is an absurd position, you observe. It is impossible to fix every bone in an animal"?—There is no report of that demonstration to the Lord Chief Justice in this report.

19925. "I said, 'You can fix an animal with apparatus of that sort (*pointing to the operating board*). That is the answer to that—you can fix an animal on that board, and he pointed to it; they had it in Court'"?—That is right.

19926. You quoted that?—"Pointing to the operating board." I then say, I suppose, "That is my answer to that."

19927. Are not those words, "You can so fix an animal," what he says that he said, following on?—"You can so fix an animal with apparatus of that sort (*pointing to the operating board*)"—that is in the report.

19928. Does he go on to say, "That is the answer to that"?—No, those are my words.

19929. Then the inverted commas are put wrong?—Yes, that is what I said to you.

19930. Then it is a mistake in inverted commas being put here as if those words were also part of his evidence?—Yes, that is what I said. That is all I have to say about that. I fail to see any justification for that kind of language. Now I come to Dr. Crile's experiments, and the letters to the surgeons, Question 15712. The question is, "He says, 'We are told in the control experiments, as well as in this, the dog was not under full anæsthesia.' Do you know by whom he is told?" and Sir Victor Horsley says, "No, that is Mr. Coleridge." It is not, it is Dr. Crile. Here is the book; I will read it out of the book. "In the control

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experiments, as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame; after the injection of cocaine he did not. There was apparently blocking of the sensory impulses from the paw." That is the quotation.

19931. What is the page of the book?—In the big book it is pages 118 and 119 in "Surgical Shock." Sir Victor Horsley attributes to me what is found in the book, so that that was a mistake of his.

19932. If I follow you, you say that those precise words at Question 15712 were taken by you from the book?—Yes, absolutely from the book, and the book is here. Then the next point is at Question 15728—this is the most important thing of all. "In 1899 Mr. Coleridge attacked attacked Dr. Crile at the Home Office, and, after he had been fully informed by the Home Office as to the meaning of this term 'complete anæsthesia,' he then wrote to several surgeons and asked them a question about incomplete anæsthesia, and he informed this Commission that he wrote exactly in the terms in which the Home Office had written to him. As a matter of fact, he did not write exactly in the terms of the letter of the Home Office, because he omitted the all-important sentence which defined this term 'incomplete anæsthesia.' And the result was that he deceived these surgeons into thinking that it was suggested that operations were done upon human beings in hospitals without adequate narcosis." If you can follow me I can show you exactly how wrong he is about this. The letter received from the Home Office had these words: "But short of this is the condition termed by Dr. Crile 'incomplete anæsthesia' in which the creature is quite insensible to pain, although the corneal and other reflexes can still be obtained." That statement was a statement given to me by the Home Office as a statement given to them by their advisers, and it contained in it the whole question at issue between me and the Home Office. It stated that the condition of the corneal and other reflexes being obtained was a condition of insensibility to pain. That was the crux that I wished to ascertain; therefore, when I wrote to the surgeons I naturally did not tell them that the Home Office thought that it was a condition of insensibility to pain, but I asked them what their opinion was about the condition of a human being when corneal and other reflexes can still be obtained; and it was in answer to that question that they wrote me those letters. The suggestion that I deceived them is wholly false. I deceived them in no way whatever. I asked them practically whether they agreed with the Home Office in saying that that was a condition in which pain was absent, and their answers were tantamount to saying that they disagreed with the Home Office. That is the whole of that. The only alternative to my mind is that, if it be suggested that my letter deceived the doctors, it must be suggested that I deceived them into telling the truth—that I tricked them into telling the truth, which they would not have told if I had not tricked them, which I think is a strange thing to suggest about the heads of a great profession. I do not make the suggestion. I think their answers to me were perfectly true when they told me that they did not commence operations upon patients in hospitals until the corneal and other reflexes were entirely abolished. I believed them. I believe them now. I do not believe for one moment that any surgeon would commence an operation until the corneal and other reflexes were abolished; and that is what they told me, and there is no deceiving in the matter at all. I asked them a perfectly plain question, and the surgeons gave me a perfectly plain reply. My motive in asking the question and their motive in making the reply have no bearing whatever upon the question. Their answer to the question was either a true answer or an untrue answer. I maintain that it was a true answer, and the allegation of deceit against me is wholly unfounded. I do not think I need say any more, except this:—At Question 15588 Sir Victor Horsley says: "Mr. Coleridge's society, to take that alone, have collected from the public £86,000, and have not added one item of knowledge to us for the relief of suffering or the prevention of disease." We are not a society for the collection of knowledge; we do not spend our money in that way. And let me say in conclusion, before I leave this Commission, that if, as the result of this Commission, inspectors are appointed who will look without protest at live dogs struggling while they are burnt with flames, the certain result will be that I for one, and, I believe, all the humane people of this country, will unite utterly to abolish what we regard as an abominable wickedness.

19933. (*Sir Mackenzie Chalmers.*) There are only two questions that I would like to ask you. I want to understand now, do you accept Professor Gotch's statement to us that during last year no experiments were made on dogs in his laboratory when the undergraduates heard this howling?—Was he there all the time himself? I should want to know whether he was there all the time.

19934. I am only asking you whether you accept his statement?—I accept his statement that, so far as he is aware, no operations were performed there, of course, but unless he is there all the time he does not know what happens in his absence. The students might operate there in his absence.

19935. Do you really suggest to us that any operation was performed there?—I do not suggest anything. You ask me whether I accept a statement of a man as to whether a certain thing is done or not done at a place where he may not have been. I will accept his statement that it did not happen when he was there, certainly; but I am not prepared to accept that he knows what happens in the laboratory when he is not there. No one would accept that.

19936. (*Chairman.*) It is not merely a person who was not there. He is the person who is the head of the laboratory, and there could hardly have been any vivisection of dogs without his knowing it. And, mind you, the statement in the letter is that this happens every day?—The howls of dogs, yes.

19937. And "in consequence of vivisection," the letter goes on to say?—Yes.

19938. Do you mean to say that Professor Gotch is not to be believed?—I have not said that. I carefully do not say that.

19939. You are carefully refraining from saying that he is to be believed?—I am not indeed. I believe his statement so far as he has a right to make it.

19940. Then has not the head of the laboratory a right to say, "There was no vivisection in my laboratory"?—No, no more than a magistrate has the right to say that no crime happens in his district.

19941. If you think those two things are the same?—But many things may happen in a laboratory when he is not there.

19942. (*Sir Mackenzie Chalmers.*) May I put it in this way? Without more hypotheses, are you satisfied yourself that no experiment took place upon a dog in that laboratory during that year?—Sir Mackenzie, you ask me that question. Am I not entitled to ask how Professor Gotch explains the fact that six young gentlemen say that they heard these howls? He does not suggest an explanation. I should have liked him to be asked that. No one asked him how he explained that these six young gentlemen, with no motive of any kind except that of humanity, should have said that they heard day after day the howls of animals in agony.

19943. (*Chairman.*) He does explain that?—How?

19944. Anyhow, he explains it in a sense. He says that the only explanation he can offer is that there were puppies there, and these puppies were very noisy, and that these gentlemen who were so ready to attribute illegal vivisection to him were perhaps a little predisposed to think that they were the howls of dogs in pain.

19945. (*Sir Mackenzie Chalmers.*) I only want to know, is your mind quite relieved on the subject of what happened there or not after having heard Professor Gotch's explanation?—No, it is not quite relieved. I am quite ready to accept Professor Gotch's statement that, so far as he knows, no vivisection has taken place there, and I am sure that he did not do it himself; but the explanation of the puppies I do not think sounds to me a very adequate explanation. These young gentlemen say that they know well enough what the cries of puppies are, and they say that these were not the same; and it is half a dozen young men, with no other motive, so far as I know, except that of humanity.

19946. (*Dr. Wilson.*) But they apologised?—No, they have never apologised for saying that they heard

the howls of dogs. They maintain still that they did hear the howls of dogs, and they have not withdrawn it; they decline to withdraw it.

19947. (*Sir Mackenzie Chalmers.*) Do they decline to withdraw the statement that the howls of dogs which they heard were the cries of dogs in pain?—Yes, absolutely; they still say that they heard the howls and they still think that they were the howls of dogs in agony. What they have withdrawn is that they had been told by a certain official that vivisection had been going on. This certain official says that he did not say so, and they accept that.

19948. (*Mr. Ram.*) At what date did you first learn that they had made the withdrawal which they have in fact made; was it before you gave evidence here or afterwards?—The whole thing was afterwards—long after I was here, of course.

19949. (*Chairman.*) The first letter in the Oxford paper was in October?—Yes. I had no withdrawal when I came here.

19950. And your evidence was given in June or July?—Yes. I had heard no whisper of withdrawal when I was before you.

19951. (*Mr. Ram.*) Did you receive a letter from these young gentlemen in which they did make such withdrawal as they did make; because I see they state that they sent you a letter?—Yes, they told me that they had to withdraw.

19952. "An explicit and unqualified withdrawal of the letters and statements sent and made by them to Mr. Coleridge." The date of that is October 19th, 1907?—I have the letter here, because I wrote at once in reply—I want to point that out—and said, "Am I to understand from you that you withdraw the fact and you now say that you did not hear the howls of these dogs?" and they wrote back, "Oh, dear me, no; we do not withdraw that."

19953. What they did withdraw was this; it is in a letter to Mr. Tomkinson of the 19th of October, in which one undergraduate writes, "I am sorry to say I have no more evidence to give. I have found that on closer investigation the facts which were told me as true—viz., that a dog and rabbits had been cut up, and had been yelling—were entirely false. I felt therefore compelled to withdraw these statements, and to apologise to Professor Gotch for my language concerning him when I heard he denied this." That is what they withdrew?—That is all they ever did withdraw.

19954. Since you received that letter in which they withdrew that statement have you published anything which has incorporated any facts concerning this undergraduate case either in your evidence or otherwise?—I do not think so. I have not dealt with it at all until I came here.

19955. I think in one of the papers which some one has been good enough to send to me there is an intimation that the evidence which you have given before us here was either published or about to be published?—Yes, it has been published by the Commission.

19956. But I mean otherwise published?—I have reprinted in a large type for our own use just a facsimile, and we have had an index put to it.

19957. Has that been circulated?—Not sold.

19958. (*Sir Mackenzie Chalmers.*) How many copies have been circulated?—Oh, a lot.

19959. Some thousands?—Yes, it has been circulated widely.

19960. Will it be circulated further?—Yes, I presume so. We have got about 200 or 300 copies left.

19961. (*Mr. Ram.*) Is it your intention with regard to either a further circulation of this particular book or the further circulation of your evidence, to give the withdrawal as given in this letter to Mr. Tomkinson of the 19th of October?—I propose to incorporate my evidence of to-day with it.

19962. And the evidence of to-day will, therefore, incorporate what I have just put to you as to the withdrawal which these gentlemen have made?—Yes, every bit of it.

19963. (*Chairman.*) But you are not proposing to incorporate in your book the whole of Professor Gotch's evidence on this point?—No, I shall not do that. At least I might do it. I had not really considered that.

19964. (*Sir Mackenzie Chalmers.*) Would it not be rather more fair to include Professor Gotch's evidence?—I only want to do what is fair. I will consider whether I will not incorporate everything that has to do with it.

19965. (*Chairman.*) It should be made sufficiently clear that the statement is contradicted by Professor Gotch and withdrawn to the extent that it is withdrawn by these gentlemen and in their own words?—Yes. My Lord, if it would give you any satisfaction I should be very pleased to submit what I propose to incorporate to the Commission, so that they may see that it is fair.

19966. I think I will not take that responsibility upon myself, but I really think it is a responsibility that you ought to undertake yourself, not only as being fair to Professor Gotch but because I really doubt whether you might not be made answerable at law for publishing anything without it?—Now that you mention it, I will consider whether it would not be proper to put in what Sir Victor Horsley said against me before I put in my reply to him.

19967. (*Mr. Ram.*) One question, please, with regard to the matter which you have just dealt with as to Dr. Crile's experiments and the question as to whether they were under incomplete anæsthesia or otherwise. What was put against you was this: "Mr. Coleridge attacked Dr. Crile at the Home Office and after he had been fully informed by the Home Office as to the meaning of this term 'incomplete anæsthesia.' He then wrote to several surgeons and asked them a question about incomplete anæsthesia, and he informed this Commission that he wrote exactly in the terms in which the Home Office had written to him. As a matter of fact, he did not write exactly in the terms of the letter of the Home Office, because he omitted the all-important sentence which defined this term 'incomplete anæsthesia.'" The question I want to ask you is, did you receive a letter from the Home Office in which there was a sentence defining the term "incomplete anæsthesia"?—Yes, I did. "One of the last of these reflex movements to disappear is the blinking of the eyelids when the surface of the eyeball is touched, then called the 'corneal' or 'conjunctival reflex.' But short of this is the condition termed by Dr. Crile 'incomplete anæsthesia,' in which the creature is quite insensible to pain, although the corneal and other reflexes can still be obtained."

19968. That is in the letter from the Home Office?—Yes.

19969. Did you in writing to the surgeons omit any sentence of that letter?—Yes, I left out that sentence that that was a condition in which they were insensible to pain. I did not call that a definition.

19970. Then the allegation that you omitted a sentence of that letter, which sentence defined the term "incomplete anæsthesia," is correct?—No, I do not say so. I do not consider that that is part of the definition. That is an assertion that an animal when its corneal and other reflexes are not abolished is insensible to pain. I do not agree to that assertion, and I wrote to the surgeons for the purpose of ascertaining whether it was true or false.

19971. May we take it that a sentence in that letter was left out in your letter to the surgeons?—Yes.

19972. And it was a sentence which Sir Victor Horsley considered did define the term "incomplete anæsthesia"?—Yes, I quite admit that Sir Victor Horsley said that it defined it.

19973. And that sentence which he said defined the term "incomplete anæsthesia" did appear in the letter of the Home Office, but was not quoted by you in the letter which you sent on to those surgeons?—Yes. Those words were left out certainly, because they were the issue between us. I never said in the letter to the surgeons that I was quoting the whole of the letter from the Home Office. I asked them one definite point, did they perform operations on patients when the conjunctival reflex was still obtainable.

19974. I was only dealing, of course, with the allegation?—Quite so.

19975. Then there is only one last matter I should like to mention. You have been good enough to send me a letter, dated the 24th of October, in reference to your qualification of the word "pain" in your previous evidence. In reply I wrote to you, and said that I would take care that that letter should come before the Commission, and that your correction should appear?—That is so.

19976. I think, perhaps, the best plan will be, as we have the advantage of your being here to-day, to give

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that correction now, so that it may appear on the notes as part of your evidence?—If you please. Will you read my letter then?

19977. Yes. Then the letter will appear on the notes, and you will say whether that is your evidence. You say: "Dear Mr. Ram,—On reading over my evidence and considering it with due care, I cannot but feel that it would be unwise to qualify the word 'pain' with any adjectives in my Bill. I write to you to say this because under examination by you, at Questions 11265, 11266, and 11267 I said I would agree to add the word 'serious.' This, I think, was a somewhat hasty concession. The addition of the word 'serious' or 'severe' before the word 'pain' would, I sincerely fear, lead to the legal permission of pain to an amount I could never think morally justifiable.—Very faithfully yours, Stephen Coleridge"?—I am very much obliged to you for putting that letter on the notes.

19978. That is the correction that you wish to make?—If you please.

19979. (*Dr. Gaskell.*) With respect to Question 15712, I should like to get clear what the words of Dr. Crile are. You say: "We are told in the control experiments, as well as in this, that the dog was not under full anæsthesia"?—Yes.

19980. I have not got in my own mind what Dr. Crile's words were. It is in Experiment CXXXIII.?—These are the words: "In the control experiments, as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame; after the injection of cocaine he did not. There was apparently blocking of the sensory impulses from the paw."

19981. That is all right. I wanted to know where those words actually occurred?—Yes, they are in the book.

19982. (*Chairman.*) Of course, Mr. Coleridge, when I suggested that it would be advisable to put some statement as to Professor Gotch's evidence in whatever you intend to publish you will understand that I was not myself suggesting at all the publication of any evidence taken before the Commission?—Quite so.

19983. I say that simply because it is not a matter for the Commission; it is a matter, as you know, for the Stationery Office?—Certainly. We have only done it because the official report is in such small type, and we wrote to the Stationery Office about it.

Note.—Mr. Coleridge subsequently requested the Commission to allow the following extract from an article by Mr. Edwin Wooton * in the Dublin Journal of Medical Science for 1885 (Vol. 80, p. 290) to be added here, as he had intended to call the attention of the Commission to it while dealing with the question of the attitude of some vivisectors. The extract is as follows: "I, some time since, began the study of experimental phenomena in the brute world in relation with tuberculosis and other major diseases, and have continued it until the time of writing. It is a pursuit full of difficulties—legal, social, and scientific. The investigator is hampered by absurd anti-vivisection laws, to evade and defy which is his simple duty; and he is annoyed by the sentimentalism of weak-minded neighbours or acquaintances who may become aware that he is engaged in research, and who regard him as a nineteenth century 'six hundred three score and six.'"

FORTY-FOURTH DAY.

Tuesday, 17th December, 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. G. WILSON, M.D., LL.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. D. J. HAMILTON, called in; and Examined.

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19984. (*Chairman.*) You are Professor of Pathology at the University of Aberdeen?—Yes.

19985. And you have been nominated by the University of Aberdeen to give evidence before us?—Yes.

19986. I believe the University of Aberdeen, like some other universities, have not been desirous to give evidence on the general question, after having read the evidence which has been given by other physiologists?—Quite so; they deem that sufficient evidence has been given on that line already, and that there is not any further necessity for it.

19987. And they only now desire you to come forward with a view to your giving evidence about some special inquiries which you have made with regard to diseases in sheep, in the course of which you had recourse to experiments on animals?—That is so.

19988. I believe the experiments which you are going to tell us of were first of all carried on privately?—Yes.

19989. And then afterwards, at the suggestion of the Highland and Agricultural Society, who desired you to continue them?—Yes.

19990. And then you were Chairman of a Departmental Committee which was appointed by the Board of Agriculture in 1901?—That is so.

19991. And who issued their Report in 1906?—Yes.

19992. That Report is contained in three parts presented to Parliament, numbered Cd. 2932, 2933, and 2934?—Yes.

19993. And in that Report there is a detailed account of the researches which have been spread over four years?—That is so.

19994. I will take you at once then to your evidence on that subject. The Committee was appointed as you told us, in 1901, by the late Mr. Hanbury, then President of the Board of Agriculture?—Yes.

19995. And the object was to investigate the two diseases of sheep known as louping-ill and braxy?—Yes.

19996. Would you explain to us what the extent of the mischief was?—These two diseases of sheep that we were appointed to inquire into are extremely disastrous, and the cause of enormous mortality all over Great Britain more or less, especially in Scotland, the northern counties of England, and also in Ireland; they prevail in all three divisions of the British Isles, and the mortality from them is something terrible, so much so that in certain districts where they prevail sheep farming as a profitable industry is so hampered that it is threatened with extinction, if that has not already occurred in many places.

19997. Has that been going on for many years?—It has been going on for a great many years—as far back as we have any records, apparently.

19998. Has it been worse of late years?—Yes, I think so.

19999. One has not regarded sheep farming as being an industry that was in danger; the general public were not aware of the extent of the mischief, at any rate, until recently?—Some parts of Scotland have become depopulated on account of the enormous loss. As much as a quarter of a million of money, it is said, is lost from braxy alone, and that is probably an under-estimate of the total.

* *This gentleman's name does not appear in the Medical Register or Directory for* 1885.

20000. A quarter of a million of money altogether, do you mean?—Annually.

20001. It affects particular areas, you say?—Yes.

20002. But it spreads?—Yes, it has been endemic more or less all over the west of Scotland for as far back as anyone can remember, at any rate as far back as the sheep industry goes. The sheep industry in Scotland on a large scale commenced only at the beginning of the last century. Previously to that the country was all laid out in cattle; about the beginning of the last century the sheep industry on a big scale commenced, and the disease has prevailed on the west coast of Scotland ever since; in fact, long before that apparently. There are records of its being a very old disease.

20003. You say it is very destructive. Up to what percentage has it been destructive in the farms most affected?—In the case of braxy I have known from my own experience, 10 to 15, 25, 50, 80, and 100 per cent. of mortality over certain districts in certain farms. From louping-ill the mortality is usually not so high, but sometimes comes up to 15, and 20 to 25 per cent.

20004. There is a great loss from mortality, and, I suppose, great interference with business in not being able to transfer sheep?—Not only in the transference of sheep, but in the whole farming operations. The mortality interferes with the regular routine of the sheep farm.

20005. I think that you propose first to deal with louping-ill which is what you first dealt with in the Committee?—Yes.

20006. Will you tell us what steps you took in investigating that disease?—Our first experience of the disease was gained in the valley of the north Tyne, the Duke of Northumberland's property. His Grace very kindly furnished us with a station there and fitted it up for us, so that we could undertake the investigation.

20007. There are large sheep farms there?—Yes, very large sheep farms. We lived in the district for two seasons of something like two or three months each, and most of my experiments were carried out in this neighbourhood on that particular disease.

20008. Will you tell us what the symptoms of the disease are?—The first thing you notice probably is that the sheep will appear duller than its mates, and separate itself from other sheep in the same run, and it shows then more or less pronounced toxic symptoms, looks almost as if it was suffering from alcoholic intoxication sometimes, a reeling gait, giddiness, and a tendency to lean up against any support. Then perhaps in 48 to 60 hours after these symptoms have shown themselves, the animal is unable to stand, and falls over on its side. It now passes into the second stage of the disease in which it is convulsed, the limbs are spasmodically contracted, and so on. The animal's intelligence does not seem affected, it recognises objects perfectly well; but after these phenomena have lasted, say, from four to five days, in the second stage, the animal passes into the third stage in which it becomes more or less paralysed in all its extremities.

20009. Does it fall to the ground?—Yes, after it once falls over on its side it never gets up again; it is quite incapable of standing, and the limbs hang listlessly down when it is held up by the fleece.

20010. After lying in that condition for some time it dies, I suppose?—It usually goes on from bad to worse. Sometimes it may linger. I have known them linger as long as five weeks in some very chronic cases, but usually within a matter of a week to ten days after the commencement of the disease the animal is dead.

20011. Does the disease always terminate in death?—My own experience favours the view that the great majority of cases terminate fatally. Shepherds will tell you that in some cases recovery takes place; they will point out individual sheep to you which have suffered from the disease in the previous season; but my own experience leads me to doubt whether these were cases of louping-ill at all.

20012. How did you proceed to investigate the cause of the disease?—We began, of course, by studying the phenomena of the disease so far as we could make them out by mere visual and other external means of examination, and an examination of the carcases of animals that had died.

20013. What did you find?—We found so very little that was tangible in this particular disease that we for a long time were puzzled.

20014. You mean that the organs appeared to be healthy?—The organs appeared to be healthy. In the vast majority of cases, if you see the animal immediately after its death, or if you slaughter it in the height of the disease, the organs appear to be practically healthy; a few punctiform hæmorrhages along the intestine are perhaps the only residue you will find; the brain is apparently quite healthy, and the spinal cord also so far as one can make out by mere naked eye examination; the membranes of the brain and the spinal cord are healthy.

20015. And the blood?—I could never find anything wrong with the blood. I examined it over and over again. I must have examined, I daresay, thousands of samples of blood.

20016. Then having learnt nothing as to the cause from what you call your *post-mortem* examinations, did you proceed to experiment on living animals?—We did. We found it absolutely impossible to get any further before we tested whether there was any materia peccans, anything pathological in the liquids or the tissues of the body.

20017. And will you tell us what you found by experiments on living animals?—We found that we produced no result by inoculating the blood.

20018. And the cerebro spinal liquid?—Yes, the same; it had no effect, the blood and cerebro spinal liquid were negative.

20019. You found it had no effect deleterious or otherwise upon the sheep, did you?—No apparent effect whatever.

20020. Did you then proceed to investigate the abdominal cavity when you found these punctiform hæmorrhages in the intestine?—Yes. As a last resource we investigated the liquid in the peritoneal cavity, the abdominal cavity. We noticed that very often it presented a turbid appearance, especially a few hours after death, and on inoculating this subcutaneously we killed the animal practically in every case.

20021. The inoculated animal?—Yes, the inoculated animal.

20022. Did you say what you found in the peritoneum?—The bacillus, a long rod-shaped organism, was present in the peritoneal liquid; where we were perfectly certain about the nature of the disease it was found practically in every case; it had peculiar characters and the same characters in each case.

20023. Did you find it in the intestines also?—Yes, both in the abdominal cavity and in the intestine. In the intestine sometimes the bacilli were in tremendous numbers.

20024. (*Colonel Lockwood.*) Was it like the anthrax bacillus?—It is something like. It resembles the anthrax bacillus in so far as it is rod-shaped, but it has morphological differences, and grows differently from the anthrax bacillus, although probably there is a certain relationship between them. I have always had a notion that they may belong to the same family.

20025. (*Chairman.*) You injected, I understand, the liquid containing these bacilli into the sheep?—Yes.

20026. With the result as you have told us that you killed them?—Yes.

20027. You killed them more quickly than the natural disease did?—Usually much more quickly. When one inoculated under the skin, the animal usually died in from 36 to 48 hours. Some cases, however, were protracted.

20028. (*Mr. Ram.*) Did the inoculated animals show all the usual symptoms?—That depends apparently upon the time that the animal lives after inoculation has been performed. In most cases the animal dies very rapidly from the toxic poisoning; it is dead within a few hours afterwards; but in those cases where the animal has lived over, say, four or five days, I have seen the most perfect reproduction of the disease—not only in the sheep, but also in the rabbit—the tremblings and spasms, and so on, and paralysis of the limbs, all exactly as in the natural disease.

20029. (*Chairman.*) Would you tell us the conclusions that you came to form these experiments?—The conclusions that we came to were that the disease was caused by a specific bacillus inhabitating the intestine, and under certain circumstances getting over into the peritoneal cavity.

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20030. Did you come to any conclusion as to the nature of the propagation of the disease which I believe up to that time you had not ascertained?—Yes, we did; we came to the conclusion that the disease is spread through the dejecta of the animal through the manure, and also through the carelessness of farmers in leaving the carcases about everywhere.

20031. You mean that these bacilli would in that way get into the mouths of the feeding sheep?—The spores will lie in the soil for a long time apparently, and they are taken up by the second host, the second sheep, and in that way the disease is reproduced.

20032. That was the result of your investigation of louping-ill?—Yes, so far.

20033. Now as to braxy, will you tell us what your investigation was, what the course of it was?—Braxy is due to an organism of the same nature, although not identical with that of louping-ill. It was discovered in Norway originally.

20034. Was it discovered by Nielsen?—Yes.

20035. And he discovered the bacillus that you believed to be the cause of the disease?—Yes, he did, and our investigations corroborated it.

20036. Do you know how he discovered it?—By looking for it and by experimenting with it. No one thought it worth their while to look for it before.

20037. Did he experiment on living animals for the purpose?—Yes.

20038. That was how he discovered it?—Yes.

20039. And what was the habitat of that bacillus?—We discovered that the habitat is the same as that in louping-ill—that the intestine is full of it practically, absolutely swarming with the organism.

20040. The symptoms of the disease are quite different, are they not?—Yes.

20041. Is it a very short disease?—It gets the name of braxy, I believe, from an old Scandinavian derivation, signifying the suddenness of the disease—the suddenness of the fatality of the disease. Bradsot it is called in Scandanavia and our word braxy is probably a lineal descendant of the same.

20042. Does "bradsot" mean quick or sudden?—Yes, quick disease.

20043. Had it also been ascertained by Nielsen that sheep might be inoculated with the bacillus, and so take the disease?—Yes; that is so.

20044. Accordingly you began your investigations in braxy with rather more knowledge; you were more advanced in your knowledge than you were in the case of louping-ill?—Yes.

20045. But so far you seem to have satisfied yourselves in both cases; in the one by your own investigations and in the other by the investigations of Nielsen, that you have ascertained what the cause of the disease is?—Yes; certainly.

20046. And its bacillus?—Yes.

20047. Did you accept Nielsen's experiment, or did you experiment yourselves for that purpose?—We made a great many experiments.

20048. To find this bacillus?—To find this bacillus, and in the way of prevention afterwards.

20049. I am coming to that. I was dealing with the question of finding the cause?—Yes.

20050. As to remedial measures for both these diseases, would you tell us what you had to look to. Was there any remedy that you could discover when the animal once got the disease?—No; there was no remedy that we could discover. There were any number of popular remedies, but nothing that was really founded on any scientific basis, and I think they were all equally bad, so far as I know.

20051. You were obliged to look to prevention, therefore?—Yes. When once an animal is attacked with either of these diseases anything in the way of treatment, from my experience, has very little effect upon it.

20052. Could anything be done in the way of prevention by merely segregating the animals that were attacked with it?—That has been tried over and over again, shifting them from one pasturage to another. It is practised by farmers only as a last resource; shifting them, say, from hill pasture down to low pasture, and so on. My own experience is, and I have seen it done frequently, that they go from bad to worse.

20053. I rather meant as soon as the animal showed any signs of the disease, separating it so that its manure after it was diseased did not affect the pasture of others?—I do not think that has the slightest effect—the disease is so widespread.

20054. And the object, I suppose, you had was to immunise the animal?—Yes.

20055. Was that the only remedy that suggested itself to you?—It was the only feasible remedy so far. The animal dies within a few hours after it is seized by braxy, and there is no time to apply any remedy.

20056. Did you experiment with that view of obtaining immunisation?—Yes.

20057. Will you tell us about your experiments and the result of them?—We thought first that the most likely method of producing immunity in an animal would be that which is frequently practised upon animals—namely, that of the introduction into the system of the animal of the organism in an attenuated condition; in a weakened condition, that is to say, when the full virulence of the organism is lessened by certain procedures. We found that that method, although it occasioned apparently a certain amount of immunity, was a dangerous one.

20058. Why?—By no meanst infrequently we killed the animal, and, moreover, the carrying out of this method on a large scale in sheep-farming districts is impracticable; it entails the use of a delicate instrument in unskilled hands; and hence we gave it up altogether.

20059. In looking for some method of immunisation, had you any guide from the manner and time at which braxy attacked the animals?—Yes, we had. Braxy is essentially a disease of hogs.

20060. That is first year sheep?—Yes. Farmers look upon their two-year-olds as practically safe. Sporadically a case occurs now and again, but the great mass of their sheep after the second summer are safe. That suggested to us that immunity, if it is an acquired immunity, must be acquired through some agency residing in the soil, and manifestly the idea struck us that the spores of the organism getting into the alimentary canal, are Nature's agent in inducing this immunity.

20061. Are sheep more liable to take braxy at one time of the year than another?—Yes; there are certain months of the year in which they are more susceptible. This is just about the height of the braxy season at present; it begins usually in September—or at any rate, what farmers call braxy—and goes on to about the middle or end of February.

20062. Is it your suggestion that the sheep which took these spores into their system, and were only immunised by so doing, and not killed, took them in at a time when braxy was not prevalent, or that they were exceptionally strong sheep?—Most of them had very likely become immunised in their infancy, whilst still lambs; that is my strong impression.

20063. You mean that they would not be so susceptible?—They would not be so susceptible. Others probably enjoy a partial immunity, and are more resistant. But my strong impression is—and I am not alone in this, I have heard it from sheep farmers—that all the animals pass through a mild attack of the disease.

20064. How did you proceed to deal with it then, when you had all these facts before you?—The method of dealing with it was that we got the organism in a pure state first of all, and isolated it in a pure condition, and after that grew it artificially. We grew it from the peritoneal liquid when it was very abundant, the abdominal liquid, we may call it, and then this was administered as a drench.

20065. That means as a liquid?—Yes, poured over the throat of the animal.

20066. Administered by the mouth, you mean?—Yes.

20067. And at any particular time of the year did you do that?—Yes, at the time when sheep are practically immune from the disease, in the month of August.

20068. Have you carried out your experiments on those lines for any length of time—that is to say, having discovered the bacillus, having ascertained

that the only mode of prevention was by administering a drench of this culture, did you experiment with this drench?—That is so; we have experimented for several years progressively—experimentally I mean—in so far as we might modify the method and study what the effect would be over a whole braxy season.

20069. And ascertaining what would be an effective dose, I suppose?—Yes, ascertaining what would be an effective dose, and in what form the bacillus ought to be administered, and so on; all these things we had to work out experimentally.

20070. At present do you consider that you have arrived at success?—Fair success, but by no means what we anticipate in the way of still further progress.

20071. I suppose all this was done by experiments upon living animals?—Yes.

20072. What animals?—Sheep, and other animals as well; rabbits and guinea-pigs, but chiefly sheep; almost entirely sheep.

20073. Do you consider that you have arrived at a true immunising remedy?—There is very good evidence to show that we have. What is defective still is apparently the method of preparing it. I think we can prepare it in the spore stage instead of the bacillary stage, and if so, there will be a much better chance of its immunising the animal.

20074. How many sheep have you administered this preventive to in the course of last year?—During August we treated, I think, some 13,000, and in the springtime, in February of this year, I think some 4,000; something like 17,000 odd have been treated this year.

20075. Are you speaking of braxy, or both braxy and louping-ill?—Both braxy and louping-ill.

20076. You treated them both in the same way?—In one line of treatment we did. A very important point was to make out experimentally whether one could immunise an animal to braxy and louping-ill at the same time, as that would mean that one could treat the animals at the same time and save the farmer gathering his sheep twice or three times a year; and these experiments are still going on.

20077. You say that you administered this drench to 17,000 sheep. I suppose you rather look upon it as medical treatment of them?—Yes, that is the view we take of it.

20078. But for the purpose of obtaining the right dose, and seeing what the effectiveness was, and so forth, you experimented upon certain sheep in the first instance?—Yes, individual sheep in small numbers.

20079. About how many?—I do not know that I could give you the figures exactly from memory.

20079A. Was it 50 or 100?—Sometimes we have used 50 or 100 sheep, or more, I dare say.

20080. Were any of those killed by the experiment?—That depended upon the nature of the experiment a good deal. In the case of subcutaneous inoculation we certainly killed a good many. There is no doubt we did.

20081. That was a line which failed?—That was a line which failed, but by administration by the mouth, if given at the proper time of the year, I do not think I can recall any death that we had from that method.

20082. Did the animals seem to take the disease to a certain extent; did they seem to be made ill by it?—Yes, sometimes they would remain ill for a day or two in the case of louping-ill. One proprietor described it as if they were all intoxicated. He thought they were all going to die, but he said, most remarkably, they all recovered, and he did not lose a single one of them.

20083. After you took to this method of administering the preventive by a drench instead of subcutaneously, there were practically none lost, I understand?—I do not think so. I do not think I can trace one. At any rate, in this present year's experiments I have had no complaints of that kind whatever.

20084. Now to what extent can you say that you have ascertained any benefit to the flocks—that is to say, to what extent, if any, do you consider that you have suppressed this disease?—Perhaps it will serve your purpose if I give you an individual instance.

20085. Yes?—A proprietor on the west coast of Scotland, who was determined that he would see the experiment carried out properly, took the very greatest pains. I may tell you that it is very difficult to get these experiments carried out as one would wish among sheep-farming people; but, being a man of education, and determined to see the thing out, he made the crucial experiment of drenching 400 sheep.

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20086. Were those the whole of his first year sheep?—He had 20 others, and that comprised his whole stock of 420. They were put under exactly the same circumstances, the first lot treated against braxy, and the second lot in its natural state. Of the 400 drenched sheep he lost 30 up to the end of May, many of them, it was said, from louping-ill. I examined the peritoneal liquid from the carcases of 11 of these, and could not discover that there were any deaths from braxy, or only two, which were doubtful.

20087. Do you mean he lost 30 from all causes?—Thirty out of 400 he lost from all causes; whereas in the case of the 20 undrenched sheep he lost 19.

20088. (*Mr. Ram.*) Were they examined after they were dead?—Yes, I examined a large number—not the animals, but the liquid from the abdomen. I got the liquid from the abdomen, and had it very carefully examined.

20089. What did it show?—It showed that they died from several other diseases, and very few, if any, from braxy.

20090. Of the 30?—I was speaking of the 30. Of the others there is no doubt there was any amount of it. That has been my invariable experience. If one has a chance of comparing two lots like these, in the one case the number of braxy cases you get is something appalling.

20091. Can you tell us what proportion of the 20 were proved after death to have died of braxy?—I am sorry to say I have not got my figures here, and perhaps I should not enter too fully into that matter as yet, as I shall have to report to the Board of Agriculture first about my year's experience.

20092. Did you examine them yourself?—I did.

20093. And did you find that a considerable number of them showed symptoms of braxy?—Yes.

20094. (*Chairman.*) Do I correctly understand—I want to have it clear—that you examined the 11 that died out of the 400 that were drenched?—Yes.

20095. And you also examined the whole of those that died that had not been drenched?—No.

20096. Take first the 11, please. The 11 you did examine?—I examined as many as possible. I provided the shepherds with means of sending in the liquid, but a shepherd, as I daresay you know, is rather lax, and it is very difficult to get him to pay attention to a thing of this sort. I got as many as I could.

20097. You examined most of the 30?—Eleven of them.

20098. I did not quite understand. Did you find any trace of braxy in any of the 11 that you examined?—I think there were two cases, so far as I remember, I do not remember more, and even these were doubtful.

20099. And I think you said the whole of those that were undrenched died?—Well, 19 of them.

20100. Of those, when you say the whole died, about how many were there?—Twenty.

20101. How many of these 20 did you examine?—I really cannot tell you.

20102. Did you examine half of them, or a quarter of them?—I should say something like a quarter of them.

20103. What did you find in those cases?—In those cases I found braxy bacillus constantly occurring in the liquid sent in.

20104. What do you mean by constantly—that you found a great many bacilli in each, or you found some free of bacilli and others apparently killed by them?—Some free from the bacilli of braxy, others containing it.

20105. You mean in many instances amongst those you examined?—Yes. It is a very complex question, and I see you are a little puzzled about it. I should tell you that there are some braxy-like diseases which are continually called braxy among farming people, but these differ from braxy in the scientific sense of the term, and a certain proportion in each case, both of the 400 and of the 20 died from these braxy-like diseases, with which we had nothing to do.

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20106. It is a complicated matter, and I agree that the distinction between braxy and braxy-like diseases are too complicated for me. I want to know, did you ascertain to your satisfaction whether three dozen or so sheep out of the 20 that you seem to have examined died from the disease which your drench was intended to prevent?—Certainly a large proportion had died from braxy in the proper sense of the term.

20107. You say a large proportion of the 12?—No, of the 20.

20108. But you only examined one-quarter of the 20?—Yes; a large proportion of them did not come under my own observation. I could only take the word of the shepherds of the total mortality.

20109. As regards the remainder of the 20 that you did not actually make a *post-mortem* examination, did you see any symptoms that that was the disease that they died of?—I did not. It was a long distance off, in a wild district.

20110. Then I must ask you generally, you have given us a particular instance which, I suppose, was as good an instance as you could get for the purpose of ascertaining?—Yes, a very good one.

20111. What conclusion did you come to as to whether or not you were on the right track?—We came to the conclusion from the returns that we got, the evidence we had heard, and the examination of the peritoneal liquid, that, with certain exceptions, the mortality in those cases treated by us was considerably lower than in those which were not treated.

20112. When you say with certain exceptions, what do you mean; do you mean on certain farms?—On certain farms where, apparently, the disease that they suffered from was not braxy at all—it was very often a disease known as blackquarter, but which they call braxy very often in a loose sense of the term.

20113. Was your prophylactic intended to meet the case of blackquarter?—No, not in these particular instances that I am referring to.

20114. Now as to the experiments which you performed; were the experiments made on the farms?—They were made on the farms and also made in licensed premises that I have in Aberdeen.

20115. The others on the farms?—On the farms a large proportion—the majority, in fact.

20116. You were acting then for the Board of Agriculture, I understand?—I was.

20117. And had you a licence for performing them on a farm?—I had.

20118. I think we have heard so much evidence about the amount of pain that is caused—if it be called pain—by mere inoculation experiments, that we need not trouble you about that; but as regards the consequences when you were giving the disease to the animals as a preventive, did they seem to suffer at all from it?—Not the slightest when administered by the mouth, with the exception of their being a little upset for a day or two, and giddy occasionally, but only occasionally.

20119. You have told us roughly the number of sheep that you had to operate on in these experiments; can you tell us what the number of sheep was that died from braxy and louping-ill, on an average, in the years that your inquiry was going on—say in 1901, 1902, and 1903?—It is very difficult, of course, to say; it varies in different years, and it varies on different farms, but as a general statement you may say, as I think I mentioned before, from braxy 15, 20, 25, 50, and 80 per cent., or even total annihilation.

20120. I was not asking you the percentages; you gave us those; but is there any return made to the Board of Agriculture of the actual number of sheep—the thousands of sheep that died from these two diseases, say in the years 1901, 1902, and 1903. Did 500 or 5,000 die in a year. I do not know what 25 per cent. of the sheep in Scotland is. Sir John McFadyean tells me there are no statistics?—If you mean statistical returns, I do not think there are any.

20121. But the number of sheep fed upon these parts where braxy prevails, I suppose, amounts to many thousands?—Yes; it must be enormous.

20122. So that if your experiments were successful, even to the extent of reducing it 5 per cent., that would mean a great many more lives saved than were lost by your experiments?—Certainly; enormously so.

20123. (*Colonel Lockwood.*) All these experiments, of which you have conducted a very large number, were conducted under licence?—Yes.

20124. Were the 13,000 sheep treated experimentally or not?—No.

20125. Do you claim that before you began the experiments on the sheep you had tried any other means of investigation?—In what way? I do not understand exactly what you mean by investigation.

20126. I mean by watching their symptoms or trying to discover by any other means except experiment on a living animal?—The first thing we set ourselves was to examine the symptoms and note those with the very greatest care, the *post-mortem* changes, and so on, but as I have explained in evidence already, it was always too late, we could not get any further with the inquiry, and it was necessary to resort to experiment to elicit further knowledge.

20127. Do you claim that by those experiments you have obtained any satisfactory results?—I think we have got extremely satisfactory results. Were it only that we now know something definitely about the nature of the diseases which stimulated inquiry, it would be an enormous result. But I think we have got more than that, I think we are on the lines calculated to prevent the diseases.

20128. But you do not claim yet to have discovered any cure?—I do not think, as I explained before, that you can cure these diseases. I have never seen anything that did the animal any good when once it was attacked by braxy.

20129. Do you claim that these experiments of yours have produced a preventive?—I do.

20130. (*Sir William Church.*) Is anything yet known of the life of these bacilli outside the body?—There is a great deal known about them.

20131. I meant whether any experiments had been made with soil or any form of vegetation to see whether they can maintain their life?—That has all been worked out. One of the original ideas, I remember, so far back as 1882, when I began working at these diseases, was that there was something contained in the soil or the nature of herbage, which was the cause of them. The Highland and Agricultural Society appointed a Committee to investigate that among other things, with the result that nothing of the kind which you refer to has been discovered from the herbage point of view.

20132. I meant that at present you do not know whether this bacillus continues to live and grow, and probably go through the state of sporification in the soil?—It is voided in the soil; the spores are voided into the soil through the manure, and through the carcases of the animals, in enormous numbers, and I know this, so far by experiment, that I have kept such spores in their natural habitat, in the natural liquid of the body, for from five to six years in full virulence. I might illustrate this by an experiment which we performed over and over again, and which bears upon the question you have asked me, that of taking a sheep from what we call a "clean" farm, where there is no disease known, louping-ill let us say, where they are perfectly free from it, and bringing these sheep on to a louping-ill field or a braxy field, you will find that two-thirds of them will be dead—if it is the proper season of the year—within ten days afterwards.

20133. And that, of course, is your reason for thinking that it is impracticable by clearing the ground for a certain time of sheep to get rid of the disease?—I will not say that. I will not go so far as that. If you could put cattle on for something like a matter of 50 years or 100 years, I should not wonder (Sir John McFadyean will, perhaps, bear me out) that the ground might be cleared under those circumstances.

20134. I should like to be quite clear about your experiments—not as to the cases treated medicinally, but your experiments. I understand that of those that were drenched 30 died, and some of those you examined and verified yourself, by finding the bacillus, that they died of braxy?—In the undrenched ones.

20135. Out of the number all of which died, you personally only examined a few of the bodies?—A few of the animals. The drenched ones were sent in to me especially, because I had given instructions that they were to be sent in; the others, the undrenched ones, were sent in more at the will and wish of the proprietor.

20136. But in the case of the 20, 19 of which died, those who were present and observing them during their illness thought it was braxy?—Yes.

20137. And by saying that you do not know that they died of braxy, you simply mean that you had not an opportunity of verifying it by finding the organism?—That is so; they were quite of opinion that they died of braxy.

20138. (*Sir William Collins.*) May I take it that the evidence that you have been so good as to give the Commission this morning is offered by the University of Aberdeen as an example of the valuable results which may be obtained by experiments upon living animals?—That is so; the Medical Faculty thought that perhaps you had not had so much evidence of this kind as bearing upon the brute creation directly, and that possibly it might be acceptable to you on that ground.

20139. Did I correctly understand from you that there are several contagious diseases of sheep, between which it is necessary to discriminate?—Yes, that is so.

20140. How did you find the organism in the case of braxy; was it by microscopical examination of the intra-peritoneal fluid?—You find it in braxy all over the body, especially in the peritoneal cavity. If there is any part of the body where you find it, it is in the peritoneal cavity.

20141. By microscopical examination?—Yes.

20142. Did you cultivate it on various media?—Yes.

20143. Are the other diseases from which you consider it necessary to discriminate braxy, those of blackquarter, malignant œdema, and louping-ill?—That is so, these are among them.

20144. May I take it that you have the means by bacteriological investigation of discriminating accurately between those four diseases?—I think so. They belong to a family, doubtless, and they have certain points in common, but by the method of culture and one thing and another, we can distinguish them with great readiness. I will not go so far as to say that there do not occur exceptional cases where I should be in doubt, but usually by means of prolonged culture and inoculation more particularly, they can be differentiated.

20145. Should I be right in saying that an expert ought in any case to be able to discriminate accurately between louping-ill, braxy, blackquarter, and malignant œdema?—I think so.

20146. Did I correctly understand you to suggest some possible relationship between braxy and anthrax?—I should not wonder if they belong to the same family. I will not say that it has anything to do with anthrax, or any anthracoid diseases; but in the manner of growth, and one thing and another, it has in some respects a resemblance to anthrax.

20147. If these be four separate diseases, should I be right in thinking that it would be necessary to employ different methods of protection against each of them?—I have tried protecting against the whole of them simultaneously.

20148. Has it been successful?—I do not know yet; it is being tried this year.

20149. Do you mean by administering some polyvalent dose?—Yes.

20150. Would that be mixing the preventive fluids against the four diseases?—Yes.

20151. But would a preventive fluid that was good against braxy be good against blackquarter?—No.

20152. Or *vice versâ*?—No. I do not think so, at any rate, my experience leads me to suppose that it would not be.

20153. You think that a prophylactic which was found to be good against blackquarter, could not be relied upon as protective against any of the other three?—I think so.

20154. There has been some confusion, has there not, in the minds of some investigators, as to these four diseases which you regard as distinct?—There is a very great deal of confusion. I think the matter wants clearing up.

20155. Has your attention been called to an outbreak that occurred on Romney Marsh?—Yes.

20156. What was the nature of that disease?—It was what that call "struck."

20157. Was that a case of braxy?—No, not what I saw of it, at any rate; the cases I saw of it were not braxy certainly. Everything else points to its not being braxy.

20158. Was it blackquarter?—I do not know; I have not had sufficient experience of it. I have not seen a sufficient number of cases. I went down to Romney Marsh and investigated it there as well as I could, but I have not had sufficient experience to say whether it is blackquarter or not.

20159. I see on page 9 of Part I. of your Report it is stated: "The disease has been pronounced by Principal McFadyean to be blackquarter"?—I do not know whether Sir John is of the same opinion still; but it is quite possible that it is blackquarter. I do not know, I would not like to say that it is not, possibly it is; only it did not look to me as if it was blackquarter.

20160. After considering that suggestion, I see on page 10 of your Report the Committee say: "The question of louping-ill being blackquarter in the sheep may, therefore, be put out of court; the two diseases are opposed to each other in all essential respects"?—Yes, I think the view is quite untenable.

20161. Then I see on page 27 of the same Report you say, "We must sound a note of warning, however, as to the result to be anticipated"; that is in regard to preventive treatment?—Yes.

20162. "Let it be clearly understood that our treatment is projected against louping-ill and nothing else. One of the first objects to be ascertained is whether the mortality upon a particular farm is caused by louping-ill or by one or more of the many other contagious diseases of the sheep which are so often associated with louping-ill and mistaken for it. Thus, we have found that malignant œdema prevails in many districts, and has been confounded with louping-ill over and over again, even by the most experienced sheep-farmers and shepherds. This is quite excusable, seeing that the symptoms of malignant œdema might easily be mistaken for those of acute louping-ill. Then stray cases of braxy and of the braxy-like diseases occur among sheep during the louping-ill season, and particularly among those which have been wintered on the east coast of Scotland, and which have returned in the spring to their native pastures in the West Highlands. The confounding of these with cases of louping-ill, of course, tends to vitiate any results obtained by our method of prevention." They think it is necessary to draw attention to that?—Yes, we discovered that frequently.

20163. Then I see you go on to say, "The problem of the prevention of the contagious diseases of the sheep is a large one, and will most likely require years of patient observation and experiment before it will reach perfection. Those which are most destructive in this country are closely related, and hence there is good reason for believing that they may be combated upon the same principles as those which have proved so effectual in the case of louping-ill. It may turn out that the sheep can be immunised to several of them at the same time." Those are the experiments you were referring to just now, I suppose?—Yes.

20164. You say, "In the meantime, however, we would beg those interested to exert a little patience, and not to draw conclusions, at present, from what may seem to them, in certain instances, a disappointing result. The pathology and prevention of this class of diseases of the sheep is far more intricate than the ordinary layman may suppose, but we feel assured that we are on the path which will ultimately lead to a successful issue"?—Yes, that is so.

20165. Am I right in gathering from that that you regard the matter as still *sub judice*?—Certainly, the matter of treatment or prevention.

20166. I think you called attention in your second report to the curious bactericidal power of sheep's blood?—Yes, that is so.

20167. Which I think you say varies in different seasons of the year?—Yes.

20168. That, I suppose, would introduce a rather complicating factor in any preventive treatment?—Most complicating.

20169. You state, I see, in the second part of your report, on page 239, among your conclusions, "The blood of the sheep at certain seasons of the year is eminently bactericidal to certain disease microbes which are the causes of most of its contagious diseases, while at other seasons it loses this property, more or

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less completely, and becomes an excellent medium for their propagation?—Yes.

20170. That must be rather a confusing factor?—It is a most confusing element; it vitiates experiments; experiments may be negative or positive, according to the season at which they are performed.

20171. Then I see on pages 201 and 202 of your second part you say, "This, however, seems fairly clear, namely, that animals in good condition are more liable to braxy than those poorly nourished. But even in this respect there are endless sources of fallacy. Is it, strictly speaking, braxy that these well-fed animals die from, or is it something else which has been mistaken for braxy? Is it so-called 'blackquarter,' for instance, or is it the disease known as 'struck' or, finally, is it a disease hitherto unrecognised? To commit one's self to such untested statements would be productive only of further confusion where confusion is already rife." Should I be right in thinking from that that it may turn out, after all, that blackquarter and braxy are the same?—No. The term "braxy" in the bucholic mind is one applied with a very wide margin; a shepherd will tell you that there are all kinds of braxies, red braxy, white braxy, water braxy, and so on, these all representing different diseases.

20172. I see you proceed on the same page to say, "All these crude and untutored notions seemed, in our estimation, quite inadequate to account for the seasonal character of the contagious diseases of the sheep. We felt there must be something deeper, probably something peculiar in the constitution of the sheep, in all likelihood something reaching far back in the animal's ancestral history which was at the foundation of this seasonal peculiarity"?—That is so.

20173. Some constitutional peculiarities, I apprehend, irrespective of any microbic invasion?—Yes, quite.

20174. Have you formed any opinion as to the method of communication of the disease from sheep to sheep, whether by tick or otherwise?—I have.

20175. Would you be so good as to state it?—We made a great many experiments with the tick to see whether we could communicate the disease through the bite of the tick.

20176. (*Chairman.*) Which disease are you speaking of?—Louping-ill. There was a prevalent notion that louping-ill might be conveyed by the bite of the tick, probably from the fact that the tick appears parasitically in the sheep at the same time as louping-ill, both in the spring months, and accordingly we had to see what truth there was in that opinion. I may say that in some of the most pronounced cases of louping-ill that we saw the sheep had not a single tick upon it, nor a single tick bite. Others were covered with ticks, but never did we see any evidence of the virus being inoculated from the point where the tick had bitten. We showed experimentally that if you tattooed the skin of the sheep with a little of the virus of louping-ill you could produce louping-ill, and you produced a local lesion, a lesion very much like that found in blackquarter sometimes; but never in any instance did we see the disease spread from a tick bite as in the case of this superficial scarification.

20177. (*Sir William Collins.*) You think the tick may be dismissed then as the medium of conveyance of the disease?—No, there is one method by which I think it may under rare circumstances (but so rare that it may almost be put out of account), become the means of conveyance, namely, when the tick is swallowed. The tick crawls all over the soiled parts of the fleece, and becomes smeared with the fæcal matter and so on, from the moist parts of the skin, and under the circumstances I can quite well conceive that it might convey the disease when it is swallowed by the sheep.

20178. Then the evidence points to the fact that the disease is communicated by ingestion rather than by inoculation?—Certainly.

20179. In your earlier observations did you find that the slaughter of the animal in the earlier stages of the disease was the best mode of finding the cause or by allowing the animal to die and examining the carcase?—By allowing the animal to die; but never in any instance when I examined the peritoneal liquid in an animal killed during the height of the disease, did I fail to recover from the peritoneal liquid by incubation the same bacillus as that which I found so constantly.

20180. I see on page 51 of the second part of your Report you say, "We discovered, however, as time went on, that the killing of the animal was an entirely mistaken procedure"?—Yes, if you want to investigate the disease.

20181. May I ask what was the earliest date of the administration of drenching?—I think there were some preliminary experiments which we made (I am open to correction) in the years 1903 and 1904. 1903, I think it would be, when we begun.

20182. When did you first try it on a large scale?—We went on increasing the number year by year up to the present time.

20183. Have you formed any idea as to the duration of the alleged immunity?—In braxy it seems to last the life-time in the majority of cases. As in the case of small-pox, you will find exceptions here and there; but in the case of braxy I think it will last the natural life, or the commercial life of the animal, at any rate—quite sufficiently long for commercial purposes. I am not so sure with louping-ill whether that is so. Two-year-old sheep take the disease, and it is said sometimes take it twice. I do not think, in the case of louping-ill, it is quite so lasting, although it tides the animal over the louping-ill season, and that is the great point.

20184. When you speak of the duration of a life-time, what period have you in your mind?—From two to two and a half years, that is to say, in the case of an animal for slaughter.

20185. Referring to the particular case of the 420, when the 400 were drenched and 20 were not drenched, has it been the subject of an official report?—Not yet; we are getting the report ready. It has been a most arduous matter to procure all the details of such a report, and we are trying to render it as accurate as possible, to sift out the kernel of truth from the many statements sent into us; it will be presented to the Board of Agriculture before many weeks. I have it nearly ready.

20186. I was wondering whether it was in a form in which you could favour the Commission with it?—I do not think so. I must present it to the Board of Agriculture first.

20187. I did not quite gather your evidence in regard to the 30 out of the 400 that died. I understood you to say that they died of various things, including accidents?—Yes, various things.

20188. In the case of how many of those 30 was the peritoneal fluid presented to you for examination?—I have not got my note-book with me bearing upon that particular subject; but I think I made up the number when I got the details returned to me to be eleven.

20189. Including those cases by drowning?—I do not know. If you take the total mortality you must always leave a margin for drowning—and all sorts of accidents.

20190. But I understood that the 30 included all these casualties?—Yes.

20191. I was wondering how many of the 30 were submitted to you for examination?—Eleven.

20192. Were there any casualties among the 30?—I cannot tell you definitely. I would not like to commit myself upon a matter of that kind. I cannot remember at present.

20193. Do you think that you examined one fourth, as I understood you to say to the noble Chairman, of the 30 that were undrenched and died?—I would not like to say. I could not commit myself to any figures definitely. I am only speaking from memory.

20194. You prefer not to commit yourself to any precise figures?—If you please. I must present them to the Board of Agriculture first. I would like to have my figures before me before I say anything further about it.

20195. (*Sir John McFadyean.*) These investigations of yours have been followed with great interest, I understand, by the agricultural community of Scotland?—They have.

20196. And so far as you know the line of investigation that has been pursued has had their approval?—Yes, it has had their approval with certain failures in certain districts.

20197. I was not referring to that. I mean that they do not disapprove of the fact that you have

attempted to solve the causation and method of prevention of these diseases by resorting to experiments?—Very far from it.

20198. Is there an Anti-Vivisection Society located in Scotland, do you know?—I think there are some ladies inclined that way.

20199. Have they formed themselves or been actually constituted into a society?—I do not think so.

20200. So that no opposition that you are aware of has been raised to your experiments?—No combined opposition. I think many persons hold very strong views upon the subject, and they may have individually complained. I do not think there is any society of that kind, so far as I know.

20201. It has been suggested here by various witnesses that experiments on animals with a view to extend knowledge with regard to human or animal disease ought to be absolutely prohibited by law. What do you think would be the general feeling of agriculturists in Scotland with regard to such a proposal?—I would not like to speak for the agricultural community, but from conversation and wide experience that I have had with intelligent farmers, I think it would be perfectly disastrous; it would raise a *furore* among the intelligent agricultural population were you to attempt anything of the kind. That is my impression.

20202. You are pretty well in touch with agriculturists?—Yes, I have seen a great deal of those interested in sheep farming at any rate.

20203. (*Sir Mackenzie Chalmers.*) Do I correctly understand that you in giving evidence to-day represent the University of Aberdeen or the Medical Faculty of Aberdeen?—Through the Medical Faculty you may say I represent the Aberdeen University. I do not think the matter came before the Senatus, but we had it up at the Faculty and discussed it there on two separate occasions.

20204. May I ask if the Faculty are unanimous or not on the subject that experiments on living animals are necessary?—Perfectly unanimous.

20205. I take it as regards these experiments that you performed that your licence enabled you to experiment on unspecified farms, that is to say, where the disease was, as well as in a laboratory?—Yes, what is called a travelling licence.

20206. Without a travelling licence could you have effectively experimented?—I could not; I should have been breaking the law had I done so.

20207. When you are dealing with animal diseases spread about over the country, is it essential that the experiments should be performed where the disease is found?—Absolutly essential. I would not trust to laboratory experiments exclusively on a matter of that kind. My experience is dead against that line of inquiry being final.

20208. Would there be this further danger that you would have to bring infected animals from a long distance to the laboratory, which would in itself be a danger?—We would not be allowed to transport the animal.

20209. I may take it that your opinion generally, and the opinion of your Faculty, is that animal experimentation is a necessity if any progress is to be made?—Most certainly; I think that is their unanimous opinion, so far as I am able to speak for the Faculty.

20210. Is there any opposition in Aberdeen itself to animal experimentation?—Very little.

20211. Is that within the University or outside?—Even outside.

20212. (*Mr. Ram.*) You have told us that you discovered the nature and the cause of these two diseases that you were experimenting for by administering to living animals a portion of the fluid from the intestinal tracts of diseased animals?—Yes, and from the abdominal cavity also.

20213. You had, before you tried that, been endeavouring for a considerable time, I believe, to ascertain the nature and cause of the diseases?—That is so.

20214. And so far as investigation of dead animals went, had you failed to discover them?—Certainly.

20215. In your opinion would it have been possible that you could have discovered what you have otherwise than by making use of living animals in the way you have described?—I do not think so.

20216. At any rate you had for years been endeavouring to ascertain the cause without success?—I will not say for years. Two sets of observations were going on relatively, observation of symptoms and *post-mortem* phenomena, and so on.

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20217. And for how long have you been endeavouring to deal with the disease?—It is very hard to specify any time, but in the case of louping-ill you may say for three months—I do not know now I could express it to give you an idea. The two things were going on very much at the same time, although experimentation came on after we had discovered that there was something to work with.

20218. Then it was not, as I imagined your evidence to be, that you had been searching for years, but you had been searching for a considerable time?—I would not say for years.

20219. I think you told us that you were first put upon the idea of immunising the animal from finding in the case of braxy that a sheep which had survived the first year was practically free from the disease?—Yes.

20220. Therefore in seeking to immunise the animals you were following the law that nature gave you in that respect?—Yes, we were.

20221. Is there any official return of the number of sheep kept in Scotland year by year?—I really do not know; I do not think so.

20222. There is none that you know of?—No.

20223. Is there anything by which you can say that the disease has been diminished greatly, either louping-ill or braxy, throughout Scotland in the last year or so?—Nothing whatever, so far as I know, except mere local evidence—nothing on a general scale affecting the whole of Scotland.

20224. You gave us an account of 350 or 400 sheep, some of which were drenched and some were not. Were there other experiments made in other parts of the country besides that one that you told us of?—Yes.

20225. Have you had any result of such other experiments?—We have.

20226. Do they generally give the same result that you told us attended that 400?—With certain exceptions here and there on particular farms and in particular districts, the returns, so far as braxy is concerned, have shown that the mortality in the drenched is very considerably lower than in the undrenched.

20227. I gather, then, that you think you have yet much to learn?—We have a very great deal to learn, I consider.

20228. Do you think that what you have learned in consequence of your investigations has been on the right lines?—Yes.

20229. And you hope that you may yet learn so much more as to effectively combat the disease generally, and what I may call commercially?—I hope so. I am thoroughly confident that we will do so.

20230. Do you find that farmers generally willingly lend themselves to this attempt to immunise their sheep by drenching?—I have not had the slightest difficulty. I have had every encouragement from farmers. I say it to their credit that they behaved extremely well under the circumstances.

20231. Is the method of administering the immunising material by drenching a process which can be done by a less skilled person than would be necessary for inoculation?—Yes, a shepherd could do it perfectly well.

20232. A shepherd, of course, is continually practising drenching?—Yes.

20233. Lastly, may I ask you this: When you were carrying on your examination to test the nature of the disease, did all the animals that showed symptoms of being contaminated with the disease show precisely the symptoms of the disease that you expected to find from the toxin you administered?—Yes, with this qualification—that in the case of louping-ill if you inoculate subcutaneously, usually the virus is so active that it kills the animal before the chronic symptoms come on; but should the animal live over a matter of four or five days, then it presents a picture of the disease. And in the case of braxy all the points are brought out.

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20234. What I particularly wish to arrive at is this: Was the disease the symptoms of which they exhibited the specific disease that you expected to find, and not an analogous disease or a disease that might be mistaken for it?—Not only was the disease the same, but we recovered the same organism out of the tissues.

20235. (*Dr. Gaskell.*) I should like to be quite clear about this matter. I understand that your method of immunisation is to put into the alimenary canal the spores that are cultivated artificially?—We have not worked with the spores yet—with the bacilli.

20236. Is that the same thing as putting into the alimentary canal the same bacillus as the animal itself would take by feeding?—That is so.

20237. There is no attenuation caused by the method of cultivation?—Not to any extent. In the natural disease exposure to weather may have the effect of attenuating the organism to a certain extent.

20238. Then your method, I presume, is to give a mild form of the disease?—Yes.

20239. And so to cause immunity to it?—To give it at the time of year at which the animal is not subject to it.

20240. That is the whole point, is it, to give it at a time of year when the animal is not naturally susceptible to the disease?—Yes.

20241. And so to cause immunity?—Yes.

20242. If you gave it at other times of the year it would not be efficacious?—You would kill them in great numbers.

20243. You say that you tried inoculation experiments?—Yes.

20244. And you tried the method of attenuating in those inoculation experiments?—Yes.

20245. Did you at that time know the difference of the times of the year?—We knew that the particular diseases occurred in the sheep at different times of the year, but we had none of the specific knowledge that we gained afterwards through further experimentation.

20246. May I gather that those inoculations were not given definitely at the time of year when the animal was less susceptible?—Some were and some were not.

20247. Can you say from any records that you have whether there was any difference at all?—I remember that the first inoculations made with louping-ill were administered at the end of the louping-ill season—let us say towards the middle of June; but always afterwards, so far as I remember, we endeavoured to make the experiments during the time that the animal was not subject to the disease, with this exception—that I remember one instance, now that you ask the question, in which we made a subcutaneous inoculation of braxy during the month of November—in the middle of November—with the result that we killed nearly half of the animals, I think, that were experimented on.

20248. Do you suppose, then, that the failure of the inoculation experiments and the success of the feeding experiments were due to the activity of the mucous membrane in the alimentary canal, or something of that kind?—That is one of the deepest questions, I think, in the whole of biology. How these particular organisms act is a matter so complex that I really prefer not to enter upon it.

20249. What I want to be clear about is—are we to understand that the failure of your inoculation experiments and the success of the feeding experiments tend in any way against the value of inoculation experiments in other diseases?—Oh, no. I think you quite mistake what I said about the failure of inoculation experiments. They confer undoubtedly a certain immunity—there is no doubt of that; we proved that; but the difficulty was in the administration of the system in unskilled hands. It means that you would have to provide a skilled person to make the inoculations, that you could not trust shepherds to do this, and in that respect (a very formidable respect, of course) the method of subcutaneous inoculation naturally fell into disuse.

20250. There is just one other question I should like to ask you with respect to your University. Are you authorised to say whether the University desire the existing Act to continue, or whether they would like to see it modified in any way?—I cannot answer for the University as a whole, but I can so far answer for the Medical Faculty. It is my opinion, and I think it is the opinion of the Medical Faculty, that they would desire the existing Act to continue. I think, so far as I know, we are satisfied with the existing Act. I would not like to speak specifically for my colleagues, but I know that those with whom I have talked are quite well satisfied with the present Act and its administration, and we would have no objection to go on under the same circumstances.

(After a short adjournment.)

20250A. (*Dr. Wilson.*) With regard to all these allied diseases, the diseases that you provisionally designate A and B, as well as braxy and louping-ill, you would call them, I suppose, all bacterial infectious diseases?—Contagious diseases.

20251. Would you call them acute febrile diseases? Have you taken the temperature?—Yes, they are acute febrile diseases.

20252. So that, as regards the febrile symptoms, braxy especially bears some resemblance to anthrax?—In some respects it does.

20253. As regards temperature?—Yes, the temperature goes up very high sometimes. It may go up to 105, 107, and 108 sometimes.

20254. And during life have you ever been able to discover what I call the braxy bacillus in the blood?—We found it immediately after death.

20255. But during life?—No.

20256. Then in that respect it differs very materially from anthrax?—Yes, entirely; it is not primarily a blood disease.

20257. With respect to anthrax, is it not very essential that a specimen of the blood should be taken immediately after death has taken place; because it is contended that other bacteria invade the carcase?—If you mean in the case of anthrax, in my opinion, the blood should be taken immediately after death, for several reasons.

20258. Otherwise you could not depend upon the bacteriological diagnosis of the case?—The organism is apt to disappear some time after death. I should not say very apt, but it sometimes vanishes from the blood.

20259. Now, with regard to this bacillus in the intraperitoneal fluid, I take it from your Report that you never find it when the fluid is clear?—You do not find it microscopically in the case of louping-ill; usually if you kill the animal you have great difficulty in identifying the bacillus by mere microscopical examination; but if you incubate the liquid, I have never failed to recover it within a few hours after the incubation has commenced.

20260. Whether the liquid has been taken from a living animal or from a dead animal?—Yes.

20261. But you state also, I think, that you always find it microscopically when the peritoneal fluid presents an opaque appearance?—I do not know that I have ever failed to find it in such cases; and that has been strengthened by later evidence during the last 18 months or so.

20262. But would you not say that even during life, when the intra peritoneal fluid assumes that milky or opaque appearance, pyæmic changes had set up?—No, there is no evidence of pyæmia in the slightest degree.

20263. But when you get the peritoneal fluid of a milky or opaque appearance in ordinary cases of disease, would you not say that pyæmic changes had set up?—Oh, no, not necessarily.

20264. Not if you had febrile symptoms along with the appearance of the disease?—Not necessarily. It might be so. If you had a case of septicœmia to deal with you would very likely find a septic organism growing in the peritoneal fluid after death.

20265. But during life?—Yes, and even during life—in septic peritonitis or peritonitis from any cause.

20266. Then you get a milky or opaque appearance?—Yes, but that of itself would not be sufficient to diagnose what the milky or opaque appearance was due to, it may be due to difference causes.

20267. May I ask what interval of time usually elapses before you receive any of these specimens?—It varies very much; we try to get them as soon after death as possible. A great many cases were received, as I have stated in the case of louping-ill, immediately after death, and, in the cases of braxy also, but with braxy it is more difficult. I think about the longest interval that I remember intervening between the death of the animal and our examination of the carcase was something like a couple of days; I do not think more than that. But I have received braxy cases just as they dropped down dead—within a few minutes after the animal had died.

20268. You admit that all these similar diseases from clinical symptoms or ordinary observations are more or less allied, and it is very difficult to distinguish them?—Sometimes it is extremely difficult. I have known experts in the veterinary profession and among farmers, who have been deceived in cases which they thought they were perfectly certain about.

20269. Does it not sometimes happen that in passing the virus from animal to animal, with the specific bacillus, say, the appearance of that bacillus is very much modified?—I do not think so—not in the case of any of these diseases, at any rate, that we are dealing with at present? It comes out exactly the same as you introduce it. You will find in the peritoneal liquid or in the serus liquid of the inoculated lamb, or in the blood, the same bacillus as that which you inoculated, giving all the reactions of the bacillus that you have inoculated, and more vigorous usually than it was previously.

20270. So that from the appearance of the bacillus, microscopically and culturally, you contend that these diseases are essentially distinct diseases, notwithstanding their similarity?—Yes, and by inoculation.

20271. But in inoculating you state that very often with braxy the animal dies very suddenly?—Yes, it dies very suddenly.

20272. In those cases of sudden death, supposing that a rabbit has been used, could you distinguish that from death caused by, for example, in testing for ptomaines?—I do not know that the two cases are exactly parallel. A ptomaine is a very different thing; it is not a living poison. I do not think you could exactly draw a comparison.

20273. Except that there might be a sudden death of the animal in both cases?—Both may kill the animal very rapidly; but you can introduce the pure spores of any of these diseases mentioned in our Report, and you get the same organism out again in the animal which you inoculate and in a pure condition.

20274. But I am referring to other symptoms. If you inoculate them with the organisms the tissues of the animal will make a cultivating medium always?—Not always, very far from it sometimes.

20275. I mean in dead animals?—Yes, but in louping-ill sometimes you will find the result is the very opposite to what you suppose.

20276. They vary from different conditions of the blood, and so on?—Yes.

20277. It all depends upon that?—Yes.

20278. But you do find, as a rule, that if you inoculate into the peritoneum the bacilli thrive as in a medium culture?—Not always, sometimes. It depends upon the nature of the experiment a great deal and the nature of the organism that you introduce. I will undertake to say that at certain times of the year you can introduce almost any of the organisms in question subcutaneously into sheep, and will hardly kill the animal in a single instance.

20279. Referring again to the instance of louping-ill, that is jumping-ill?—Yes, louping is an old Saxon word: *chorea paralytica ovis.*

20280. You state that so long as the lambs are being suckled as a rule they do not take the disease?—Sometimes they do, but as a rule they do not. I have seen both mother and lamb affected with the same disease.

20281. Does that apply more to braxy? Do lambs suffer from braxy in the suckling period?—Braxy occurs at the time of the year when they have ceased to be suckled.

20282. That would show that lambs are more or less immune, would it not?—Oh, no. Hogs become more or less immune, but the lambs are very far from being immune.

20283. But they do not take it, you mean to say?—They do take it.

20284. What, braxy?—Yes, they take braxy. Not while suckling; they are away from their mothers at that time, weaned.

20285. Would not that be the time to administer your prophylactic? Have you tried feeding lambs with your prophylactic to see what the result may be?—Yes, August is the best month. We have tried all the months of the year, but August we find the best.

20286. At that time, before the lambs are weaned?—Weaning time is most convenient for another reason, because you have to consider the convenience of the farmer. August is most suitable, because he is weaning them, and he then gets them together.

20287. With regard to that lamb to which you refer in your Report, in whose *post-mortem*, if I may say so, you first discovered the habitat of the bacillus, was that a weaned lamb?—No, it was not; it was at the time of year when it was still following the mother.

20288. After the louping-ill?—Yes.

20289. Was it in an emaciated condition?—No.

20290. So that you could not say that it was improperly fed in any way?—Not in the very slightest degree.

20291. Do you think that these diseases are influenced at all by the amount of food which the sheep may be able to obtain at times?—In the case of braxy the better fed they are the more liable they are to it. I do not think that is the case in louping-ill, but in the case of braxy it appears to be the case. It is always the best member of the flock that is affected.

20292. Is there any relation between outbreaks of the disease and very severe wintry weather, for example?—There appears to be a certain relationship, but I do not know whether it is a direct one. It is always said that on a frosty morning animals are more liable to die from braxy. I expect that is more from the deteriorating influence the frosty weather has upon the general condition of the animal. Any change in the weather is said to favour braxy. I have seen the very worst outbreaks, however, in the most beautiful weather; but I think it may be admitted that any sudden change in the weather increases the mortality.

20293. Have you any idea of the incubation period of the disease?—It is almost impossible to say in the case of braxy. From the fact that you very seldom see the animal before it is advanced in the disease, or, at any rate, for some hours after the disease has commenced. But in the case of inoculation experiments the usual period is something like 24 or 25 hours. I expect in natural braxy it may be a little longer.

20294. In the Western Highlands, the South of Scotland, and the North of England, where the disease is more or less prevalent, they have these old sheep-folds, have they not?—Yes.

20295. Do you think that they influence the spread of the disease at all?—Practically on an infected farm they have the disease everywhere.

20296. But these folds are never changed?—Very seldom.

20297. Most of them are made of stone walls?—Yes, they are.

20298. So that during very severe and stormy weather, when the sheep are huddled up together in these pens or folds, you would call those conditions more or less insanitary?—But I do not think they huddle them up in severe weather at all. That is the very time that they do not huddle them up.

20299. But sheep must take shelter in the course of a storm?—No, they do not.

20300. What are the folds for, then?—For gathering in dipping time, mostly.

20301. Not for shelter?—No, the sheep lives out in the open unless it is buried in the snow, and then they dig it out—it is only in dipping time that they are brought in, or for castration, or anything of that sort.

20302. You make some reference in your *précis* to the influence of the Gulf Stream, but are you still of opinion that it can have any influence in the spread of these diseases?—It is quite likely that it may be a fostering influence.

20303. But braxy is also very prevalent in Iceland?—Yes.

20304. And in Norway?—Yes.

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20305. And in certain parts of New Zealand?—I believe so, although I have no direct evidence bearing on the allegation.

20306. So that the Gulf Stream influence is not of general application?—I do not know; it comes pretty close to it. The waters of the Gulf Stream, as I understand, pervade directly or indirectly all round these shores.

20307. Do you think it is felt as far north as Iceland and the coast of Norway?—Indirectly, at any rate.

20308. You also state, I think, that even although the bacilli of these diseases may be found in the intestines, yet when the sheep are moved down into the lowlands, to be fed, say, on turnips or pasture, they never fall with braxy or louping-ill?—I would not say never.

20309. Well, hardly ever, I was going to say?—Much less often.

20310. It is seldom that cases of braxy or louping-ill crop up amongst the sheep when they are being fed on turnips?—Comparatively seldom, but I have seen it.

20311. Of course, you have found that inoculating with the bacilli, the vaccine, in braxy or louping-ill is attended with a considerable amount of risk?—Yes, it is.

20312. On account of the mortality?—Yes.

20313. But that in administering these bacilli by their mouth there is little or no risk at all?—If it is done at the proper time of the year.

20314. Have you made extensive experiments, say, during and before weaning amongst lambs, to ascertain whether that would have any influence upon it then?—That is the very time when it ought to be done, any time up to weaning time. That is the time in which the sheep is immune from practically all these diseases.

20315. I am referring to lambs. Do you feed the lambs with the prophylactic before weaning?—No, we cannot; they are too young, and they cannot be gathered.

20316. Does this prophylactic that you call drenching, scour or produce diarrhœa?—I have seen it produce a little diarrhœa sometimes, but not as a rule.

20317. In the natural disease, in louping-ill, or in braxy, do the sheep suffer from diarrhœa?—Sometimes they do.

20318. Then you would expect a considerable increase of bacilli, such as the coli bacilli, in the intestines if the sheep was suffering from diarrhœa?—Quite likely. It would depend greatly upon what the diarrhœa was due to, of course. The sheep is remarkable in this respect, that its intestine contains so few bacteria of any kind.

20319. I have seen it stated that in the intestines of animals in the Arctic regions no bacilli are discoverable; is that your experience?—I have no experience of this. I do not know. I know from examination of healthy sheep that the amount of bacteria found in the intestine is comparatively slight.

20320. Is not this prophylactic mode of yours unique in the bacteriological treatment of disease. I mean you do not know any other bacteriological disease, or bacterial disease, for which a prophylactic is used in this way?—The old method of drenching is not a new one by any means. It has been applied in veterinary practice for long.

20321. I mean to drench with a preparation or culture of bacillus?—The drenching has usually been performed, so far as I know, with some of the liquids of the body of the animal suffering from the disease, as in pleuro-pneumonia, for instance.

20322. Such method has not been successful?—The method of drenching has been employed for the prevention of pleuro-pneumonia of cattle, for instance, but I do not know that it has been applied to these diseases of sheep, nor in the particular way in which we employed it, by the method that I have described.

20323. Is not this drenching method applied in similar diseases in other animals?—I will not say that drenching has not been a method in use, but it has been applied empirically.

20324. But specific drenching?—I do not think so, but I would not like to be positive on the matter. Of late the alimentary canal has been looked upon as the source from which one can treat several contagious diseases and produce immunity, and much more so than formerly.

20325. In cholera, as of course you have heard, the cholera bacilli can be swallowed with impunity, and have been swallowed with impunity?—Yes; but I would not like to try it.

20326. I think you make reference to the system of preventive treatment followed in bye-gone days of drenching with the dung of pigs fed on the manure of sheep?—No, fed on the same pasture as the sheep.

20327. But do you not state that pigs may feed on the manure?—No, the pasture.

20328. Does that mean pasture covered with the manure of sheep?—Yes.

20329. Was your treatment suggested by that, or was it an induction from your discovery of the habitat of the bacillus in the digestive tract?—We supposed that this pigs' manure treatment of these diseases (which is a thoroughly empirical one), had nothing in it at first; however, the farmers were so positive about its virtues, that we made further inquiry into it, and after we discovered the intestinal habitat of the organisms we were dealing with, the explanation was afforded of how the pigs' manure might act as a prophylactic.

20330. Just because it might contain the bacillus?—That is so; the pig is always put out to feed upon the grass over which the sheep are grazing, the manure is collected after it has been upon the grass. The manure is filled with spores of some kind, most likely the spores which are the cause of the disease in the sheep, and if you administer the manure to the sheep you confer an immunity upon the animal. It seemed quite rational after that explanation was available.

20331. Now the bacillus, you say, was first discovered by Professor Nielsen in Norway?—The bacillus of braxy.

20332. Did he discover it in the peritoneal fluid?—No, not in the peritoneal fluid.

20333. Where?—In other parts of the body. You find it in the blood in some cases.

20334. But according to your experience you find it very seldom in life in the blood of living animals?—You find it pretty often in the blood of the living animal immediately after death.

20335. Did Nielsen also find it in the intestines?—Yes, he did, in the stomach, not in the peritoneal liquid. I do not think he ever examined that.

20336. Have your experiments been repeated by any Norwegian bacteriologists?—I do not know whether they have been repeated, but the Norwegian Government sent a representative over here from their Agricultural Department to interview me, and learn about them. Professor Jensen, also, from Copenhagen, is very much interested in the matter, and I have had a good deal of correspondence with him about it.

20337. But you cannot say that your experiments have been yet confirmed by other investigators?—I do not know.

20338. Have they been disputed?—I have had no evidence of their being disputed or confirmed, either one or the other.

20339. Of course, your inquiry is incomplete still?—Yes.

20340. Now as to the results, I suppose the farmer would look upon what I may call the total general mortality as the index of the healthiness of his flock without differentiating?—Quite so, they take a general view of the total mortality.

20341. In your statistics I see, for example, with regard to braxy, in your first series of 34, you have no deaths at all?—Yes, I think so.

20342. But you have deaths from other diseases—3 or 9 per cent.?—Yes.

20343. These other diseases, including the allied diseases to which you refer as A and B, which you have provisionally so named?—Yes.

20344. As well as malignant œdema?—Yes.

20345. Then in your second series of experiments you drenched 1,545, I think?—I think that is so.

20346. And the deaths from braxy amongst those amounted to 9, or 0.6 per cent., but there were 144 deaths from the other allied diseases or 9.3 per cent.—Yes.

20347. So that there is, even excluding the braxy, still a considerable mortality amongst the flocks?—There was at that time. That was the second year we commenced our operations.

20348. So that from statistics it is sometimes very difficult to obtain what you might call reliable results?—I confess that is the case. One requires either one way or the other to examine the data upon which your statistics are made up with the utmost care, and to repeat the experiment over and over again in order to be perfectly certain.

20349. I think you have also stated that your experiments have not been equally successful on the various farms on which they have been tried; the results have not been equally satisfactory, on some farms they have been satisfactory, and on others they have failed?—On certain farms—on the majority of farms—they have succeeded wonderfully well; on other farms they have been a failure; the mortality has been fairly high—not exceptionlly high, but fairly high—and apparently for a very good reason. The sheep were treated for one disease and they died from something else. We did not know what disease was present on the farm. I now always try to get an inkling of what diseases they have to begin with. Rationally, of course, one would consider that sort of basis at the beginning of the treatment on the farm.

20350. Now, with regard to the prophylactic itself, is it prepared by the Highland Society or by the Board of Agriculture?—It is prepared by myself.

20351. Is it distributed to the farmers free?—We have usually distributed it free until this last year, and, I think, part of the foregoing year (1907 and part of 1906), when we had to make a small charge for it. The applications we had were so numerous that the preparation of it was attended with a fair amount of expense, and we made a small charge for it, which they willingly paid.

20352. Then, I think, you say that these braxy carcases are still eaten with impunity in Scotland?—Yes.

20353. Do you think there is any risk yourself in eating that food?—There is no other animal except the sheep that takes braxy. I do not know that I would like to eat braxy mutton myself, but other people do. They eat it and it does them no harm, apparently. Dogs and other animals eat the carcases with impunity.

20354. But you would not think that there would be the same immunity attaching to eating meat from a tuberculous animal?—I would not like to give an opinion on tht subject at all. That is a matter *sub judice* at present.

20355. It would depend upon the amount of the disease, I suppose?—I do not know. I really would not like to express an opinion upon the subject.

20356. But you yourself have carried out investigations with regard to tuberculosis?—I have.

20357. Do you believe in tuberculin, as a diagnostic agent, as a very reliable agent?—Which tuberculin do you refer to? There are so many nowadays.

20358. The tuberculin used by veterinary surgeons?—You mean in cattle, as a diagnostic in cattle?

20359. Yes?—Certainly.

20360. Did they not find at Aberdeen that when using this tuberculin on tuberculous cows there was no reaction?—There are several circumstances that influence it. If an animal is far advanced in tuberculosis there may be no reaction, whereas a single tubercular gland sometimes will give a marked reaction.

20361. Have you ever tried it on young calves?—In what respect do you mean?

20362. In testing?—I do not know that I have.

20363. Would you expect a young calf to react?—I would expect a young calf to react. Certainly with an animal just out of the calf stage it is done often enough, and it reacts there.

20364. But the calves are free from the disease; they are not, as a rule, tuberculous?—Usually.

20365. Would you expect tuberculin to have a reaction in stall-fed cattle, cattle ready for the butcher?—I do not know that stall-feeding has anything to do with reaction.

20366. But supposing it were tried?—I do not exactly see the point of the question.

20367. Tuberculin I mean to say is exploited as a test for the disease. We know that it cannot be used in fairs or on cattle landed from America at Birkenhead, because they are in an unstable condition, although they may have no tuberculosis, and yet they will react. I want to know whether the experiment has ever been tried of testing stall-fed cattle with tuberculin, which are presumed free from the disease, to see whether those will react?—It is used for stall-fed cattle; they are tested with tuberculin.

20368. I mean cattle fed up for the butcher?—Do you mean a milk cow?

20369. No, any sort of animal, any sort of cow or heifer fed up for the butcher which is presumed not to be tuberculous?—I think so. I do not know why it should not. I have known it react.

20370. Even if the animal is not tuberculous?—No.

20371. So that it is not a very reliable test?—One of the most important points, with American cattle at any rate, is to know whether they have not been tested with tuberculin before you got hold of them. That is a very important point.

20372. (*Sir John McFadyean.*) Dr. Wilson understood you to say that such an animal might react, although it was not tuberculous; is that so?—No. I understood the question to be whether a stall-fed tubercular animal tested with tuberculin would not react. I have seen cases that failed to react, but what the cause of it was I do not know. I have seen a milk cow very far advanced in disease that did not give any reaction at all, or only the very slightest reaction; nothing you could swear by.

20373. (*Dr. Wilson.*) Do you know anything about sheep-pox?—Very little. It is a disease I have not had any experience of.

20374. There has been no prophylactic discovered for that yet?—I do not know.

20375. I am going to read you a passage from the evidence submitted by the late Sir John Simon, who, you know, was Medical Officer to the Local Government Board. This evidence was submitted by him before the last Royal Commission in 1876. Referring to sheep-pox, he says:—"By these experiments on sheep it has been made quite clear that the contagion of sheep-pox is something of which the habits can be studied as the habits of a fern or a moss can be studied, and we look forward to opportunities of thus studying the contagion outside the body which it infects." Has any advance been made since that evidence was given?—Do you mean in relation to sheep-pox?

20376. Yes?—I cannot tell you. I would not like to commit myself to any statement in the matter.

20377. (*Sir William Collins.*) Is there any organism recognised as the cause of sheep-pox now?—I do not know. I do not think so.

20378. (*Dr. Wilson.*) No more than there is in small-pox?—No.

FORTY-FIFTH DAY.

Wednesday, 18th December 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. G. WILSON, M.D., LL.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Sir GEORGE KEKEWICH, K.C.B., M.P., called in; and Examined.

Sir G. Kekewich, K.C.B., M.P.
18 Dec. 1907.

20380. (*Chairman.*) I believe you are a Member of Parliament, and Honorary Secretary of the Parliamentary Association for the Abolition of Vivisection?—I am.

20381. And you come here to represent the views of that Association at their request?—I do.

20382. I believe you do not profess to have any expert knowledge of either medical or surgical matters?—I have only the knowledge that an ordinary man of common-sense, I suppose, can obtain from reading. I have no medical knowledge.

20383. That is what I meant to convey by the word expert?—Quite so.

20384. (*Chairman.*) I shall ask Sir William Collins to be good enough to ask you the questions which arise upon your *précis* which you have sent in; he has been good enough to say that he will do so.

20385. (*Sir William Collins.*) At the request of the noble Chairman, I propose to take you through the *précis* with which you have furnished the Commission. I am sure that I shall not do it so effectively as his Lordship, but I will endeavour to obey the direction of the Chair. May I ask what is the object of the Association on whose behalf you appear?—The object of the Association is the total abolition of vivisection.

20386. What is the name of the Association?—The Incorporated Parliamentary Association for the Abolition of Vivisection. It is called the Parliamentary Association because its vice-presidents consist exclusively of Members of Parliament of the two Houses, and they include a great number.

20387. On what ground do they advocate the abolition of experiments on animals?—They advocate it on the ground that, first of all, they regard them as inhuman, immoral, and degrading, and a disgrace to civilisation; and, further, they consider that the experiments which have been made under the present Act, and which have been carried out at the expense of so much torture and so much animal life, have been practically unproductive of any beneficial result to the human race.

20388. Would not the latter statement involve some medical knowledge?—From a certain point of view it will; but I think that any man of ordinary powers of observation and common-sense can judge what results have been obtained from the experiments and researches which have been carried out.

20389. Have you considered the mode in which the Cruelty to Animals Act, 1876, was passed?—Yes. Its motive power was the Report of the Royal Commission on Vivisection, which sat in 1875, and was appointed in consequence of a strong public agitation on the subject. Horrible cruelties were reported to have taken place on the Continent, and vivisection was also in a less degree in vogue in this country. The fact that M. Magnan, a French physiologist, had performed the experiment of injecting alcohol and absinthe into the veins of dogs before a medical congress at Norwich showed that there was danger that the medical profession in England might become generally permeated by the vivisection propaganda. M. Magnan was prosecuted under the then existing law, and left the country.

20390. What was the date of that prosecution?—I think it was 1874.

20391. And under what Act then was the prosecution?—Under the Prevention of Cruelty to Animals Act, an Act that was known as Martin's Act.

20392. What was the result of the prosecution?—The result of the prosecution was nothing, because he left the country, so that I believe no conviction was obtained, but I have not been able to find out exactly what happened.

20393. (*Mr. Ram.*) Did he stand his trial?—I think not.

20394. (*Sir William Collins.*) It was a threatened prosecution only?—I cannot quite make out from the records on the subject what happened, but, anyhow, he left the country, and so no decision was arrived at.

20395. Then as the result of the deliberations and Report of the Royal Commission of 1875, was a Bill introduced into Parliament?—A Bill (now the Cruelty to Animals Act, 1876) was introduced in 1876 by Lord Carnarvon into the House of Lords, and read a second time, after debate, on May 22nd. In the course of his speech he said that under the then existing law only domestic animals were protected, that as to vivisection the protection afforded to those animals was insufficient, and that for animals which were not domestic there was no protection at all. I wish to call the attention of the Commission to the fact that Lord Carnarvon introduced this Bill as a Bill to afford further protection to animals. He said further that the Bill was intended to prohibit in express terms and as a general principle the practice of vivisection, but it allowed exceptions, for certain purposes of teaching, with anæsthetics, and for purposes of research, even without anæsthetics, but only in the most rare cases. Those were his words.

20396. Were any representations made to the Government by the medical profession at that time?—Yes; the Committee stage of the Bill was deferred until June 20th, in consequence of the action taken by the medical profession. Some hundreds of medical men attended a deputation at the Home Office in order to present a memorial with some 3,000 signatures, and the consequence was that, so far as the Bill was amended in passing through Parliament, it was altered in accordance with their views. I have been unable to discover from Hansard, which I have looked at, at

what particular point in the Bill the amendments took place. There was one amendment that was introduced in the House of Commons, which, I think, was in the interests of the medical profession, to the effect that no prosecution should be instituted without the consent of the Secretary of State.

20397. The Bill was brought into the House of Commons in August, was it not?—Yes, the Bill was brought into the House on August 10th; it passed through all its stages by August 15th, on which day it received the Royal assent, and Parliament was prorogued. I wish to call the attention of the Commission to the fact that the whole consideration given to this Bill in the House of Commons was five days; it passed through all its stages in five days. And I think there is no Member of Parliament who will not tell the Commission that that was a totally inadequate consideration for a Bill of this importance and character.

20398. Did the discussion occupy any considerable portion of those five days?—No, it did not, so far as I remember. There was a debate on Second Reading; there was a little said on the Committee stage, and the last stage passed through without any discussion at all; the fact being, of course, that it was a wearied House at the end of the Session; and as you, Sir William, know very well, members are very glad to get rid of all these measures as fast as they can in order to get away for the prorogation. What I wanted to point out was that a measure so passed through the House of Commons has not really, although it has technically, the same authority as a measure which has received full consideration from the House, and has been fully debated throughout all its stages.

20399. Perhaps you will state the objections that your society entertains to the Act?—We object to it because the principle of the Act, namely, the prevention of pain and cruelty, which is contained, I think, in Section 1, is entirely nullified by the exceptions allowed on certificates given by the Home Secretary.

20400. Is the certificate given by the Home Secretary? We have been told, I think, by previous witnesses, that it is open to the Secretary of State to disallow a certificate, but the certificate is actually signed by other authorities?—I have no doubt that it is so. From my own experience in the Civil Service I know that that kind of routine work is always delegated by the Minister to other people.

20401. Is it a matter of delegation to those within his own office, or is it that he recognises the action of other persons, such as Royal Societies, in granting a certificate, and under the Act the power is reserved to the Home Secretary of disallowing that certificate if he sees reason?—I believe that is so.

20402. Will you proceed with your objections to the process of certification under the Act?—We think that the Act has been practically powerless to stop either pain or cruelty. The Act provides that the animal experimented on must be anæsthetised, and that it must be killed before recovering from the anæthetic. But if the Home Secretary gives a certificate anæsthetics need not be used, nor need the animal be killed before recovery. The "most rare cases" referred to by Lord Carnarvon (that was cases for purposes of research even without anæsthetics) developed into 379 certificates in 1906, and there were also issued 88 certificates for performing experiments on cats and dogs. It may be added that no less than 256 certificates were held by medical men, dispensing with the obligation to kill the animal before recovery, and 118 certificates dispensing with the obligation to kill cats and dogs before recovery.

20403. Are the 88 certificates for performing experiments on cats and dogs included in the 379 certificates?—I understand that they are not.

20404. They are in addition, are they?—Yes, in addition.

20405. Do you suggest that these were all painful experiments?—I do not quite understand how any experiment can be other than a painful experiment, either *in initio* or in its subsequent results.

20406. Would that remark apply to cases of experiments conducted under anæsthetics in which the animal is destroyed before it recovers from the anæsthetic?—I should answer to that that we have no guarantee that in all cases the anæsthesia is perfect. It may be that the animal is completely anæsthetised in many cases, and consequently the animal does not feel any pain during the experiment. On the other hand we have no guarantee that the process of anæsthetisation is complete.

20407. Possibly you may be asked questions upon that in cross-examination. Will you proceed to state your objections to the Act in the matter of experiments performed in illustration of lectures?—The Act lays down that experiments are not to be performed as an illustration of lectures, and then proceeds to nullify this provision by further provisos. I should like to say here that the form of the Act is what I should call absurd. It seems perfectly ridiculous first of all to insert a clause saying one thing, and then to nullify the whole of that clause by provisos; for I think that every one of the restrictions in Section 3 is practically nullified by the provisos attached to that section. I have never seen any other Act of Parliament in that form myself.

20408. I think you have already stated that you object to the provision whereby a prosecution against a licensed person shall not be instituted, except with the consent in writing of the Secretary of State?—We think that bears out our contention that this Act instead of being an Act for the protection of the animals, is an Act for the protection of the medical men who carry out the experiments.

20409. Did the subsequent passing of the Public Authorities Act, 1898, affect the question of prosecutions under the Act of 1876?—Yes, Section 1 (A) of the Public Authorities Act, 1898, renders it illegal for a prosecution to be instituted more than six months after the offence, and therefore prosecutions have become impossible. The only means of discovering illegal proceedings under the Act is the Report of the Home Office Inspector, and his reports are never issued until the matter reported on is at least six months old. No doubt the framers of the Public Authorities Act had not this result in view (I do not think they had this particular Act in view at all), but such is the effect of the provision. In answer to a question I asked the Home Secretary on the subject in the House of Commons on November 13th, 1906, he said that he could not issue the Report quarterly (it is now issued half-yearly) because of the immense mass of documents that had to be received and examined, and because it would very greatly increase the labour involved, with little or no advantage. He added that contraventions of the Act were reported to the Home Secretary as soon as they were discovered, so that the institution of proceedings for performance of illegal experiments was not dependent on the date of the Report. That would be well enough if the Home Secretary would institute prosecutions himself, or give to others the information enabling them to do so. But I am not aware of any case in which he has adopted either course.

20410. Do you think that the Act has operated in the direction of the restriction of vivisection?—No; I think it has acted as a great stimulus to vivisection. The proof of that is the number of experiments, which has risen from 481 in 1878, which was two years after the passing of the Act, to 43,287 in 1906.

20410A. Our attention has been called to the fact that a considerable number of such experiments are of the nature of inoculations, involving merely the prick of a needle. Have you anything to say in regard to that?—I am aware that that is the case, but I am not aware that inoculation does not lead to painful and undesirable consequences. I have often heard that if you inoculate with a bit of pus, say under the tongue, you may produce very great pain and a great deal of trouble. An instance, which I deal with later, is the inoculation for plague with Haffkine's serum.

20411. Have your Association embodied their views in a Bill?—They have, but I believe that Bill has not been furnished to the Commission.

20412. I think we have not had it?—No; you have not had it. The reason being because we find it extremely difficult to draft a Bill, and therefore I should like to eliminate that, if I may, from my *précis* altogether.

20413. Do you desire, on behalf of the Parliamentary Association for the Abolition of Vivisection, to give us any specific recommendation as to the form of a Bill for carrying out their object?—We could do that later. But we are rather of opinion that the time is premature for framing a Bill. We think we had

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better delay it until after this Commission has issued its final report, and then we shall introduce a Bill into Parliament expressing our own views on the subject.

20414. So that at the present moment the views of the Association are not embodied in any Bill?—We have not yet embodied them in any Bill.

20415. Do you desire to call attention to certain omissions and discrepancies which, in your opinion, demand the attention of the Commission?—Yes; to some which have characterised the evidence before the Royal Commission. I want to point out that such evidence, far from relieving the position, on the contrary greatly aggravates the widespread and daily-increasing disquietude in the public mind, as regards the suffering inflicted upon animals, and the hopeless confusion and anomalies of present-day medical science. The Commission will understand that I am representing experts although I am not an expert myself.

20416. Is that the opinion of the Parliamentary Association?—It is the opinion of the Parliamentary Association.

20417. (*Sir Mackenzie Chalmers.*) I do not quite understand, do you represent the experts in your Association?—So far as my *précis* goes I represent the opinions of persons who are experts in the sense that they belong to the medical profession. I do not say that they are experts in the sense that they have taken part in experimental research or vivisection, except, perhaps, in their student days in hospitals.

20418. (*Sir William Collins.*) You desire to suggest certain reasons for alleging this increasing public disquietude, I understand?—We wish to say that in consequence of the increasing public disquietude we have been enabled to add recently, since the publication of the Blue Books detailing the evidence supplied to this Commission, no less than 29 members of Parliament to the list of our vice-presidents, and we have received from a number of other members such expressions of interested sympathy as experience has shown are prefatory to their becoming staunch opponents of the practice of vivisection.

20419. Then while not volunteering medical opinions yourself, do you desire to make some statements in regard to medical science?—I wish to confine my remarks to the important subjects of the infliction of pain, anæsthesia and anæsthetics, vivisections and demonstrations before students. These, as I consider, being subjects with which any man of common intelligence is qualified to deal, and, moreover, subjects which are of the most vital interest to the members of the Association which I represent, and to the general public, bearing as they do upon the systematic infliction by men in high and responsible positions of pain and considerable suffering upon helpless creatures, and also (I think this is a most important point) upon the demoralisation of the young men and women who are the future physicians and surgeons at whose hands the community will presently benefit or suffer.

20420. Do you suggest that such practices have a detrimental effect upon the humane sense and morality?—Yes. I would remind the Commissioners that the development in man of the humane sense has been the slow growth and higher moral awakening of many millions of years of evolution, and that, should they favour or further facilitate such demonstrations before students they will, by rendering them callous to the sight of pain and mutilation inflicted upon living, intelligent creatures, incur serious risk of blighting that humane sense which is the very highest characteristic of civilised men, and is, beyond all others, the faculty most needed in those who will one day have the lives and welfare of their fellows in their trust.

20421. It has been suggested to the Commission that the practice of vivisection under the Act would have no more demoralising effect upon students than witnessing surgical operations under anæsthesia. What do you say with regard to that?—I think undoubtedly it would. I think the witnessing of a series of experiments by students upon living animals undoubtedly renders them more callous to suffering than if they witnessed an operation or operations which are done for the benefit of the person undergoing the operation.

20422. Would your objection and your fears in regard to the lowering of the moral sense be met if you could be assured that vivisection was conducted entirely painlessly?—No, I do not think it would. I think the whole sight is degrading and immoral—the sight of the animals strapped down and cut to pieces I do not know that I have anything more to say on that point.

20423. Do you suggest that the same advantageous results which have been claimed for vivisection could have been obtained by some other mode?—By clinical research.

20424. Do you desire to call attention to what you mention in your *précis* as to the barren state of medical knowledge?—Yes; I think that it is obvious that the present state of medical knowledge is barren. We see disease and degeneracy on all sides, against which it is equally obvious that medical art is powerless, or they would cease to exist. To take a single instance, since the year 1729 influenza in various forms and of varying degrees of severity has been an ever-recurring scourge. Now it is perennial, even endemic. Yet our medical scientists cannot give us one satisfactory explanation of its origin or of its prevention; nor, if we judge by its constitutional sequelæ and its mortality, can we suppose that they know much about its treatment. Yet they have been supplied with abundant material, not only animal, but also human, for observations with a view to the discovery of means to rid us of this scourge. We are, however, apparently no whit nearer ridding ourselves of it than we ever have been. Here is a practical, everyday test, and surely, also a convincing evidence of the barren state of present medical knowledge.

20425. Do you suggest that medical science has remained stationary during the last 30 years?—I should say that it has not made any great advance. I have seen it stated that we know little more than we did in the days of Galen and Hippocrates as regards medical science. I do not go so far as that.

20426. You say that you have seen it stated. May we take it that the view which you have put forward is that entertained by the Parliamentary Association?

20427. (*Sir Mackenzie Chalmers.*) And its experts?—Yes. I was going to say (I happen to know something about it from home knowledge) that I have heard medical men say that so far as remedies are concerned there are constantly new remedies, such as antipyrin and sulphonal, which were at one time ushered in with a great flourish of trumpets, but which, I think, are not any longer in general use; and, as a matter of fact, many medical men, I think, are really limiting themselves to a very small number of efficient remedies.

20428. (*Sir William Collins.*) I understand that you desire to call attention to certain points in the evidence which has been laid before the Commission. Of course the Commission has an opportunity of reading that evidence, but you may desire to direct attention to one or two particular points?—Yes. As regards the infliction of pain, inasmuch as obviously the opponents of the practice are not in a position to obtain experiences, it is necessary to rely upon the admissions and representations of vivisectors themselves. For this reason, their contradictions and admissions are most valuable, as warning the Commissioners and the public that the evidence as to the pain suffered by animals in process of mutilation is wholly untrustworthy and unreliable, either because of the inaccuracy of statement or the inaccuracy of observation of the operators, or because, under the Act as at present carried out one operator actually experiments without pain while another confesses that he has done painful experiments, and indeed, adds that he is not ashamed of the fact. Professor Starling (Question 3451, First Report) commits himself to the bold and sweeping statement: "Though I have been engaged in the experimental pursuit of physiology for the last 17 years, on no occasion have I ever seen pain inflicted in any experiment on a dog or cat; or, I might add, a rabbit, in a physiological laboratory in this country, and my testimony would be borne out by that of anyone engaged in experimental work in this country." Professor Cushney says (Question 4932, Second Report): "I am quite satisfied that no animal suffered pain under my experiments." (Question 4962) "I should say that the statement that cruelty occurs in laboratories in England at the present time is grounded on misconception, and is false."

20429. Have you any reason for disputing these statements?—As a mere question of pain I do not see that there is anything illegal in the causation of pain because the operator may hold a certificate enabling him to dispense with anæsthetics.

20430. (*Chairman.*) That is not the statement that you make. The statement that you object to of Professor Starling's I understand is that he has not caused pain to animals in his experiments?—Yes.

20431. I understood Sir William Collins to ask you whether you dispute that statement?—Yes.

20432. (*Sir William Collins.*) And that the testimony would be borne out by anyone engaged in experimental work in this country?—He says so.

20433. Do you desire to suggest any reason for doubting that?—I do not know enough about the persons engaged in experimental work, but I should rather doubt whether they could honestly say that. Dr. Thane, an inspector under the Act (Question 450, First Report), says: "I have not seen nor have I ever heard of any operation—a procedure that was seriously painful—being performed without anæsthesia." I do not know whether Dr. Thane's experience is large. He is an inspector under the Act, but I understand that last year he was only present at 15 experiments, and I understand that the whole of those experiments were in Sir Victor Horsley's laboratory; he did not go any further than that.

20434. Do you suggest that Dr. Thane did not inspect last year any vivisections under the Act except those performed by Sir Victor Horsley?—Yes, I understand that to be the case.

20435. What is the source of that information?—I understand that Dr. Thane said that he had inspected 15 experiments, and that Sir Victor Horsley stated that Dr. Thane had inspected his laboratory 15 times.

20436. (*Chairman.*) What question is that?—I have not the number, but I understand that in Dr. Thane's own evidence he said that he had only witnessed 15 experiments altogether, so that if he had inspected Sir Victor Horsley's laboratory 15 times and had only witnessed 15 experiments altogether it is obvious that all those 15 experiments were in Sir Victor Horsley's laboratory and nowhere else.

20437. (*Sir William Collins.*) Would it not be possible for an inspector to pay an inspection visit to a laboratory and not to witness an experiment in the course of his procedure?—That is so, of course, but I should have thought that those visits were of very little use.

20438. That is the ground for the statement you have made?—That is the ground.

20439. Is there any other point in Dr. Thane's evidence to which you desire to call attention?—Yes, he later admits that, on the other hand, "In some cases of this group (that is, experiments performed under Certificate A) the infection or injection is followed by great pain and suffering. I may mention the injection of tetanus toxin and the infection with plague; also the insertion of certain drugs." "The injection of tetanus toxin," he states, "will produce tetanus, which . . . is manifested by convulsions which are of a painful nature." "Injection with plague makes the animals very ill and makes them look miserable." "It is obviously impossible to say to what extent mice, in which such tumours (implanted cancer) are growing, are suffering pain, but observation of their behaviour and habits does not justify the assumption that, so long, at all events, as the tumour is of moderate size, they are suffering acutely or severely." Nevertheless, the facts and the Inspector's evasive way of stating them justify the assumption that these creatures may suffer acutely or severely according to their own humble powers of bearing pain, whether or not the tumour remain of moderate size, and we are not told of means taken to restrict the growth. When asked, may the result of the operation be painful, although the operation itself is performed under an anæsthetic? (that is in the case of renewed operations) "That comes on," he replies, "two paragraphs further on;" and later says, "When in operations under these certificates the operation has been performed and the wound has healed, it does not follow that the animal remains in a state of pain or suffering." Nobody, of course, would suppose that after the healing of the wound it necessarily follows that the animal remains in a state of pain or suffering. But he carefully ignores the truth that in extensive cutting operations with excision of organs there must necessarily, upon waking from anæsthesia, be very severe and prolonged pain, until, that is, the severed tissues heal. He reiterates that the operation is conducted aseptically, which may hasten the closing of the wound, but cannot certainly diminish sensation and the degree of suffering which is inseparable from extensive cutting operations. Then Professor Pembrey has made some admissions.

20440. Do you desire to call attention to them?—The freemasonry of reserve and evasion as to the fact and the amount of suffering inflicted is candidly broken by the very frank and damning admissions of Professor Pembrey, Professor of Physiology at Guy's Hospital. He says (Question 14084):—"I confess that I have done painful experiments, and I am not ashamed of admitting it. They are absolutely necessary. I want to show that pain is part of the scheme of nature, and that we must recognise its existence." He believes (Question 14093) "that a recognised physiologist should be given a licence to cover all experiments." He was asked whether that licence should be given for experiments without anæsthetics, and he said, "Without anæsthetics or with anæsthetics and without certificates." He believes that the Vivisection Act is entirely opposed to the advancement of physiology. Further, he expresses the opinion that the infliction of suffering is a meritorious act. Then, at Question 14147: "I consider," he says, "that it is perfectly right to inflict pain upon animals." Then there was a question asked: "Not unnecessarily?" and he said, "No." In answer to another question, he says: "I will be perfectly straightforward. I say that you should not inflict pain if you can obtain the knowledge in any other way, but I say that even where there is an operation the pain there is of a protective nature; it may produce syncope, and therefore less sensation of pain." I presume that a knock on the head will do the same thing. This suggests that such an extreme degree of suffering should be inflicted as to cause the unfortunate creature under operation to become insensible, in order that while it is in this condition of insensibility a further operation may be done. "Further," he continues, "I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of syncope. . . . If you give them anæsthetics you are introducing a complication which you could remove, and therefore without anæsthetics you actually save life and actually diminish the infliction of pain." That seemed to us to be a very extraordinary doctrine. By these admissions we get at the truth. Professor Pembrey has, at all events, the courage of his convictions. He has done "painful experiments, and is not ashamed to admit it." He considers it "perfectly right to inflict pain upon animals." "Painful experiments," he adds with conviction, "are absolutely necessary." "Pain is part of the scheme of nature." It is "protective" and "beneficent." He is right, no doubt, in that pain is protective when it calls the sufferer's attention to the presence of an injury, and also in rousing the reparative processes to make good that injury. But it is difficult to trace the protective character of pain inflicted under vivisection, as it will certainly not protect a creature, clamped powerless upon a board, from further investigations by an operator. And surely only the strangest perversion of mind can find anything "beneficent" in the infliction of suffering by intelligent man upon a helpless creature of lesser intelligence.

20441. If I remember rightly, some of the painful experiments admitted in this country by Professor Pembrey under licence had to deal with the destruction of rats by sulphur dioxide with a view to disinfect ships. Do you suggest that it is undesirable that experiments of that character upon rats should be performed with a view to prevent the propagation of plague?—I do not quite understand why painful experiments should be performed under those circumstances. I have not sufficient medical knowledge to know whether it is possible to carry out such an experiment as that without pain.

20442. I suppose that would be dealing with two different respirable gases, the anæsthetic on the one hand and the sulphur dioxide on the other?

20443. (*Chairman.*) If you are going to kill rats in a ship you could not first catch the rats and anæsthetise them, and then proceed to poison them with gas. You must do it at once?—This, I imagine, was a question of the suffocation of rats, was it not?

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20444. (*Sir William Collins.*) Yes. May I ask if the alleged fact that sulphur dioxide is a good mode of destroying rats with a view to prevent the propagation of plague had been discovered by other than vivisectional means, you would object to its use for that purpose?—I am met by the fact that I do not understand how suffocation comes within vivisection. I thought vivisection was cutting.

20445. You think that these experiments might be performed without being brought under the Act at all?—I should imagine so.

20446. The term vivisection is not, I think, defined in the Act?—I do not think it is, but I was speaking really of its obvious meaning—vivisection is the cutting of living animals.

20447. Does your Association distinguish between experiments upon living animals of a cutting character and painful experiments upon living animals such as the administration of suffocating gases?—I think we should oppose both, but, on the other hand, I do not know that anything except cutting experiments comes under this Act. The words of the Act are: "A person shall not perform on any living animal an experiment calculated to give pain." I do not know whether this particular experiment you speak of does give pain.

(*Sir Mackenzie Chalmers.*) Undoubtedly, if you have read his evidence.

20448. (*Sir William Collins.*) I thought you were alluding to Dr. Pembrey's evidence to show that he had conducted painful experiments?—I am not dealing with his experiments specifically; I am dealing with his general statements, where he says that painful experiments are absolutely necessary, that pain is part of the scheme of nature, that it is protective and beneficent, and all that kind of thing.

20449. I understand that you desire to draw attention to what you conceive to be a discrepancy between the evidence of Professor Pembrey and Professor Starling?—Professor Pembrey, in direct contradiction to Professor Starling and others, tells us that he does painful operations, and that, moreover, "they are absolutely necessary." Thereupon we are confronted by the question: Were those other gentlemen, then, imposing upon our credulity, or were they the victims of their own? Can extensive mutilations under anæsthesia administered, as Professor Starling confessed, by the laboratory boy, be wholly and invariably painless? Painful experiments, he says, are "absolutely necessary." This, then, is the candid truth of the matter. Vivisection is necessarily, that is, unavoidably, a painful—possibly a gruesomely painful—process. Some notion of the degree of that pain, and of the callousness of the operator, is indicated by Professor Pembrey's recommendation that anæsthetics should be dispensed with, and that the animal should be subjected to suffering so severe as to cause insensibility in order to avoid the "complications" attending the administration of an anæsthetic. That, as I understand, means that he advocates the infliction of pain of such severity as to make the animal collapse under the pain. That hardly seems consistent with the principle of the Act that anæsthetics should be administered. Professor J. N. Langley, F.R.S., says (Question 15251), "The degree of pain is, of course, always a relative thing. . . . I have indicated the difference, so far as I could, by saying that some of my experiments were clearly not painful, and some of them were of the nature of a severe operation in a hospital, and those I should think, possibly, at times do cause pain, as they do in a human being." I should have thought that they always did.

20450. We have frequently had our attention directed to Professor Klein's evidence before the last Commission?—No doubt. When he gave evidence before the last Royal Commission in 1875 he was asked at Question 3539, "Then for your own purposes you disregard entirely the question of the suffering of the animal in performing a painful experiment." His answer was, "I do," and he is still employed under the Local Government Board. Further indications of the blunting effects of vivisection upon the humanity of the operator are supplied by Professor Pembrey's observations upon the relief of human pain by anæsthetics. At Question 14119 he is asked, "Did I rightly gather that in your opinion anæsthetics are unnecessary," and he said, "Yes. I am convinced that they are often unnecessary, not only in the case of animals, but of men." And at Question 14120 he is asked, "For vivisection and for surgical operations?" and he answered, "For vivisection and for surgical operations, and I mentioned the case of midwifery." He condemns anæsthesia for women in childbirth (the suffering during which, it is acknowledged, is atrocious), and himself circumcises children without anæsthetics. "If they cry," he says, "it does not matter; it is much better that a child should cry and moan than that its life should be lost; the introduction of anæsthetics has done a considerable amount of harm." Professor Pembrey tells us that he has a class of 70 or 80 fresh students every year. We may judge for ourselves the moral effects upon these students of their professor's doctrines, and of the sight of the painful operations to which he confesses.

20451. Are you not rather ignoring the extent to which we are told that anæsthetics are employed in cases of experiments upon living animals?—As I understand, the point here is that you have a man in a very high position in a hospital who actually advocates the dispensing with anæsthetics altogether.

20452. I thought you were going to give us some reason why you doubted the assertion that anæsthetics are so largely employed in the case of experiments on living animals?—We have no absolute proof that anæsthetics are ever used, because no anti-vivisection doctor has ever been allowed to witness any experiment.

20453. (*Chairman.*) Then nobody is to be believed on his oath except an anti-vivisection doctor. We have had plenty of people, not speaking on their oath no doubt, but do you say that nobody's word is to be taken for a fact of that description unless he is an anti-vivisection doctor?—No.

20454. That is what you stated just now; "we have no absolute proof." That is to say that if 20 people came and said that they were present at an operation and that anæsthetics were used, and that the animal was perfectly quiet and still, and showed no sign of pain, and was not strapped down even, nevertheless you would disbelieve them because there was not an anti-vivisection doctor among them?—We should say that we want to send a competent witness from our own side.

20455. (*Mr. Ram.*) To be present on every occasion?—I do not know that on any occasion——

20456. On every occasion, I said?—But I do say that up to the present time, so far as I am aware, there has been no occasion on which any man of that kind has been allowed to be present—a person like Dr. Hadwen, for instance.

20457. (*Sir William Collins.*) Did not Professor Starling offer to admit other persons to his laboratory than those who are at present authorised to attend?—Yes, and I should like to say, partly in corroboration of what I have said, that anæsthetics are not always used in the case of painful operations when they might be, that I notice that in some of the evidence that I saw, given before this Commission, Colonel Lawrie said that he had seen the painful operation of dissecting the neck in order to establish cross circulation done without any anæsthetic whatever, but I observe that in his evidence he did not say where it was done, and he did not say whether the operator held a certificate enabling him to dispense with anæsthetics.

20458. (*Dr. Gaskell.*) Do you say that he saw it done without any anæsthetic whatever?—Yes.

20459. (*Mr. Ram.*) Can you give us the reference?—Yes, I have the reference here.

20460. (*Sir William Collins.*) Was not morphia administered in those cases?—Yes, but I understand that morphia is not an anæsthetic.

(*Chairman.*) It might have been worth mentioning that it was given.

20461. (*Dr. Gaskell.*) When you say without any anæsthetic whatever you mean with morphia, and that you do not consider morphia to be an anæsthetic. If that is what you mean, it is hardly the same thing as saying without any anæsthetic?—Yes, but Colonel Lawrie said that the operation was performed with a small dose of morphia only; he does not say with a fatal dose of morphia.

20462. What was the operation?—It was the operation of tracheotomy. He was asked, at Question 16816: "What was done; was tracheotomy performed without chloroform?—(A.) The whole dissection was performed without chloroform." Then he was asked: "Do you say it was performed without anything in

the way of an anæsthetic?" And he answered: "It was performed with a small dose of morphia." He was asked: "What was the operation?—(A.) What is called a cross-circulation experiment. The necks of two animals are dissected and the large blood vessels cross-connected." Then he was asked, What animals they were, and he said dogs. Then he was asked: "But it was under an anæsthetic?" And he answered: "I do not call morphia an anæsthetic." I think that has been supported by other evidence, I think by Professor Hobday, if I recollect rightly, of the Veterinary College.

20463. (*Sir William Collins.*) Do you desire on behalf of your Association to make any suggestions as to the admission of persons representing anti-vivisection views into physiological laboratories?—Dr. Starling said that he would admit any Member of Parliament or layman, but not "any" doctor.

20464. (*Mr. Ram.*) It was not that he would not admit "any" doctor, but not every doctor?—I understand that no doctor holding views opposed to vivisection has ever been admitted to witness these experiments. And then further on Professor Starling said that no person would be admitted to a physiology class who came merely from curiosity. I do not know whether he would admit me.

20465. (*Sir William Collins.*) Seeing that your Association advocates the entire abolition of the practice I suppose it is idle to ask you whether an increase of inspectors would in any way meet your objections?—No, I do not think that any increase of inspectors would meet our objections.

20466. Even anti-vivisection inspectors?—Even anti-vivisection inspectors, because we are opposed to the practice altogether. The only object, so far as I understand it, of the appointment of what you would and what we would call anti-vivisection inspectors would be to see that the operations were carried out in accordance with the certificates, that is to say, that under a certificate allowing experiments on animals with anæsthetics, the anæsthetics were used and that complete insensibility was effected; and, further, that if the holder of the certificate was enabled to dispense with anæsthetics altogether to see (I do not know what he would see then) that the experiment was carried out in accordance with the certificate. But we are opposed on the ground of its inhumanity and inutility to the whole practice of vivisection. That is our position.

20467. If the Act were not abolished but modifications were made would your Parliamentary Association desire that the number of inspectors should be increased, and that provision should be made for the representation of those who are sceptical on the subject of vivisection on the inspectorate?—I do not quite understand what you mean by their being sceptical on the subject of vivisection. The only object, as I said before, would be to see that the experiments were painless. Of course our Association would prefer that to the existing practice, but we hold that the Act should be repealed and another Act passed in its place, abolishing the whole practice of vivisection of animals.

20468. Does your Association think that the result of the entire abolition of the practice in this country would be to reduce the totality of animals submitted to vivisection. It has been suggested to us that if abolition were decreed either the practice might be carried on surreptitiously or that those who were desirous of practising it would be driven abroad where there would be no, or fewer, restrictions upon their conduct?—I do not say that I am not concerned with what takes place abroad. I view it with as great horror as the English practices, but, after all, what takes place abroad is not our business; we have to look after our own country. And as for a surreptitious performance of vivisection, under those circumstances I apprehend that, if the penalties were large enough, there would not be much surreptitious practice of vivisection.

20469. (*Sir Mackenzie Chalmers.*) You would prohibit it under penalty; you would not leave it to the ordinary law of cruelty?—No, I should not be satisfied, for the reasons that Lord Carnarvon gave in introducing this Bill, with a mere repeal of this Act. I would not be satisfied without the passing of a fresh measure prohibiting the practice of vivisection altogether.

20470. (*Sir William Collins.*) Would you put further restrictions upon the submission of living animals to experiment for scientific purposes than exist in regard to cruelty to animals arising from other than scientific investigations?—No, I think I would bring them all under one and the same Act. For instance, now, I believe that if a horse has lockjaw, and it is not killed immediately, that is punishable under the Cruelty to Animals Act, and is dealt with in the ordinary way by the magistrates, and the horse is directed to be killed. If an experimenter produced artificial lockjaw on a horse, I should inflict precisely the same penalty upon him, but if he, as they do now, produced artificial lockjaw in a horse, and kept that horse alive as long as he possibly could for observation, I should inflict a heavier penalty in proportion to the degree of cruelty. May I add that Miss Lind-af-Hageby, who is the only anti-vivisectionist who has actually attended advanced classes in England (so far as I am aware), affirms that "the most unsatisfactory and obviously unreliable methods of anæsthetising are in vogue in the vivisectional laboratories." She makes that statement on page 159 of a book she calls (not very complimentarily) "The Shambles of Science."

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20471. I understand that you desire to draw attention to experiments upon living animals, with a view to the prevention of plague?—In the examination of Mr. C. J. Martin, Director of the Lister Institute of Preventive Medicine, and a member of the Advisory Committee for the Investigation of Plague in India, he was asked by you whether he thought Haffkine's vaccine, or any other vaccine, in India is having a valuable influence in checking the spread of plague, and he replied, at Question 12300, "Very valuable." I should like, in passing, to call attention to the word "vaccine." I do not understand that the plague serum is vaccine at all; it does not come from the cow or calf. It is mere plague serum, and induces simply a modified attack of plague. You then asked him whether plague has ever been more prevalent in India in the memory of man than during the past year, and he answered, "Not that I am aware of." I wish to lay this matter somewhat more fully before you as a grave national responsibility. The point which brings the question of the plague in India under the consideration of the Parliamentary Association is the manner in which the serum used for the inoculations is prepared and tested; that is to say, the fact that it is prepared by inoculation of animals. W. Symners, of Cairo, gives a record of experiments made to determine the therapeutic efficacy of plague serum (Central b.f. Bakt., April 15th, 1899) carried out at the Serum Institute of Abbasieh, in Egypt. Cultures were made from the bacilli of bubonic plague obtained from Bombay, and experiments (that is to say, inocculations) tried on guinea-pigs, white rats and mice, and horses. Cultures on agar or on bouillon were found to possess little virulence, and it was necessary to pass the bacilli through white mice—in other words, to inoculate them with plague—before sufficient virulence could be obtained; the bacilli obtained from these mice were then used to make cultures in bouillon, and the new bacilli thus obtained were injected into horses, generally under the skin of the neck. Local swelling, and inflammation of the glands, and slight fever followed these injections. Sometimes these injections were repeated as many as fifteen times on the same horse; then the jugular vein was opened, and from the blood thus withdrawn the serum was obtained, which was then injected into the abdominal cavity of white rats. As the plague serum thus obtained was not very virulent, it was suggested that more virulent bacilli should be employed, and that the injections should be carried out in large quantities, and for longer times on horses. The details of the preparation of plague serum, no doubt, vary very much; each manufacturer will have his own process; but in all there is the same system of injecting these filthy products of disease into healthy animals, so as to give them the plague, of finally injecting horses with the disease, and, when they do not die of it, bleeding them to obtain the serum, which is to be used for human inoculations. Monkeys have been used in the preparation of the testing of plague serum, as well as cats, guinea-pigs, mice, and rats. Dogs, we are told, are most refractory to the infection. "Lawson could not infect pigs, either by alimentation or by subcutaneous injection. Experimenting on sucking pigs he succeeded in causing tumefaction and œdema at the site of the inoculation, but could not discover the bacillus in the ganglia." That is out of "Bubonic Plague," page 16, by Montenegro, who wrote on plague

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at Oporto. The same author, on page 13 of his book, gives this case: "Batzaroff received some broth, the virulence of which had decreased so much that a large dose inoculated in a rat did not kill it. . . . Depositing a certain quantity of the cultivation in the nostril of another guinea-pig he succeeded in producing plague pneumonia, and killed the animal, but in eight days." "Simond (page 20) made a monkey breathe infected flour, but did not succeed in producing the pneumonia of plague." "Simond made a rat eat pieces of diseased spleen without any result" (page 21). Plague pneumonia is described (page 59) by the same author: "At the end of thirty hours fever begins, which rises very high; the animal appears to be miserable, depressed, breathes with difficulty, coughs up a foamy liquid. From its nose there flows a serosity which irritates the mucous covering of the snout; the inflammation . . . causes serious mischief in the eye. Meanwhile the exhaustion of the animal increases, respiration becomes strident, there is a distinct wheezing . . . and finally in twenty-four or thirty-six hours the animal succumbs." Thousands of animals have been used for these inoculations, and an enormous amount of suffering caused in the preparation and testing of the various serums. When Mr. C. J. Martin speaks of the "very valuable" influence of inoculations on the plague in India, he is probably not aware that the present epidemic has lasted eleven years, increasing in violence with every year, whereas the normal duration of an epidemic of plague is seven months; nor is he aware that during those eleven years (or, to be exact, ten years and seven months, from September, 1896, to the end of April, 1907) there have been 5,326,000 deaths from plague in India. Will he dare to say that without Haffkine's and Yersin's serums there would have been a greater number of deaths than five and a quarter millions? It is, to say the least of it, a significant coincidence that never within the memory of man has an epidemic of plague been of such immensely long duration, and never within the memory of man has plague been treated by inoculations of plague serum. When the plague broke out in Bombay in the end of 1896, all the experts were unanimous in declaring that the ordinary duration of the epidemic was seven months. In the "British Medical Journal" of February 6th, 1897, it is said: "As regards the epidemic, no better news is forthcoming, nor, we fear, is any great abatement of the disease in Bombay, or wherever else it may appear, to be anticipated with any confidence until six or seven months after the commencement of the outbreak. Bombay, it may be hoped, will be free of the epidemic in the month of April, that is seven months after its appearance in September, 1896." On February 13th, 1897, the same authority repeats: "We do not hope to hear that Bombay is free from plague before April, but we do rely on a distinct improvement by the end of that month." On February 27th, 1897, "the six or seven months of its wonted period of activity are over." On this same date occurs this ominous note: "Dr. Yersin is on his way to Bombay with a stock of serum." With the arrival of Dr. Yersin's serum, and other serums, all hope of the epidemic keeping to "its wonted period of activity" was at an end. A manufactory of plague was established, which has been hard at work ever since, producing its five and a quarter million deaths. These figures, 5,326,000 deaths from plague from September, 1896, to the end of April, 1907, were given by Mr. Morley in the House of Commons on June 6th, 1907, on the occasion of a debate on this subject, when Dr. Rutherford said that the plague was one of the chief causes of the unrest in India, that it was a preventible disease, that it had been brought under in most countries, and that only in India had it such awful effects ("British Medical Journal," June 15th, 1907). In the months of January, February, and March of this year the deaths from plague in India reached the enormous number of 58,438, 98,397, and 171,522 respectively.

20472. Do I correctly understand you to suggest that the use of these sera of Haffkine and Yersin has been the means of propagating the plague?—I do.

20473. Or that the use of them has diverted attention from other means in regard to plague prevention?—I do.

20474. Do you make both those suggestions, or which?—I suggest both. I suggest that in the first place the use of these sera was an actual propagation of the plague. These sera are not at all analogous to vaccine which is used for the prevention of small-pox. They are plague itself, and they give the plague. I think there is no proof whatever that a man who is inoculated with Yersin's or Haffkine's serum is not just as much a centre of plague as a man who has caught the plague in the ordinary way.

20475. (*Chairman.*) Are you speaking now from your own knowledge or from something that you have read?—I am speaking from my reading, not of my own knowledge. I have fortunately not been in these centres of plague, so I cannot speak from personal knowledge, but it seems to me that the inoculation with these sera for plague is very analogous to the old system of inoculation for small-pox, which is not only discredited, but is actually forbidden by law, I believe, at present.

20476. (*Sir William Collins.*) Are you not aware that it is claimed by some investigators that vaccine can be made out of small-pox virus?—I have heard so, but most assuredly if you inoculate a person from the cow pox virus you do not get a case of small-pox. I have heard it, but I find it extremely difficult to believe.

20477. Have you not also heard that it is claimed that by the use of small-pox virus the vaccine may be obtained by inoculating a calf or cow with human small pox?—No, I do not know that. I do not know whether it is proved or not.

20478. (*Sir John McFadyean.*) Might I ask why the material from this cow disease, cow-pox, is not regarded as "filthy materials" by your Association?—Our Association is not prepared to offer any evidence as regards the question of vaccination, because vaccination is a subject of special Acts of Parliament, one of which was passed as lately as last year, and we did not understand, and we do not understand, that it comes within the function of this Commission to inquire into it.

20479. That was not my question. I did not wish you to express an opinion as to the efficacy of ordinary vaccination, but you class sera and other products of disease as "filthy materials" when used with the object of curing or immunising human beings. I want to know, do you place the so-called cow-pox lymph in the same class, and, if not, why not?—I have already said that I do not come here prepared to give any evidence on the subject; as representing the Association, I offer no evidence whatever on the subject of cow pox. If the Commission would like to know my personal views on the subject of vaccination I shall be delighted to give them.

20480. (*Sir William Collins.*) Have you ever seen a case of natural cow-pox in a cow?—Natural cow-pox in a calf?

20481. In a cow?—No, I have never seen it.

20482. (*Sir John McFadyean.*) But I understood that it was you yourself that introduced the subject of cow-pox vaccination and contrasted the material used there, or the benefit of the operation with the use of serum?—I introduced the subject of inoculation.

20483. Of human beings?—The inoculation of small pox.

20484. I beg your pardon.

20485. (*Sir William Collins.*) Will you proceed to state the grounds on which you think that the practice of the use of sera has diverted attention from other means which you regard as more satisfactory?—As the counter proof that this treatment of the plague by serum inoculations has aggravated instead of "stamping out" the epidemic of plague, I will give the history of the outbreak in Alexandria in 1899. Before that time a Commission was sent by the Egyptian Government to inquire into the treatment of plague in Bombay. The "British Medical Journal" for August 14th, 1897, gives the report of this Commission formed by Dr. J. G. Rogers, Director General of the Sanitary Department, Egypt, Dr. Bitter, and Dr. Ibrahim Pasha Hassan. "The Commission agreed that improved sanitation is the only method of staying the inroads of the disease. The members declare that prophylactic inoculation is a method, the efficacy of which is not yet established, and contend that to render a whole nation immune on the outbreak of an epidemic is a practical impossibility." In May, 1899, the Commissioners had an opportunity of proving the truth of their report, when plague broke out in Alexandria. It was "dealt with on lines directly opposed to Mr. Haffkine's views," as Dr. J. G. Rogers reports in the following letter to the "Times" of August 4th, 1899: "Sir,—M. Haffkine's address on

the subject of preventive inoculations against plague delivered before the Royal Society, and the leading article in the 'Times' on the same subject, will excite considerable interest amongst practical sanitarians all over the world, and nowhere possibly more so than in Egypt, where plague is now being dealt with on lines directly opposed to M. Haffkine's views. In considering those views it must be remembered that M. Haffkine is not a medical man, and that therefore in all probability he looks on disease from the laboratory point of view. It is otherwise difficult to explain his persistent attitude of opposition to the work of the practical sanitarian, opposition which, in the opinion of many, has had a dangerous, if not a pernicious, influence in India. It must never be forgotten that, at a moment when, though somewhat late, practical proposals were put forward by a committee of practical medical men of long Indian experience for checking the spread of plague in Bombay, M. Haffkine alone dissented, and by throwing the full weight of his reputation into the scales against any really useful sanitary measures being applied, was mainly instrumental in these measures being rejected. Plague has been known to exist in Egypt since May 4th, and no doubt the infection was introduced before that date. By applying the sanitary measures so despised by M. Haffkine and denounced by him before the Royal Society as useless in the prevention of the saprophytic forms of disease such as plague, results not altogether unsatisfactory have been attained, and the disease has at least been kept well under control. The total number of cases has never risen above 13 in the week. In the weeks ending June 28th, July 5th, 12th, and 19th, they were respectively 12, 11, 9, and 6 cases of the disease. In 100 houses in which plague cases or suspicious cases occurred, and which were efficiently disinfected, in not one instance has a second case been noted. Had practical measures been abandoned and protective inoculations substituted, would these results have been attained? I venture to think not.—I am, Sir, yours truly, J. G. Rogers, Director-General, Sanitary Department, Egypt."

20486. Did Dr. Haffkine reply to that letter?—I have no reply. I do not know.

20487. Did he not on August the 12th in the "Times" decline to discuss details with Sir J. G. Rogers?—I do not know; but the consequence was that the plague was stamped out in Alexandria within seven months.

20488. Has Dr. Haffkine not recently returned to India to resume work under the Government?—I believe he has. At all events this epidemic at Alexandria in consequence of the application of sanitary measures without plague serum lasted only seven months.

20489. (*Chairman.*) In consequence of what do you say?—Apparently in consequence of complete sanitary measures having been adopted, and no plague serum having been used, the epidemic was stamped out or ceased in seven months, which is the usual time for the duration of an epidemic of plague in any particular place. And on the 22nd October, 1899, the Minister for Foreign Affairs received a telegram from Lord Cromer stating that it was then three weeks since a case of plague had occurred at Alexandria. The "British Medical Journal" congratulated Sir John Rogers and Dr. Ruffer, "on the success which has attended their carefully devised scheme for stamping out the disease in Alexandria."

20490. (*Sir William Collins.*) Did not they employ sera in any form?—I understand not. I understand that no serum was employed; on the contrary, they declined to employ it.

20491. (*Chairman.*) When you say you understand so I do not always follow the source of the information. Do you mean from newspapers or books?—No, I understand from persons with whom I have conversed who are qualified to speak about it.

20492. Conversation?—Yes. But we have the testimony of Mr. Rogers to that effect.

20493. That you have read, I think?—Yes, who was Director-General. He at all events was of opinion that the sanitary measures were the cause of the cessation of plague.

20494. (*Sir William Collins.*) Is there any further question to which you wish to direct attention?—I now wish to call attention to the commercial aspect of the present system of sera and the present system of vivisection. Dr. George Wilson, in his Presidential address in the section of State medicine at the annual meeting of the British Medical Association in August, 1899, used these words: "The whole bacteriological theory and practice is steeped in commercial interests." This is at the back of all the arguments in favour of experimenting upon animals; that it constitutes in itself a well paid occupation.

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20495. (*Dr. Wilson.*) You are not quoting now from me?—No, only those few words. This is mine. The inventors of sera and antitoxins receives royalties upon the sales; the manufacturers of sera and antitoxins charge for them as for any other manufactured article is charged for. Mr. Power, the Medical Officer of the Local Government Board, explained in his evidence (Question 4885) that the vivisectors who do their pathological work receive a sum which pays for the animals used, pays for laboratory expenses, and includes their own personal remuneration.

20496. (*Chairman.*) What question do you say that is?—Question 4885.

20497. You must have got a wrong reference?—That is the reference that was given to me.

20498. You have not looked at it yourself?—No; you can easily imagine that I cannot verify every reference.

20499. Mr. Power's evidence begins at Question 4281.

20500. (*Sir Mackenzie Chalmers.*) You said you could not quite verify every reference, but this evidence is your own evidence?—Yes, only the remark in inverted commas is Dr. George Wilson's statement.

20501. (*Chairman.*) When you gave us just now the statement about payment to inventors and the manufacturers of sera, was that your own statement, or was it from some other source? Were you quoting?—Yes, that was quoted from Mr. Power's evidence.

20502. That is the point. I dare say it is quite correct, but we have not got the reference?—I can get the proper reference and insert it in the proof.* Then there is a quotation from Dr. Luteand to Dr. Bantock, stating that the Pasteur Institute is the property of a limited company——

(*After some discussion.*)

(*Chairman.*) I do not think we can have evidence of one doctor on matters of medical skill, simply through a letter written to another doctor.

20503. (*Sir William Collins.*) You desire, I understand, to draw attention to Messrs. Burroughs and Wellcome's price list?—I do. It is very evident that it would not be worth their while to keep up large laboratories and horse farms for the production of sera if the sale was not profitable. I desire to put their price list in simply to show that these are expensive sera; it shows the commercial aspect and the commercial value of these things.

20504. (*Chairman.*) I do not see what the distinction is between selling serum at a price and selling medicine at a price. Chemists do not sell medicine at a price that does not pay them. Is there anything immoral in selling serum at a price that pays any more than in selling castor oil or anything else?—There is nothing immoral about it any more than there is in any tradesman pushing the sale of his goods, but all I want to prove here is that there must be a motive in pushing the sale of these goods. I imagine that they are pushed just the same as the sale of any other article is pushed.

20505. Do you think that, in the case of a general practitioner in the country who made up his own medicines and sold them, it would be a reflection on his evidence about the value of the serum which you put to him in cross-examination to show that he was probably not speaking the truth?—I do not say that he is not speaking the truth.

20506. Or doing anything discreditable. I do not quite see what the discredit is. If people invent a serum which costs money to make I do not see why they should not sell it, and sell it at a profit?—I do not say that they should not sell it, and sell it at a profit. All I say is that when you sell things at a profit, and the profit comes to you, there is always an inducement to push the sale.

20507. (*Sir William Collins.*) Have you suggested that there is anything immoral in the sale of these

* *The reference is in Mr. Power's evidence—Questions* 4409 *and* 4411.—G. K.

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sera?—I have not suggested that there is anything immoral in it.

20508. (*Chairman.*) Anything to discredit?—What I suggest is that there is an inducement to push the sale.

20509. (*Sir William Collins.*) May it be that the sale of these goods is so extensive because they supply a felt want?—I do not say that it is not possible.

20510. (*Sir John McFadyean.*) I should like to put another question to you. Do you wish to put that list in as evidence, that the inventor in this country of any serum shares in the profits of Messrs. Burroughs and Wellcome?—That I have no means of knowing.

20511. What is the motive of putting in Messrs. Burroughs and Wellcome's price list if you do not suggest that any British inventor of a serum or any medical man who recommends it is getting any of the profits?—I suggest that the inventors of sera receive royalties on the sale.

20512. What is your source of information with regard to that?—At this moment I cannot tell you, but I will furnish the Commission with the information.

20513. Might I ask that you will furnish the Commission, in the proof of your evidence, with evidence that some British inventor of a serum is sharing in the profits obtained by the sale of it?—Yes, I will.

20514. (*Chairman.*) I think we should not admit any evidence of a personal kind of that nature unless it is really first-hand evidence; it must be something that would be really evidence in a court of justice?—I quite agree, My Lord. I think there ought to be something to support that statement that the inventors of sera and antitoxins received royalties upon the sales.*

20515. (*Sir John McFadyean.*) It is particularly British inventors that I wish you to notice?—The whole object of what I suggest here is not to prove that anybody has done anything disgraceful, but merely that there is an ordinary commercial aspect of the question and a monetary stimulus to pushing the sale as in any other case.

20516. My impression is that you have not produced any evidence to show that the inventors are commercially interested. That is what prompted my question?—I will endeavour to do so when I get proof.

20517. (*Sir William Collins.*) Do you desire to call attention to the question of vivisection as a means of training medical students?—The Parliamentary Association does not consider that there is any evidence to show that vivisection is necessary for the training of medical students. We consider that there is strong evidence to the contrary. Mr. Morris, the President of the Royal College of Surgeons, has reached that eminent position, according to his evidence before the Commission, without ever having performed an experiment on an animal. He also stated that there are many other practising surgeons who have never practised vivisection (Question 7786). Sir H. R. Swanzey, F.R.C.S., President of the Royal College of Physicians of Ireland, speaking of operations on the eye, declares (Question 9896-7) that he does not consider it necessary for a student to operate on an anæsthetised animal in order to acquire skill in such operations, but that he might obtain that skill by operating on the eyes of dead persons (I think his expression was dead persons). Mr. H. Morris, F.R.C.S., President of the Royal College of Surgeons, England (Questions 7751 and 7752) approves of some demonstrations before students for teaching, but not for operative purposes, and he gives the following evidence: (Question 7783) "You made your name, I think, which is a very good one, by experiments on the kidney, was it not? (A.) I never made an experiment in my life." (Question 7786) "Were those operations, which you have conducted so skillfully and fortunately, discovered, and were you made to do them by experiments on living animals? (A.) Personally, not at all. The operation that I did in the first place, and was the first to do, was done quite irrespective of any experiment on animals; that is, the cutting of a stone out of a kidney." (Question 7828) "I gathered from one answer . . . that you rather objected to the use of vivisection for the purpose of obtaining manual skill? (A.) Those are my feelings, I do not think it is necessary." (Question 7831) "Shall I be right in thinking that there are many practising surgeons who have never practised vivisection? (A.) Surely yes."

20518. Of course, all this evidence is before the Commission. Is there any special point that you desire to make upon it?—Merely this, that if the President of the Royal College of Surgeons, England, can obtain such eminence without having ever made an experiment in his life, that seems to be a refutation of the claim that operations on living animals are necessary for the training of students in manual skill.

20519. You also desire to refer to certain answers given to questions by Sir H. R. Swanzey. Perhaps if you cite the number of the questions it will be sufficient?—Questions 9896 and 9897. I think I have already stated the effect of those questions. Then there is Sir W. Thornley Stoker, Inspector for Ireland, who says (Question 761): 'I believe that such demonstrations during lectures are used more for the purpose of interesting and attracting a class than for any direct teaching value they possess. I am strongly of opinion that in any legislation which may be undertaken with a view to the revision of the Act 39 and 40 Vict. c. 77, experiments on living animals should be forbidden in illustration of lectures, on the ground of their uselessness, and perhaps cruelty also. . . . Such demonstrations cannot but be demoralising to the young men and women who witness their performance. They seem to me to be an offence against humanity." Then he was asked at Question 904: "But the fact that the experiment can be carried out without pain would not modify your attitude in the least?" and he said "It would not." That is the end of what I should suggest as to the evidence. I have been asked by the Committee of the Parliamentary Association to make an explanation with regard to an oversight in Mrs. Cook's evidence if the Commission will hear it.

20520. (*Chairman.*) I do not see how, when we have had a witness before us who has given evidence and been examined, we can ask another witness to give an explanation of what she said. It must stand for what it is worth?—She also represented the Committee of the Parliamentary Association.

20521. But I do not think that makes any difference?—Then that will be all I have to say.

20522. (*Sir William Collins.*) Does that conclude all that you desire to say in examination-in-chief?—That concludes all I desire to say if the Commission rule this explanation inadmissible.

20523. (*Chairman.*) There are just a few questions that I should like to ask you. I observe that you say on page 2 of your *précis*, at the bottom of the third paragraph, that the Government of the day were no doubt glad to get rid of so unsavoury and controversial a subject so speedily and so easily. The reference is to getting that Bill through in the last four days of the sittings of the House?—Yes.

20524. You say that it was a controversial subject; but is it not the very strongest possible evidence that it was not controversial that it was allowed to pass through the House in all its stages in the last four days? Could you have stronger evidence that the Bill was not a controversial Bill?—I think that a Bill, for the purposes of the House of Commons, if it is being pushed through in the last four days loses a great deal of its controversial character for practical purposes.

20525. Do you not know very well that the Government are always asked towards the end of a Session what Bills they propose to press, and they say: "We propose to press such-and-such Bills, but not if they are controversial"? Unless they are really absolutely essential Bills they do not at that stage bring in a new Bill and pass it through unless both sides of the House are prepared to let it go?—It is a controversial subject. I am not prepared to say whether in those days it was regarded as a controversial Bill. Of course, as you know yourself, there are many degrees of the controversiality of Bills.

20526. When you spoke of it as controversial, and at the same time as having passed through the House in the last four or five days of the Session in all its stages, it struck me as a very strong piece of evidence that it was not a controversial Bill, but was generally

* "*Behring has patented his diphtheria anti-toxin serum on the Continent; Koch for years has made a princely royalty out of his tuberculin.*" *From the Presidential Address in the section of State Medicine, at the Annual Meeting of the B.M.A., August*, 1899. *By George Wilson, M.A., M.D., LL.D.*

assented to?—What I rather referred to was that it was, at all events, controversial in the country, if it was not controversial at that time in Parliament. Of course at that time, between the 10th and the 15th of August (and particularly in 1876, I think it was), I do not suppose the attendance of members was as it is now. They had mostly gone away for the grouse shooting, and so forth, before the 15th.

20527. I should have thought, after some experience of the House of Commons, that it was very strong evidence that it was not considered controversial?—I bow to your greater experience.

20528. You speak of there being 43,000 experiments in 1906. Do you know at all how many of those were inoculations?—No, I do not.

20529. The vast majority?—I do not know whether there is any means of discovering that.

20530. Are you aware that a large number of the 43,000 were experiments ordered and conducted on behalf of the Board of Agriculture, the Board of Trade, and the Local Government Board?—Yes, I believe that a great many of them were.

20531. I am told that about 6,000 were operations under that head, and about 20,000 in cancer research?—Those 20,000 in cancer research were inoculations.

20532. They were for what we may call a semi-public inquiry; it was not on the same footing as a Government inquiry?—No.

20533. Then I see you rather suggest that Dr. Starling's evidence on the question of pain is inconsistent with the views of other physiologists; I am leaving out Dr. Pembrey for the moment, but inconsistent with other statements. You say, for example, in the middle of page 7 of your *précis*, that he has said, and you seem disposed to doubt whether it is true, that he had on no occasion ever seen pain inflicted in any experiment on a dog or cat?—Yes.

20534. If you read the whole context I think you will see that it is quite plain that Dr. Starling is speaking about experiments under anæsthetics. He says that he always administers anæsthetics and that he administers them efficiently, and it is with reference to that that he is speaking when he says that he has never seen any pain inflicted in any experiment. You speak afterwards as if he was guilty of want of candour in saying that, because it is admitted that after animals have recovered (in cases where they are allowed to recover), they feel pain from the consequences of the operation. You observe that Professor Starling has never said the contrary of that. He says that in an experiment he has never inflicted pain, because he always used anæsthetics. He says that there may be pain afterwards in cases where the animal afterwards recovers. There is surely no want of candour there. You accuse him very strongly of want of candour. Is there any want of candour in those two statements?—I cannot find the second statement.

20535. You say on page 10 of your *précis*: "Professor Pembrey, in direct contradiction to Professor Starling and others, tells us that he does painful operations, and that, moreover, 'they are absolutely necessary.' Thereupon we are confronted by the question: Were those other gentlemen then imposing upon our credulity, or were they the victims of their own? Can extensive mutilations under anæsthesia administered, as Professor Starling confessed, by the laboratory boy, be wholly and invariably painless? Painful experiments, he says, are 'absolutely necessary.' This, then, is the candid truth of the matter." Are you aware that Dr. Starling's evidence on that point, as to pain not being suffered under anæsthesia, and as to a certain amount of pain being suffered afterwards, is the evidence of, I may say, a dozen of the most eminent physiologists and representatives of universities who have come here to give evidence before us?—I am perfectly well aware of that, but, as I understand, the anæsthetic, which I suppose was chloroform, was administered in the case of Dr. Starling's experiments by the laboratory boy. I think somewhere in his evidence, if I am not mistaken, there is some statement that when the animal began to move the boy increased the dose of chloroform.

20536. I do not know whether he said "boy"?—The laboratory boy.

20537. But that is hardly the point. The question here is that you are contrasting Professor Pembrey, who says that he does painful experiments, and that they are absolutely necessary, with the evidence of Professor Starling and others; and you speak of it as a question of candour, because you say that Professor Pembrey is a man who speaks the candid truth in the matter, implying that the others are not candid?—We certainly do imply that; and it seems to me obvious that if the anæsthetic is administered by the laboratory boy, and if the laboratory boy is entrusted with the duty of putting on more chloroform if the animal moves, those experiments cannot be absolutely painless.

20538. You will see that many questions have been asked on that subject, of how it is administered and by whom, and so forth, throughout the evidence, which I cannot take you through; but what I am pointing out now is this. I understand you to say that, although these 12 gentlemen practically agree in their statements about anæsthesia and its being complete, and about there being no pain suffered at the time by the animal, but that some pain is suffered afterwards, they are not candid witnesses, because they say that and because Dr. Pembrey says the reverse. You accept him as a candid witness, and the other 12 you do not?—He certainly was candid. He said "painful experiments are absolutely necessary."

20539. But you contrast him with their evidence. Do you suggest that these other 12 gentlemen were uncandid witnesses in what they said?—They have——

20540. I should have thought it would not be difficult to answer that with yes or no?—I always presume that these gentlemen have a different idea of pain from the idea that we ordinary laymen have.

20541. Then it was not so much a want of candour, as that they were under a delusion?—You may put it in that way if you like.

20542. Which do you prefer to put it as: that it was delusion or want of candour?—My Association think it is want of candour. I am somewhat doubtful whether it was from want of candour or a mistaken idea—whether it came really from a certain callousness imbued in them by their profession.

20543. Your society's suggestion then is, that all these very eminent members of their profession have come here and, as you say, have been "imposing upon our credulity"?—Well, we are told by another of them that pain is caused—that is "absolutely necessary."

20544. But do you say so. Never mind about the evidence of others at present. You say, "Were those gentlemen imposing upon our credulity or were they the victims of their own"? You say that your society (whatever you may think) do not think that they were the victims of their own credulity. Then we must adopt the other alternative, that they were imposing on our credulity. Is that your society's view?—I said: "Were these gentlemen imposing on our credulity?" In that case it would make them out as making an immoral statement. "Or were they the victims of their own?" That means, were they under a delusion? We ask the question.

20545. But you said that your society's view was that they were not under a delusion?—I should say that that is the view of the society.

20546. Then it is a want of candour?—Yes.

20547. Then we are to understand that these 12 gentlemen came here for the purpose of deceiving us, and Dr. Pembrey is the one honest man?—I do not say that he is the one honest man, but he seems to have been honest.

20548. You are aware that Dr. Pembrey's views on the whole subject differ very largely from those of his confrères?—Yes, I know they do.

20549. His view seems to have been that pain is a blessing and that anæsthetics are a curse?—Yes.

20550. (*Mr. Ram.*) To human creatures and to animals?—Yes.

20551. (*Chairman.*) And that the Vivisection Act is entirely opposed to the advancement of physiology in every part?—Yes.

20552. You are aware probably, if you have read the evidence, or have had it read for you, that I think I might say almost all the witnesses who have been called as physiologists are not opposed at all to the present restrictions as to anæsthesia; they are content with the restrictions imposed, with the obligation to use anæsthetics?—I believe those exceptions and restrictions in the original Act were really concessions

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to the medical profession, and therefore it is to be supposed that they are satisfied with them.

20553. I am putting the question, not for the purpose of arguing whether they were right in saying so, but as showing that their views and Dr. Pembrey's views are quite different on the main points in dispute, that they take quite a different view?—I was going to say that on the Second Reading in the House of Commons, Dr. Ward, one of the representatives of the medical profession, moved the rejection of the Bill, and after explanations from the Government, who professed great sympathy with the medical profession, he withdrew his opposition to the Bill.

20554. What Bill was that?—That was this Bill that is at present operative, the Cruelty to Animals Act, 1876; thereby admitting that the Bill was in conformity with the views and desires of the medical profession.

20555. There are such things as compromises, but I am putting it that it was in conformity with the present views as regards anæsthesia?—I may say that the society would not admit that there was any compromise about the Cruelty to Animals Bill.

20556. I do not know how the fact was in the least. I am merely suggesting that the mere fact that a person withdraws his opposition to a second reading does not mean that he agrees with everything in the Bill always?—No, that is so.

20557. (*Sir William Church.*) Referring to the bottom of the fifth page of your *précis*, do you think that the community suffers at the present moment from a want of humanity in the medical profession?—I think it does to a certain extent. Of course, you are asking me a question which I can only answer from my personal experience, and from what I have heard, and so forth. I think that there are a certain number of uncommonly rough physicians and surgeons in the profession.

20558. And you think that that number has increased since 1876?—In consequence you mean of what?

20559. I do not say what it is in consequence of; I ask you whether you think that the number of rough physicians and surgeons now existing is proportionately greater than it was before the Act of 1876?—That I cannot tell; it is impossible to tell. I was not acquainted with the circumstances before the Act.

20560. Then you have no evidence whatever that the introduction of the Act and the working of the Act have led to the demoralisation of those who enter the medical profession?—I think the medical student is a good deal rougher than he used to be.

20561. Is that the generally received opinion?—That is the general view, I should think.

20562. Do you mean to tell the Commission that it is the generally received opinion now in the world that the medical student is a more uproarious and rougher individual than he is described in books written some 50, 60, or 70 years ago?—I am not prepared to say that, but I think he is rough enough, at all events.

20563. That is not my point. I wish to know what evidence you can give us of the statement that the medical profession is rougher now, and I suppose you mean by that is less regardful of the feelings of those who come under their treatment, than the profession was 30 or 40 years ago?—The only evidence that one can give of that is hearsay. I do not for a moment suggest that the great mass of the medical profession are not exceedingly humane. I think they are. Personally I have never come across a doctor who has not been humane. But at the same time I can only imagine, speaking for oneself, that seeing these experiments carried out day by day all the horrors of it would have the effect of blunting one's sense of sympathy.

20564. Therefore if I might so say, it is only what you imagine to be the case. You have not any evidence to bring before us that it is the case, but it is what you imagine might be the case?—It is just the same thing, or it is very much the same thing as that a man who shoots a great deal is utterly insensible to the sufferings of the animals that he wounds, and the more he shoots the more insensible he becomes to it. I cannot think that it is improving the humane sense in them.

20565. So that we are all degenerating—our sportsmen have degenerated, too, because they kill more pheasants in a day now than they used to do 60 or 70 years ago?—I do not know that our sportsmen have degenerated, but I know that I myself gave up shooting because of the horrors of it. I used to shoot a great deal, and in consequence of the horrors that I saw I gave it up altogether..

20566. (*Mr. Ram.*) Then it did not have the normal effect upon you, Sir George?—What do you mean?

20567. You are putting it that the effect of much shooting is to inhumanise a man. It had the opposite effect upon you, I gather?—As a general rule I mean, but sometimes it turns the mind in the other direction, and it did so with me.

20568. (*Sir William Church.*) Then with regard to what you say on page 6, has clinical research been neglected, do you think since demonstrations have been shown in teaching medical students?—Of course, I am not an expert, and I am not qualified to answer these questions thoroughly, but I have heard from certain doctors whom I have come across in my position as honorary secretary of this Association, and whom you would call anti-vivisection doctors, that clinical research is to a great extent subordinated in these days to experimental research.

20569. Have you any knowledge of the present curriculum which is required of a medical student?—No.

20570. Then you have no knowledge, I suppose, of the medical curriculum that was required before; say 40 or 50 years ago?—No.

20571. You are not aware that now it is incumbent upon a medical student to attend for a fixed number of months upon clinical teaching and clinical observation?—I am not acquainted with the course of study of medical students.

20572. But you stated in your evidence that there are other means, that medical scientists are not driven to use experiments only, and you said, in answer to Sir William Collins, I think, that you meant by that that clinical research might be carried further?—Those are the views of my Association, and I think they are the views of a certain portion of the medical profession.

20573. But I rather gathered, I may, of course, have misinterpreted you, that you considered that the barren state of our knowledge was because clinical investigation and clinical research were not carried out, and were neglected for experimental research?—No, I have not said so. What we have said is that in all these years during which vivisection has been practised it has not had any great effect in either curing disease or preventing disease.

20574. Have the other means of research which have been in use for ages had any great effect?—I think at all events it produced all the knowledge that we have.

20575. Is it not the case that the great incentive for scientific research in physiology and in pathology is in the hope of supplementing the knowledge that we get from mere observation of disease?—I presume that that was the reason of the introduction of vivisection, but it does not seem to have had any result up to the present time. That is what we contend.

20576. I would beg leave to differ from you, because surely the foundations almost of medical knowledge arise from experiments made upon animals two thousand years ago, when the first cases that we have information of began. Do you think that no knowledge was obtained by the experiments of Galen?—I am not acquainted with the experiments on animals at so distant a date; but all that we affirm is that no real knowledge has been obtained as to curing disease or preventing disease from the vivisection experiments under the Cruelty to Animals Act. That is what my Association are prepared to contend.

20577. You mean that your Association are unable to find any direct practical application of any group of experiments since the Act of 1876 was passed?—I do not mean any practical application, because there has been plenty of practical application, but no beneficial application.

20578. Therefore you do not consider that any benefit whatever has been derived from the use of any what I will call sera, in the treatment of disease?—No, on the contrary, we should be inclined to hold that more lives have been sacrified by the introduction of sera than if they had not been used. We have an instance that such is the case in Koch's tuberculin, which I believe led to the loss of a considerable number of lives, and has now been exploded.

20579. I beg to differ from you there altogether, it has not been exploded. But that is your position, that really no benefit has been proved yet to have resulted either to men or animals from the use of sera?—We should go further and say that harm has resulted, as in the case of plague.

20580. But you say you have no medical knowledge, and therefore I will not go into any actual specific cases in which I think you would find a difficulty in maintaining your view?—You quite understand that I am not a medical expert. I am merely dealing with this question as an outsider, with it is to be hoped a certain amount of common intelligence and sense; that is all.

20581. I think you told us that you are in the habit of reading books on medical subjects?—I read some that are germane to this particular subject.

20582. I suppose your reading is chiefly directed to works which are written by those who hold the same views as yourself?—No, I do not say that. One question which I have been greatly interested in, on public grounds of course, is the question that I have dealt with in my evidence at some length, namely, the question of bubonic plague in India. That is a question on which I have read.

20583. I should like to ask you a few questions on that point. You gave us as an instance of a disease in which no increase of knowledge had taken place, influenza. What makes you take 1729 as the year that you start from; is there any reason for taking it?—That is our first record of the invasion of influenza, so far as I know.

20584. I think if you look you will find that influenza epidemics were described long before that time. The first one that is described goes back to the fifteenth century. I should have thought that the argument that you make use of with regard to influenza is just an argument the other way. Perhaps you are not aware that experimentation in bacteriology has rendered it probable that we are acquainted now with the organism with which is always associated, if it is not the one which causes influenza; so that we have advanced somewhat in our knowledge?—I did not know that a particular bacillus had been discovered; but, of course, I am aware of the theory that the bacillus is not the *causa causans* of zymotic diseases generally.

20585. You do not think that the bacillus that is stated to be at all events always found with influenza is the *causa causans*?—Of course, I am always obliged to guard myself by saying that I am not a medical expert; but as I understand the two theories, one is that the bacillus is the cause, and the other is that there is a poison surrounding the bacillus on which really the bacillus feeds.

20586. If the bacillus produces the poison?—I did not say that. I said on which the bacillus actually feeds, in which it lives, and that the bacillus is really a cleansing and curative agent.

20587. Then I will not pursue that any further?—But all I say is that I am aware of those two theories. I am not medical expert enough to be able to adopt one side or the other.

20588. I will not pursue that any further: but I think that, in answer to a question by Sir William Collins, you mentioned the drugs of antipyrin and sulphonal, which you said had had great repute, and were now exploded or were not in favour. What leads you to say that?—They are not used now.

20589. Would you think I am wrong if I tell you that these two drugs and several others which are similar in composition are used daily, and I might say hourly, throughout certainly this country—I should think almost throughout the world?—I am informed that antipyrin is very seldom prescribed now because of its effect on the heart, and that sulphonal is also not prescribed generally because of its cumulative effects.

20590. May I ask you who was your informant? Where did you get that information?—I got it from a member of the medical profession.

20591. Did he tell you that he did not use them, but that he probably used bodies closely resembling them, which have also been introduced of late years?—That I do not know; but I do know also that in my domestic pharmacopœia sulphonal and antipyrin once played a very considerable part, and now they are not prescribed.

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20592. I will not say the contrary?—That is all.

20593. Then on page 7 you say that "the evidence as to the pain suffered by animals in process of mutilation is wholly untrustworthy and unreliable, either because of the inaccuracy of statement or the inaccuracy of observation of the operators, or because under the Act as at present carried out one operator actually experiments without pain, while another confesses that he has done painful experiments." I should wish to ask you whether you are alluding to experiments carried out with the same object. Do you mean to say that when two experimenters carry out the same object, in one case the experiments are done with pain and in another without?—Do you mean similar experiments?

20594. I do not mean similar; I mean the same experiment done by two experimenters—an experiment directed to the same end?—That it may be carried out with pain in one case and not in the other.

20595. Yes?—I imagine that that is quite so, according to whether the anæsthesia is complete or not. Of course we do not presume to say that complete anæsthesia does not prevent pain. Of course it does.

20596. I did not gather that. What you mean, then, is that the difference in the result may be due to one experimenter having his animal under an anæsthetic and the other having his not under an anæsthetic?—Or, rather, one having the animal completely anæsthetised and the other having it imperfectly anæsthetised.

20597. You do not give the experimenter (who is a man whose whole attention has been directed to these subjects) the credit of being able to know whether he is performing experiments under one condition or the other?—In many cases, as I understand, for the practical purposes of the experiment the anæsthesia is kept as light as possible. I do not mean that it is not complete, but it is not very heavy anæsthesia.

20598. (*Mr. Ram.*) Do you mean that it admits of suffering?—I mean that in that case there may be intervals of waking, and therefore intervals of consciousness, during which pain is caused.

20599. (*Sir William Church.*) Then I gather from that that you would not grant that any experiments are valuable for the acquirement of knowledge, but that, if they were, you think they ought to be done without any anæsthetics at all?—No. I say that they ought not to be done at all, with or without anæsthetics—at least, that is the view of our Association.

20600. Then you rather complain on page 8 with regard to the mice that have tumours. You say: "We are not told of means taken to restrict the growth." Have you ever made any inquiries as to what is done with mice when the tumours become large? Has your society ever asked for any particulars?—I do not know, but I confess that as a mere ordinary layman I do not understand the advantage of implanting a piece of cancer in a mouse, and making it a tumour, because that is not the way in which cancer comes in a human body.

20601. I will not go into that; but, as a matter of fact, your Association has never either written or asked the Cancer Research Fund anything about the mice, and never asked to be allowed to see them, or anything of that sort?—I do not know that they have.

20602. They are not aware that if a tumour is likely to be produced, say in a mouse, which in any way produces symptoms of pain, that mouse is killed, the object having been probably obtained?—I have no doubt that it is killed. I take it from you that it is. But of course I do not know that we have made any inquiries as to what happens. I suppose it is killed.

20603. Of course, no method is taken to restrict the growth, because the growth is what is required. It is the only way by which material for experimentation can be obtained, by keeping up the growth in a succession of animals; but no questions have ever been asked as to whether the animals are kept in pain or not?—I should imagine that during the growth of the tumour they must feel pain, judging from the analogy of human beings, at all events. I am not prepared to say more.

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20604. It is a very natural thing to imagine, but I merely mean that your Parliamentary Association has not made any inquiries into the subject?—That I cannot say.

20605. What is the reason that leads you to state on the same page, "There must necessarily, upon waking up from anæsthesia," (that is after operations on man) "be very severe and prolonged pain until the severed tissues heal"? Is that the result of your reading or your information?—That is in the extensive cutting operations, with excision of organs. I should have thought that any ordinary layman would know that.

20606. Then that is not the result of any inquiries that you have made of persons who have undergone severe operations which have healed by first intention?—This *précis* has been prepared by members of the medical profession, and I imagine that they, at all events, know from experience that such is the case, although I have no experience of the kind.

20607. Might I ask by whom this *précis* has been prepared?—It was prepared by the Parliamentary Association generally and their executive, and there are in it members of the medical profession.

20608. Mr. Stephen Smith?—There are several. I do not wish to mention their names.

20609. Is he one?—He is not one.

20610. Miss Arabella Kenealy?—Yes, she is one.

20611. (*Mr. Ram.*) You told us that you represented experts?—Yes, to a certain extent. There are a certain number of experts connected with our Association. When I say experts, I mean fully qualified medical experts.

20612. Who assisted in the preparation of your *précis*?—Yes.

20613. Will you give us the names of any of those who assisted in the preparation of your *précis*?—I have already said Miss Kenealy.

20614. Any other?—Dr. Bourchier, and, I think, some others.

20615. (*Sir William Church.*) Your Association has not inquired into that question, I gather then, from those associated with the large general hospitals, and who perform many operations both in hospitals and in private?—I understand that the drift of the question is to show that with extensive operations with excision of organs, after the operation has taken place and the anæsthesia is over, there is no pain. That is what you would suggest?

20616. Not severe and prolonged pain. I suggest certainly that it is not necessary that that should take place?—I have not had many operations performed upon myself. I have had some little ones, and I have always found that any cutting out of anything, even the cutting of a fishhook out of a finger, is followed by pain. I have had a salmon-fly through my finger, and had to have it cut out. I have always found that that is followed by considerable pain, even that small operation. I cannot for a moment imagine it possible that after an operation such as the excision of organs there can be no pain until the tissues heal.

20617. First of all you say that it is in accordance with what you think yourself would be the case, without inquiry, and it is founded upon information that you have received from those people whom you call experts, whom the Parliamentary Association have consulted?—That is to say, it is a statement with which I, simply as an outside observer and an ordinary layman of some experience and a little intelligence, agree; that is all.

20618. Would you tell me where Dr. Pembrey says that he himself circumcises children without anæsthetics; I do not read his answer quite in that way?—The reference is not given.

20619. What he states is that circumcision when performed as a religious rite is not done under anæsthetics, and he does not say that he always does it himself without anæsthetics. What he states at Question 14,161 is: "I have circumcised children without anæsthetics." He does not say that he habitually does it. "If they cry it does not matter; it is much better that a child should cry and moan than that its life should be lost. That circumcision is not such a serious operation is shown by the fact that the ordinary medical man does circumcision under anæsthetics and the Jewish non-medical man does it without anæsthetics." And he himself, apparently having seen a death in a child from anæsthetics during the operation of circumcision, does not always use it for circumcision. But he does not state that he never uses anæsthetics.

20620. (*Sir Mackenzie Chalmers.*) I understand that he says that Jews never use it, and they circumcise largely?—He says that he has done it without anæsthetics, but he does not say that he habitually does it. That is the point, is it not?

(*Sir William Church.*) Yes.

20621. (*Sir William Church.*) I should like if I might to ask you a few questions about the plague in India. I gather from what you have said in answer to questions, and also from what you say on page 15 of your *précis*, where you say "A manufactory of plague was established," that was by the use of serum, "which has been hard at work ever since producing its five and a quarter million deaths," that the view of your Association is that the use of Haffkine's prophylactic has actually caused a spread of the disease?—The view that we hold is that the use of Haffkine's so-called prophylactic has not only not acted as a prophylactic and not reduced the number of cases, but that it has actually increased them; that it has created centres of plague where none existed before, and that it has as a matter of fact kept up the epidemic. I do not know whether it is all due to Haffkine. I think Yersin's serum is also answerable for some.

20622. But at all events the use of sera has caused an actual spread of the disease directly by its use?—Yes, that is our view.

20622A. Not because that has caused the Government to cease taking other precautions; that is what I want to get at?—I understand that the sanitary method of combating the disease is not taken to a very large extent in India.

20623. That I gather. You contrasted the sanitary steps which have been taken in India with those which were taken in Egypt?—I understand that sanitary steps have not been taken to the extent, at all events, that they have been taken in Egypt.

20624. In your opinion, therefore, the Government of India has been rather remiss in not taking more active sanitary steps in India in combating the plague?—I think so, and I think also that the Government is very much to blame, with all these facts before it, for not having taken active steps to ascertain whether Haffkine's serum did actually propagate plague or not.

20625. I will go to that in a short time. Are the circumstances of Egypt and India quite comparable. I mean with regard to taking what are called ordinary sanitary precautions?—I take it that it is a great deal more difficult to effect complete sanitation in India than it is in a country like Egypt, which is what I may call more closely governed.

20626. Therefore there would be greater difficulties in carrying out in India measures such as were adopted in Egypt?—No doubt.

20627. You allude, I see, to the state of unrest in India, and you say that the plague was one of the chief causes of unrest in India?—So we read—that plague, famine, and so forth, are the chief causes of unrest in India, or among the chief causes.

20628. In what way did this unrest in India arise in connection with the plague?—I think the people, whatever the Government may think, are gradually becoming converted to the same view as I am taking to-day, that the inoculation is actually causing plague amongst them.

20629. You think that it was the introduction of inoculation that caused the unrest?—I will not say the introduction of inoculation, but the continuance of the inoculation, and the fact that the plague has not diminished at all by the inoculation, but rather increased.

20630. You do not think that it is the attempt to put the rules of sanitation in force that has caused the unrest?—I have not heard so.

20631. And you would prefer not to give any opinion, I suppose, whether you think it is probable that that is the cause of the unrest?—Of course, if we were to go into the causes of unrest in India, we should be getting into quite political matters, if we deal with that very much. But the causes of unrest are of various kinds.

(*Chairman.*) Do not let us go into that question.

20632. (*Sir William Church.*) But, as a matter of fact, are there not reasons why it is very much more difficult for the Government to put laws of sanitation in force in India, than it is in Egypt?—There are reasons, of course, connected with the religious creeds of the people.

20633. In fact, you cannot move the population about, you cannot move the population from a village to another spot without making arrangements for the different castes being separate, can you?—That is so. But I was not arguing merely that if they had taken measures of sanitation in India to the same extent that they have in Alexandria they would have stamped out the plague. I was rather arguing that if they had left things alone without the introduction of Haffkine's serum, the plague would, so to speak, have burnt itself out.

20634. That is to say the actual use of Haffkine's and Yersin's sera have disseminated the plague?—That is our view.

20635. And how have you arrived at that view?—No epidemic of plague, I believe, before this one has ever lasted more than about seven months, and the only condition in which this epidemic of plague differs from others is in the introduction of these sera. That is one reason.

20636. Has the present epidemic of plague that is going on in India been equally severe over all parts of the country?—That I cannot answer you.

20637. India is a very large country?—Certainly.

20638. You are not aware at all of the way in which the mortality has varied in different parts of India, according to the Government Returns?—No, but I can well understand that from natural causes it would vary in that way.

20639. I have the returns before me for five years, from 1901 to 1906. Your Association have not looked at those to see how the mortality has varied in different spots at different times during those five years?—Probably not.

20640. Have your Association read any of the reports of what I might call the experimental use of Haffkine's or Yersin's sera?—I myself have a book here which is very interesting on the subject of these sera, by Professor Montenegro.

20641. That was before the present epidemic, was it not? Was not his book prior to the present epidemic in India?—This book was written in 1900, during the epidemic.

20642. At the commencement of it?—Yes, and it gives sundry particulars about Haffkine's lymph and Yersin's lymph and Luslig's vaccine. I observe that one of your witnesses before the Commission, who was an Indian medical man, Colonel Lawrie, said that when Haffkine's serum came to him, as a general rule it was putrid.

20643. Do you know the results of the use of the serum at the Byculla gaol?—No.

20644. Would you accept it as correct that of the prisoners living exactly under the same conditions so far as regards lodging and food and possible communication with the outside, 172 were not inoculated, and among those there were twelve cases of plague, six of which proved fatal, and on the other side of the jail, where there were 147 inoculated, there were two cases and no deaths. If the serum spreads the disease, how was it that there were not more cases on the inoculated side of the jail than on the uninoculated side?—As I understand, the whole of one side of the jail was inoculated.

20645. 172 were not inoculated, and 147 were?—Were they all mixing together?

20646. No, they were not?—I thought you said that they were on different sides of the jail?

20647. We will leave that out, as I am not positive about it. But it is the case that 172 were non-inoculated and there were twelve cases of plague and six deaths, and of the 147 cases inoculated there were two cases and no deaths?—But everyone of the inoculated cases had plague.

20648. No, this was for prophylactic purposes?—With Haffkine's serum?

20649. Yes?—But I read here that Haffkine's serum whenever it is given always induces a mild attack of plague.

20650. I beg your pardon. This was for a prophylaxis purpose against the plague?—I quite understand that, but the prophylaxis consists in the inoculated persons having a mild attack of plague.

20651. I will not go into that question, but they were healthy persons. It was not done for cure; it was done for prevention?—Yes, I understand that; but my point was that inasmuch as all these persons who were inoculated had a mild attack of plague, they could not possibly disseminate it—they all had plague, although it was mild. The two cases, I suppose, that died were cases of virulent plague.

(*Sir Mackenzie Chalmers.*) The two cases did not die.

20652. (*Sir William Church.*) Two cases had it?—Yes, then they were cases of virulent plague.

20653. Two cases apparently, according to you, had a second attack, one when they were inoculated and subsequently another attack?—Or had it virulently. But it is admitted, as I understand, that Haffkine's serum causes a mild attack of plague.

20654. (*Sir John McFadyean.*) Do you mean admitted here before the Commission?—I understand that it is admitted by the medical profession, and by Haffkine himself.

20655. (*Sir William Church.*) At all events, you are not prepared to contest those figures that I gave you?—No, only I should distinctly contest the deductions from them.

20656. I only wanted to know whether your Association have really taken the trouble to have any of the returns, which could easily be got now, before them and investigated them?—That I cannot tell you. But I do not think I should judge on such a return as that. There is no reliance whatever to be placed upon it, because although the return may be perfectly correct, it is vitiated by the fact that all cases inoculated with Haffkine's serum have a mild attack of plague; that is, they have a mild attack of plague, and they are centres of infection themselves wherever they go, and, as I understand, also they are only immune for a very short time, I forget how long.

20657. I quite appreciate your position now, and I will not ask you anything further about it, as it is a scientific and medical question. You say the action of the serum so far as plague goes is similar to that of inoculation in small-pox formerly?—Yes.

20658. I did not understand that that was your position before. Does that also apply to the use of other sera besides plague sera, that when you make use of sera you give either the animal or men the disease that you are trying to give it immunity from?—I take it that it does in some cases. I presume that you give it a modified form of the disease, otherwise no immunity would be conferred.

20659. Have your Association taken any trouble to inform themselves on that question?—I have no doubt that they have.

20660. But surely you must know whether they have; you come to represent them?—Of course, I cannot possibly tell everything that my Association have done.

20661. It is one of the most important and most difficult subjects, and one that you seem to have had a great deal of advice upon?—I cannot tell exactly what my Association have read. There are a good many members of it.

20662. So that really I may take it, I do not wish to say anything offensive, that personally you have brought forward the plague in India as an instance before us without having any personal knowledge of the subject, and apparently without the Association having taken very great pains or labour to investigate the subject?—I do not admit that at all. I admit that I have not had any experience from the point of view of a medical expert. I have not been in India, and I have not seen the plague; but I do say that I have read a good deal about it, and that I know as much (as I have indicated originally in offering my evidence to the Commission) as any ordinary intelligent educated man would know about such a subject, and I think I am entitled as one of the general public to draw my deductions from the facts that are reported to me.

20663. But your deductions were a most serious charge against the Government. It comes to this, that not only have they neglected enforcing what are called the ordinary sanitary laws, but that they actually spread plague by the means they have taken?—

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It is a very serious charge, but undoubtedly it is a charge that the natives of India are beginning to make, as well as some people at home.

20664. I will not pursue it?—If I might supplement my answer to one question as regards the effects of Haffkine's serum, I will read a passage from Montenegro, at page 73: "The inoculation causes a mild attack of plague. In six hours the patient suffers from malaise, headache, depression, and fever, and this condition continues a couple of days, at the end of which there is usually vomiting and diarrhœa. The symptoms disappear slowly. The site of the inoculation is much congested, the skin desquamates, sometimes eruptions appear." That is suggested here by this Professor Jose Verdes Montenegro, a distinguished medical man in Spain.

20665. (*Chairman.*) Do you know that he is a distinguished physician?—He is ex-Interne of the Central University of Medicine, Associate Physician of the Hospital de la Princesa, and Professor at the Municipal Micographical Laboratory, Madrid. He is a distinguished man.

20666. If this is to show that the fever which is given by inoculation is of a serious kind, I think that we are all agreed that the plague is a very serious disease?—It is to show that the inoculation actually causes plague.

20667. That is the question I asked you. Is this a description of the disease which is imparted by inoculation?—Yes.

20668. (*Sir Mackenzie Chalmers.*) Did you read Dr. Martin's evidence, who came here, who had inoculated himself twice or three times?—No.

20669. He gave us his personal experience of inoculation, which does not quite accord with Professor Montenegro's.

20670. (*Sir William Church.*) I think I can throw light upon it. I think I am right in stating that Dr. Montenegro's work was published in the last century; that is to say, about 1899, or some such date as that?—1900.

20671. And is it possible that knowledge with regard to these bodies has advanced since then?—I am not prepared to say. I do not know whether the lymph is differently obtained now, or what happens.

(*Dr. Wilson.*) But your contention is, I think, that immunity cannot be conferred in the case of any of these diseases for which vaccines or sera are used as preventives unless a mild attack of the disease is induced.

(*Chairman.*) Is not that a medical criticism? I do not think the witness really professes to lay down general rules about medical effects.

(*Witness.*) No; only that immunity is stated to be conferred by Haffkine's lymph, and it continues for six months, but it does not continue for longer.

20672. I understand your view to be in this case that Haffkine's remedy given as a prophylactic gives the disease itself in a mild form?—That is so.

20673. And that the disease in a mild form is capable of being communicated to others in a virulent form?—Yes.

20674. And therefore it spreads it?—Yes.

20675. But, of course, you are here speaking from information that you have gathered from books and conversations, and not from your own personal knowledge?—Of course.

20676. (*Sir John McFadyean.*) I think you said that you were so impressed with the pain caused to animals in shooting that you had given it up?—Yes, that is so.

20677. Could you tell us how long it is since you gave it up?—About 15 years.

20678. I understood you to say that, while disclaiming any expert medical knowledge, you felt quite competent to give evidence before this Commission on such questions as anæsthesia, and the results of experimentation on animals, in virtue of the fact that you were a person endowed with ordinary powers of observation and common sense?—Quite so.

20679. To what extent have you exercised your power of observation in judging as to the possibility of anæsthetising animals sufficiently? One of the questions you have given evidence on is anæsthetics?—Yes.

20680. To what extent have you exercised your powers of observation with regard to the controversial matters relating to that?—I am afraid that I do not understand the purport of the question.

20681. I do not want to misquote you, but I think you said that ordinary powers of observation and common sense would enable one to judge with regard to such questions as anæsthesia, and the value of the results obtained by experimentation on animals?—I do not mean observation in the sense that I have witnessed experiments on animals, for I have not.

20682. Then what did you mean by observation, may I ask?—I was rather referring to observations that one had of pain in animals; and you yourself referred to one thing, one's observation in the shooting field.

20683. But it was no question of anæsthesia that came in there?—No, it does not come in.

20684. I think the question with regard to which you made the statement was whether experimentation on animals had advanced medical knowledge or not, and I took it down that you said that ordinary powers of observation and common sense enabled one to judge with regard to that question?—Observation in that sense means that one observes that the results, at all events of medical knowledge, are no greater than they used to be so far as one can see.

20685. But can you really claim that your observations with regard to that point ought to be allowed to have any great value with this Commission?—That is for the Commission to judge.

20686. Would you not admit that, in so far as your evidence has any value on that and other subjects, it is derived mainly from the fact that you have weighed the evidence bearing on the question?—To a certain extent, yes.

20687. Is it not mainly that as regards the question of anæsthesia, for instance?—I suppose that most men get their knowledge from reading unless they are engaged in actual experiments, and I admit that I have not been so engaged, and therefore my knowledge must come from reading.

20688. And, as we are all aware, many of the points are controversial, and therefore when you formed your opinion as the result of your reading it must have been because you weighed the evidence; is that not so?—You know how one forms opinions. One considers the credibility of the evidence.

20689. That is weighing the evidence?—I suppose that is weighing it, after a fashion.

20690. Now I want to put this to you. We have had here the President of the Royal College of Surgeons and the President of the Royal College of Physicians, and we have had representatives of most of the Universities, and a great many other men who are medical men, and I think I may say that they have been almost unanimous in their opinion that great advances have been made in medicine within the last 55 years?—In medicine, did you say?

20691. In medicine and surgery; and most of them hold that a great deal of that advance is due to experimentation on animals. Now the Commission has to weigh against that your evidence and the evidence of a smaller number of persons, most of them non-medical, and do you think that we would be violating the principles of common sense if we accepted the opinion of the majority in this case?—I think I may divide it into two things. First of all, I think that one distinguished surgeon who was examined before you said that he had rejected remedy after remedy until his remedies had been reduced to a number that he could count on the fingers of his two hands.

20692. Who was that?—Dr. Bantock.

20693. He is one of the minority?—I have no doubt he is one of the minority. And another answer that I might make to you is that the fact that the majority is in favour of a thing is absolutely no proof that their opinion is right. That has been shown in medicine more than in almost any other line—more even than in politics.

20694. (*Chairman.*) I do not think you need argue that to us; if I thought that that was the meaning of Sir John McFadyean's question I think we should all of us say we are not to be bound by the mere counting of heads of witnesses?—But Sir John spoke of the majority—would I put my opinion and that minority against the opinion of the majority.

20695. (*Sir John McFadyean.*) No, I beg your

pardon, the important part of my statement was that the majority was made up of men of great eminence in medicine, and presumably men qualified to form an opinion, as well qualified as the minority; and I therefore asked you whether you thought we should be violating the principles of common sense if we accept the opinion of the majority, not because they were the majority?—It is entirely for you to consider whether you will accept the opinion of the majority or of the minority, it seems to me; but what I was going to point out was that there was just as much common consensus of opinion in the medical profession years ago in favour of Lord Lister's antiseptic treatment of wounds; it was thousands to one in favour of it, I suppose; and as I understand at the present day Lord Lister himself has given up his antiseptic treatment.

20696. Have you taken the trouble to read the evidence which has been given to the Commission on that point?—Yes, I have. And I understand that he has given up his antiseptic treatment, and its place has been taken by the aseptic treatment.

20697. I might put the same question to you with regard to that. We have had evidence from distinguished surgeons here, who have scouted the view that you have just put before us, and we have really had no respectable evidence to show that what you have said is true.

20698. (*Chairman.*) If you only read the evidence on one side, that would be the conclusion that you would come to; but if you read the evidence on both sides, I cannot help thinking that you would come to the conclusion that it was not so?—But the evidence that I read was that of Lord Lister himself, I believe, in some speech that he made at Edinburgh.

(*Sir John McFadyean.*) Is that something that appears in the proceedings of this Commission?

(*Chairman.*) Yes, it has been referred to, and used on one side and explained on the other. There has been a good deal of evidence about it.

(*Witness.*) I think I could give some other instances if I may be permitted.

20699. (*Sir John McFadyean.*) Surely?—In former days the medical profession was uniformly in favour of blood-letting and strong purgative treatment by calomel. All that is gone. Inoculation for smallpox is another instance, and all that is gone, and yet it was supported by the great majority of the medical profession. My only point is that the majority are not always right, neither are they often right.

20700. I never suggested that they were, but may I ask whether that is the strongest reason that you can urge why in this case the Commission should accept the opinion of the minority rather than that of the majority?—As I said before, I have nothing to say as to the Commission accepting one view or the other. All my business is to place before the Commission our views, and the Commission will adopt which view they think the most consistent with common sense.

(*At this point Sir William Church took the Chair.*)

20701. (*Sir John McFadyean.*) On page 1 of your *précis*, in the second paragraph, you say that "The Association desires the total abolition of experiments on animals, believing the existing law which permits them, to be inhumane, immoral, and degrading, and a disgrace to civilisation. Further, the Association considers that the experiments which have been made under the present Act and which have been carried out at the expense of so much torture and so much animal life, have been practically unproductive of any beneficial results to the human race." When you spoke about experimentation upon animals as being degrading, I understood that you had in view principally the effect that it had on students. Is that so?—Yes, partly.

20702. Are you aware that under the Act, it is illegal to experiment on animals for the instruction of students, unless anæsthetics are used efficiently?—I think that anæsthetics must be used.

20703. And that the animal must not be allowed to recover; that in fact the experiment must be painless under the Act. (*The witness referred to the Act.*) I should have thought with regard to such an important point as that that you would not have had to refer to the Act?—There are six clauses and there are five provisos, I think. What the Act says is: "An experiment shall not be performed as an illustration of lectures in medical schools, hospitals, colleges, or elsewhere"; but then it goes on to modify that by saying: "Experiments may be performed under the foregoing provisions as to the use of anæsthetics by a person giving illustrations of lectures in medical schools, hospitals, or colleges, or elsewhere, on such certificate being given as in this Act mentioned," and so forth. There is nothing laid down as to the animal being killed.

20704. Do you suggest that it is possible under the existing law and under the Act to experiment on animals for the purpose of instructing students without the use of anæsthetics, throughout the whole course of the experiment and without killing the animal at the end of the experiment?—Without killing the animal at the end of the experiment. I do not see in this Act any provision of that kind.

20705. Will you take it from me that that is the state of the law; that this Commission is, I think, satisfied that that is the state of the law?—I am afraid I am not satisfied myself.

20706. Assuming that to be the state of the law, would you explain to the Commission wherein the experiments are calculated to demoralise and degrade the students?—I think that any body of young men witnessing a series of experiments involving horrible cuttings of animals, and practices of that kind would become degraded. I cannot conceive it to be otherwise. I should not like to put one of my own boys through it; that is all I can tell you.

20707. And you would say that, even if you were satisfied that the experiments involved absolutely no pain?—But as regards the killing of the animals afterwards——

20708. Would you please answer the question. I ask would you say the same if you were satisfied that the experiments involved absolutely no pain?—You mean if the animal continued under the anæsthetic the whole time and was killed at the end of it?

20709. I mean if the Act is complied with?—I am not prepared to say that the Act lays down that the animals under those circumstances should be killed.

20710. I did not ask you to give evidence on that; I think I may say that the Commission is entirely satisfied on that point. I ask you to assume that it is possible to experiment on animals for instruction without causing them any pain, and I ask you then whether you would give the same answer, namely, that such experimentation is calculated to degrade and demoralise the students?—Yes, I should.

20711. Would you explain why?—I have already explained why. I cannot imagine these horrible cruelties——

20712. Did you say cruelties?—You may say it is not cruelty because it does not give pain, but I do not at all agree. I think that if a number of surgeons were to tie me on a table, a perfectly healthy body, and to proceed to cut me up under the influence of anæsthetics, I should consider that was cruelty. And I consider that it is equally cruelty in the case of animals.

20713. Equally?—Yes.

20714. Observe that your argument seems to imply that man has no right to kill one of the lower animals?—I have very great doubts whether man has the right to kill one of the lower animals.

20715. I am quite content with that. I understand that another point about which you are in doubt is whether it is really possible to efficiently anæsthetise an animal under an experiment?—No, I do not say that it is not possible to anæsthetise an animal. I know perfectly well that it is possible to anæsthetise an animal under experiment. What I am in doubt about is whether as a uniform practice complete anæsthesia is obtained. I think there is a great deal of evidence that one has read in different places to the effect that complete anæsthesia is not obtained in these cases; that the animal wakes up, and so forth. I remember seeing a series of lectures by a man called Professor Crile, I think, in the course of which it was shown by his own admissions that the animal did wake up and move its legs and its body, and so forth.

20716. It has been given in evidence before this Commission that in most cases the experimenter has absolutely no motive for not anæsthetising the animal efficiently; that, on the contrary, it is greatly to his advantage as a demonstrator that the animal should be efficiently anæsthetised. What do you say to that

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argument?—I have no doubt that in a good many cases the animal is completely anæsthetised. But all I say is that there is evidence in a very considerable number of cases that the animals are not fully anæsthetised, and, more than that, that the anæsthetising is left to very incompetent hands.

20717. Does that mean that there is some difficulty in causing continuous efficient anæsthesia to animals like dogs, or is it that there is a deliberate evasion of the Act?—I take it that there are cases of both kinds. I take it that there are cases where there is a deliberate evasion of the Act. I am not prepared to say that 43,000 experiments take place without some cases of deliberate evasion of the Act. Surgeons are only human after all.

20718. Do you want to give any evidence on that, other than hearsay or supposition? You said that there were many cases you believed in which the animal was not sufficiently anæsthetised. Will you cite a case in point or half a dozen, let us say. You said many?—I cannot carry these cases in my mind, but I certainly have read of a good many cases in which the anæsthesia was not complete. The sort of evidence that I would put before the Commission is what I have put in my *précis*.

20719. Which *précis* is singularly lacking in detail. I was trying to help you to supplement it. Perhaps it is unfair to ask you for these cases?—I did not care to inflict all the cases upon the Commission.

20720. Will you kindly add six cases to your evidence when you revise the proof?—I know that this lady is not a lady who carries much weight with vivisectors, but Miss Lind-af-Hageby affirms that "the most unsatisfactory and obviously unreliable methods of anæsthetising are in vogue in the vivisectional laboratories." That is the kind of evidence.

The following cases were subsequently forwarded:—

1. *In the "British Medical Journal" for November 10, 1906, Dr. W. G. McCullum, recalling previous experiments, writes:—"It will suffice to recall to your memory the fact that in dogs, cats, rabbits, monkeys, and many other animals, complete parathyroidectomy is followed within a few days by the condition commonly described as tetany, in which convulsions, spasms, and rigidity of the muscles of all parts of the body render the animal almost helpless. Respiration becomes exceedingly rapid and laboured, profuse salivation occurs, and death supervenes in the attack, although occasionally the violent symptoms gradually give place to a stuporous condition which may last several days, terminating in death.*

2. *In Kiske's (Halliburton's) Physiology, 15th edition, p. 287, it is stated:—"Another experiment, originally performed by Salathe, can be demonstrated on a 'hutch rabbit.' If the animal is held by its ears with its legs hanging down it soon becomes unconscious; and, if left in that position for about half an hour, it will die."*

3. *In Dr. Crile's experiments upon "shock" the foot of a fox-terrier was crushed under "incomplete anæsthesia." Dr. Crile states also in his book that when he applied flame to a dog's paw "the animal struggled on the application of the flame."*

4. *Professor Schäfer experimented in drowning and resuscitating 36 dogs (2 without anæsthetics at all).*

5. *In a paper read at a meeting of the British Medical Association, at Toronto, Professor Cushney described a number of severe abdominal sections which he had performed upon dogs, cats, and rabbits, to which no anæsthetic, but merely morphia, urethane and paraldehyd, had been given.*

6. *In "The Journal of Physiology" for August, 1906, in a paper on "The Functions of the Thyroid and Parathyroid Glands," Professor Swale Vincent and Mr. W. A. Jolly, Assistant to the Professor of Physiology in the University of Edinburgh, record a number of experiments on monkeys, cats, dogs, prairie-wolves, badgers and rats from which the thyroid and parathyroid glands had been completely removed. They state: "Dogs, cats, foxes, and prairie-wolves frequently suffer severely and die."*

20721. That is the kind of evidence that weighs with you?—But I am not for a moment saying that the absence of anæsthetics is against the law.

20722. I have not asked any questions about that. I understood you to contend that the Act is being violated in many cases, inasmuch as animals are being experimented upon for the instruction of students without being under the influence of an anæsthetic all the time?—I did not speak of experiments for the instruction of students. I spoke of the general experiments.

20723. You will forgive me for saying that my question was limited to that?—I have not the smallest doubt that the same thing applies to the case of animals which are experimented upon for the instruction of students.

20724. Then it need not be qualified?—I have not the smallest doubt that there are many cases in which the anæsthesia is not complete.

20725. With regard to the ease with which animals may be anæsthetised with certainty, might I ask whether you have read the evidence given before this Commission by Mr. Hobday?—No, I just looked at it.

20726. You quoted his opinion, and with approval, I noticed, with regard to morphia?—I just looked at it.

20727. But you do think that he is a man whose opinion is entitled to a good deal of weight?—I do.

20728. Perhaps when you have time you would refer to his evidence given before this Commission, in which he told us that he had himself anæsthetised 1,200 consecutive cases, and that when he was asked whether he thought there was any difficulty in keeping an animal under complete anæsthesia for a long period he said no. He was also asked whether he thought there was any difficulty in deciding whether the anæsthesia was sufficient or not, and he also said no. And finally he was asked whether he administers the anæsthetic himself, and he said no, that his dog nurse does it—the attendant at the laboratory. I would like also to call your attention to the evidence which he gave with regard to the use of morphia. What was the source of your information with regard to his opinion concerning morphia?—He said that morphia was not an anæsthetic, I think.

20729. Where did he say that?—I understand that he said so.

20730. Have you read his evidence here?—I have looked at it. I have not read it. I cannot say that I have read it carefully.

20731. Then you must have seen the part that I have just been giving you the substance of with regard to anæsthetics. Do you attach much importance to that?—With regard to morphia not being an anæsthetic do you mean?

20732. No, to the evidence which I have just summarised with regard to the ease with which animals may be anæsthetised and kept under anæsthetics for a long period?—Yes, I am bound to believe what he says, that they can be.

20733. At Question 16462 he was asked: "Would you consider that morphia is a narcotic to a dog? (A.) Yes, I do consider it a narcotic. (Q.) Do you use it in your practice in the same way as medical men do in their practice with human beings for allaying pain? (A.) I use it largely for stopping pain. (Q.) And you find no difficulty? (A.) I find no difficulty in the dosage." Does that modify the opinion that you had when you came here with regard to the value of morphia for stopping pain?—I believe that morphia deadens pain. I am not sufficiently an expert to be able to understand the exact effects of morphia and the exact effects of anæsthetics generally, but I have always understood that morphia is used to deaden pain, but that it does not act as an absolute anæsthetic or stop pain unless indeed it is given in an excessive dose so as to induce insensibility.

20734. I point out to you it was you yourself who introduced Mr. Hobday's name to convince us that morphia was not of any use?—I do not know whether there is anything more in his evidence.

20735. Oh, there is a great deal more; but I think that you will find it very difficult to discover anything that seriously qualifies that?—If you will allow me I will make a search and see what I can find.

20736. In one or two places in your *précis* you speak about "hideous cruelties" being caused under the Act and "the torture of animals." Were you referring to any particular experiments that you know to have been performed in this country under the Act?—Setting aside the cases where anæsthesia is obtained, there are large numbers of certificates issued for experiments without anæsthetics.

20737. That is not the question. My question was, Do you know of any instances in which hideous torture has been caused under the Act?—Surely where

experiments are performed without any anæsthesia there must be hideous torture.

20738. You mean that if they are performed without anæsthesia they must cause hideous torture?—Yes.

20739. Does that sound logical? May they not be performed without anæsthetics because they are not calculated to cause torture? I should have thought that was logical?—The certificates, I believe, are not limited in that way.

20740. That is not my question. My question was do you know of any case in which hideous torture has been caused by any experimenter in this country under the Act?—I answer that by saying that it is impossible under the conditions specified, that hideous torture should not be caused.

20741. (*Mr. Ram.*) But I understand that you are not prepared to give any case. Is that the result?—It is very difficult to give any case of hideous torture, because, after all, the only fair witness as to hideous torture is the animal itself, and you cannot examine the animal.

20742. That is your answer?—You cannot possibly tell except from inference that the animal is suffering hideous torture.

20743. You assert that there is hideous torture, but you say that there is no means of proving it?—That would be so.

20744. (*Sir John McFadyean.*) Then what would be the use of inspection in such a case? I understand that you deprecate inspection; you do not think that inspection would be of any value. You would not rely upon inspection, at any rate?—For what purpose?

20745. For preventing cruelty?—No, because our view is that vivisection should be put an end to altogether, and therefore there would be no necessity for inspectors.

20746. On page 6 of your *précis* you select the ignorance of the medical profession with regard to influenza as evidence, I understand, of the fact that nothing is to be gained by experimentation on animals?—What I say is that in 150 years the medical profession has not discovered either the cause of influenza or how to cure it.

20747. That is a fact which I think is very much open to dispute; but assuming it to be true, I want to know why you set it forth, what influence did you expect it would have upon the minds of the Commissioners?—As an instance of the failure of the medical profession; and they have equally failed in the case of nearly all other diseases.

20748. Failure in what respect; failure to discover something by experimentation, was that it?—Well, they have not made their position any better.

20749. Did you want to prove to this Commission that it is impossible to learn anything by observation either?—No, I think you can learn more by observation; that, in fact, it is the only reliable source of information.

20750. Then would you explain how it is that this fact discredits experimentation but does not discredit observation? The disease has been open to investigation by observation and clinical research, as you say, since 1729. If you are correct in maintaining that nothing has been discovered in that interval, that seems to me to be seriously to the discredit of observation?—The medical profession, as I understand it, have discovered nothing about influenza, as to either its cause or its cure by observation; but since they adopted experimental methods they have not succeeded in discovering anything more.

20751. Might I ask was this put in your *précis* by your medical experts or is it something within your own knowledge?—Partly one and partly the other.

20752. Which is it, is it your part or the medical experts' part which assumes that you can experiment with human influenza on animals? Did you ever hear tell of human influenza, which is the disease you are referring to, affecting any animal other than man?—No, I never did, but I should have thought it could be communicated to animals.

20753. You "should have thought." That is not a thing about which you can ask us to accept your evidence.

20754. (*Sir Mackenzie Chalmers.*) You have no intuitive knowledge with regard to it. Have you inquired into it?—I have no knowledge that influenza has been communicated to animals.

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20755. (*Sir John McFadyean.*) I suggest to you that you have been singularly unfortunate in your selection of an illustration, because you have taken the one disease regarding which it is admitted that there is much ignorance, largely because we cannot experiment upon animals with it. But, if the facts were as you state them, they seem to me to throw no discredit whatever upon experimentation, but to throw great discredit upon what you call clinical research and observation?—These paragraphs are to show how little medical knowledge has advanced. So far as I personally am concerned, I give other instances. The only zymotic disease which I know that is materially less than it was in the days of my childhood, so far as I can see round me, is typhus fever, and typhus fever has been entirely got rid of apparently by sanitation.

20756. (*Mr. Ram.*) What about small-pox? Is that less or more?—If you are asking my personal view, in my opinion small-pox has been stamped out by sanitation also.

20757. Is there less or more than when you were a boy?—A great deal less; it has been stamped out by sanitation.

20758. It is not only typhus, then, that is less in degree since you were a boy?—I say it has almost disappeared.

20759. But not only typhus fever, but small-pox has also disappeared?—That is quite true; I think they have been both stamped out by sanitation.

20760. (*Sir William Collins.*) Was there a greater epidemic of small-pox in living memory than there was in 1871 and 1872?—I am afraid that my memory does not carry me back to that.

20761: (*Sir John McFadyean.*) At any rate, will you admit that, if it is a fact that human influenza is not transmissible to the lower animals, there is no force in your illustration as showing the futility of experimentation with a view to advancing knowledge with regard to human diseases?—I think that, so far as my illustration concerns the barrenness of our present medical knowledge, there is force in the illustration, but if influenza cannot be communicated to the lower animals, and therefore cannot be made the subject of experiments, I admit that under those circumstances one may be wrong in saying or implying in any way that the failure to discover the cause or the cure of influenza is in any way due to the failure of experiments on animals.

20762. What line of research would you suggest as probably being more fruitful?—That is a question that I am not competent to answer.

20763. I can understand that in that answer you hesitate to say observation. You have recommended it before, and clinical research. Would you say that here?—Generally so, but as to the exact methods I am not competent to answer.

20764. I do not mean the exact methods; but does not your argument seem seriously to discredit research observation?—It has shown that research observation, at all events, has never produced any effect as regards a knowledge of the cause and cure of influenza.

20765. Would you turn, please, to page 8 of your *précis*. In the fourth paragraph you say: "Nobody, of course, would suppose that, after the healing of the wound, it necessarily follows that the animal remains in a state of pain or suffering. But he" (that is Dr. Thane) "carefully ignores the truth that in extensive cutting operations, with excision of organs, there must necessarily, upon waking from anæsthesia, be very severe and prolonged pain until, that is, the severed tissues heal." That appears to me to almost amount to a charge of untruthfulness against the witness whose evidence you are quoting, and I want to put it to you that the subject is controversial. There is evidence on both sides, is there not? Do you think that there is evidence on both sides?—I have no doubt there is evidence on both sides.

20766. Why did you accuse the witness who espoused one side of carefully ignoring the truth?—Because he said, as I understand——

20767. He took the opposite view from the one you

approve of?—Not at all. He said that "when the wound has healed it does not follow that the animal remains in a state of pain or suffering." Everybody knows that when a wound is healed the animal does not remain in a state of pain or suffering; but what he ignores is the condition of the animal between the operation and the period of the healing of the wound.

20768. Is this expert evidence that you are giving?—I am simply reading.

20769. But you are criticising the evidence of this expert?—I am simply reading what he has said. He said: "When, in operations under these certificates, the operation has been performed and the wound has healed, it does not follow that the animal remains in a state of pain or suffering." There is no doubt that it does not when the wound is healed.

20770. You are assuming that it is an obvious falsehood to say that a considerable operation may be performed on an animal under anæsthetics without that animal suffering any pain when it comes out of the anæsthesia?—No. I make the charge that he carefully ignores the truth that in extensive cutting operations, with excision of organs, there must necessarily, upon waking from anæsthesia, be very severe and prolonged pain until the severed tissues heal. What he has ignored is the period, I understand, between the cutting and the healing.

20771. That is the period that I refer to. You want us to believe that a person who contends that in that period the animal may be free from pain is telling an obvious untruth. Will you allow me to read the evidence of Mr. Hobday, of whose opinion you express approval. Just please listen to this question and answer. It is Question 16352. "It has also been represented to us that extensive surgical wounds inflicted in physiological experiments on the lower animals must necessarily be painful when the animal comes out of the anæsthesia. Have you any evidence bearing on that question?—(*A.*) I have no experience of a physiological laboratory; I never worked in one; but my own experience is that the statement is absolutely untrue. I argue in this way: The wounds are the same, whether the human surgeon makes them with the scalpel or I make them. If they are done antiseptically they are the same in principle. . . . By far the greater majority of my wounds heal by primary union." You would perhaps think that that refers to operations which did not involve the cutting out of organs, but elsewhere in his evidence he told us that it applied to extensive operations in which he opened the abdomen and cut out organs. Does that not lead you to suppose that you have rather severely criticised this witness when you charge him with carefully ignoring the truth?—No, I do not think so, I am sorry to say. You asked me just now whether I believed that that was the case. I simply say that I do not believe that the animal does not feel pain between the time when it comes out of anæsthesia and the healing of the wound.

20772. That is not the question. It is a question of whether, when the opinion is not unanimous on a point like this, you are entitled to charge a person who adopts an opinion opposite to yours of carefully ignoring the truth?—I am absolutely justified in saying that the answer I have quoted above carefully ignores the period between the waking from anæsthesia and the healing of the tissues. As I understand in the evidence that you read to me, Mr. Hobday maintains that under certain circumstances, in certain operations and under certain conditions, there is no pain during that period. So far as that is concerned, I simply say that I could not bring myself to believe it.

20773. It is not a case of asking you to believe it. Can you contest the truth of it? You do not pretend that you have any first hand experience as to the result of healing of large wounds after an operation?—I do not understand what you mean by contesting the truth of it. I do not contest that that is his opinion.

20744. But I ask you, have you any experience of your own to show whether things are as you hold they are?—But there are a certain number of things that one regards as practically self-evident to oneself which one has no experience of.

20775. Would you mind answering the question—have you such experience?—You asked me whether I have operated——

20776. No, I did not?—That is experience.

20777. I asked you whether you had had any opportunity of forming an opinion from observation as to whether this is true or not true?—I really do not understand the purport of the question, but what I have felt always when I have cut myself severely is that I have always had pain between the time of the cut and the healing of the wound. Neither can I conceive that it is possible that it should not be so.

20778. Were these wounds of yours treated aseptically and sutured?—Aseptically, certainly, yes, but not antiseptically.

20779. But the point is that you hold one opinion and the witness whom you criticise holds another opinion, and his opinion is supported by that of a man of great experience, Mr. Hobday. I suggest that when that is so you should not accuse the witness of having carefully ignored the truth?—I really do not know that I am quite responsible for the verbiage.

20780. (*Sir Mackenzie Chalmers.*) I am afraid that I have a few questions that I must ask you, because we naturally attach considerable importance to your evidence, and there are some points that I should like to clear up that I do not quite understand. Your society wishes totally to abolish vivisection?—Yes.

20781. You have given us two definitions of vivisection. When your society is anxious to abolish vivisection, do you mean cutting operations, or do you mean any animal experiments?—When you say animal experiments, that was one of the grave difficulties we had in mind when we tried to draft a Bill. The difficulty as to experiments is that there may be experiments in feeding or other innocuous experiments.

20782. Taking the temperature is an experiment?—Yes, an innocuous experiment. But we wish to abolish all experiments, cutting or inoculation.

20783. Inoculation with any disease?—Yes.

20784. Would that include, for instance, the use of animals which are used for the preparation of vaccine, and the use of animals which are used for preparing antitoxin?—Yes.

20785. You would prohibit those by law?—Yes.

20786. For instance, calf vaccine?—With regard to calf vaccine, I wish to say, as I said before, that I do not consider that vaccination is a subject on which I am prepared to give any evidence, unless the Commission wish to hear my own personal opinion. For my Association I give no evidence.

20787. Your personal opinion would, I think, be of interest and importance. Do you object personally to using calves for the preparation of vaccine?—I object to vaccination altogether.

20788. And also to antitoxin for diphtheria?—Yes, I do so far as my knowledge goes, and so far as my reading goes.

20789. You have seen diphtheria cases; I suppose we all have?—I have seen one or two in my own family, and the patients recovered.

20790. Have you seen any where antitoxin has been used?—No.

20791. I may take it, then, that the main object of your Association, apart from drafting considerations, is to abolish every experiment with an animal, whether by vivisection, inoculation, or otherwise, which causes pain?—Yes.

20792. Would you carry that principle so far as to prohibit mutilations of animals for commercial purposes?—I do not know what mutilations you refer to.

20793. For instance, every horse in England which is not used for stud purposes is gelded, usually without anæsthetics. Would you draw any distinction between an operation for the purpose of increasing knowledge and an operation for commercial purposes?—Yes, I should. Besides, the one is not an experiment, the others are experiments. I should draw a distinction between them.

20794. You would draw a distinction between an experiment for the purpose of increasing knowledge or alleviating suffering and an operation for commercial purposes; would you interfere with the commercial purposes or not—for instance, gelding a horse?—I should not interfere with that. That is not an experiment.

20795. Therefore, it being for a commercial purpose and not for a humanitarian purpose, you would allow

it?—I am hardly prepared to admit the word "humanitarian."

20796. An experiment is for a humanitarian object, is it not—for the purpose of alleviating suffering in the future?—It is supposed to be.

20797. And gelding a horse is for purely commercial purposes—to make it a more saleable animal. You would admit that, but not the other?—I should admit that. It is not an experiment. It is a matter of long custom. I should not, for instance, put down circumcision among the Jews.

20798. Or require it to be done under an anæsthetic?—I really do not know that it is a sufficiently serious operation. I am afraid Moses did not use anæsthetics.

20799. I may have misunderstood you, but I thought that you rather complained of Dr. Pembrey doing circumcision without an anæsthetic?—I did not say anything about that; it was merely mentioned casually. I do not complain of circumcision being done without anæsthetics; it is not a sufficiently serious operation, and it is not an experiment.

20800. Take the case of spaying sows. A great many animals are spayed; you would leave that; that is outside?—That would be on the same footing as gelding horses.

20801. Outside altogether?—Besides, it is not an experiment, neither is circumcision, and it is not an experiment on animals either.

20802. Still, I take it that you would not allow purposeless mutilation of an animal, even if it was not experimental?—That, of course, comes under different legislation, and it comes under a different category; it comes under the Prevention of Cruelty to Animals Act; it is quite a different thing, and it is amenable to the ordinary law of the land.

20803. As regards an increase in the number of experiments, I have no doubt that you have looked at the last Return?—I have not seen anything after that 43,000.

20804. The Return ordered by the House of Commons to be printed on the 13th of May, 1907; you have seen it, no doubt?—I thought that was the last; it is for 1906. 43,000 is the last, is it not?

20805. Yes. You are aware that what is called vivisection proper only accounts for about 2,700 of the experiments, and the rest are under Certificate A, which is for inoculation experiments?—I thought there were 21,000 inoculations only, and 6,000 for Government Departments.

20806. No; under Certificate A 43,287 inoculations is the exact number. What I was going to ask you is this. Do you accept Dr. Thane's statement in the report: "In no case has a cutting operation more severe than a superficial venesection been allowed to be performed without anæsthetics"?—I am bound to accept it if Dr. Thane says so.

20807. Does that relieve your mind in any way? Is that any relief to you?—If that is the case, how did it come that Colonel Lawrie saw a dissection of the neck without any anæsthetic?

20808. I am afraid I cannot give evidence; I can only suggest it to you in a question?—Whatever Dr. Thane says, there is a very serious and very painful operation which a witness says is done without any anæsthetic.

20809. I can suggest the answer to you, but only that; we called a witness before us, Dr. Shore, who performed the operation, and if you read his evidence I suggest that you will see the explanation which (whether you accept it or not) was laid before us. You have not seen it, perhaps. He performed the actual experiment that Colonel Lawrie saw. It would take you some time to read it. I can only suggest to you that the Commission at once investigated that statement made by Colonel Lawrie, and we called a witness who gave the explanation. You will judge of that explanation when you read it?—Yes.

20810. As it is late, I will leave out some things that I should have liked to have asked. In your *précis* you speak about certificates given by the Home Secretary. Are you aware that the Home Secretary does not give certificates?—Yes, I know he does not; that is a mistake.

20811. May I put it to you in this way: Those certificates are given by certain eminent authorities?—Yes.

20812. They go through a considerable routine before they are allowed to come into operation, and the Home Secretary has the power of disallowance?—The section says: "The Secretary of State may at any time disallow or suspend any certificate given under this section."

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20813. You agree that a power of disallowance is not the same as the power of making?—A power of disallowing would only be exercised in the event of serious complaint being made against the experimenter.

20814. I must ask you on that, have you made any inquiries as to the process which is gone through with regard to these certificates?—As to whether they are ever disallowed, do you mean?

20815. No, I mean as to what happens before they are allowed to come into operation. Have you made any inquiry as to that?—I thought that had been placed before the Commission in sufficient detail by Mr. Coleridge, so that I did not think it necessary.

20816. Do you know, at any rate, that five pairs of eyes have to examine a certificate before anything is allowed to be done under it?—Yes, I know that a considerable number have, but as I say I did not take up the question of certificates in this evidence because I agreed with the evidence given by Mr. Coleridge on this part of the question, and so I thought that would be sufficient testimony.

20817. Do you know what inquiry he made upon the subject?—I believe he is very familiar with it.

20818. With certain aspects of it perhaps. You referred to the Public Authorities Protection Act, 1898. Why do you say that that affects the case of experiments under the 1876 Act; what is your ground for saying that?—I believe that the persons holding these certificates are for the purposes of the Act decided to be public authorities.

20819. I never heard of it. I should like some authority for it. What is the authority for it?—I do not know; but you will observe that in answer to my question in Parliament the Home Secretary did not canvass that at all.

20820. Did he not?—I referred in my question to the Public Authorities Act.

20821. What was his answer; have you got his answer?—I have got it all down here. I have read it to you.

20822. It is news to me, certainly, that that Act applies?—It does, though.

20823. May I suggest to you that a publican has to have a licence, but he does not come under the Public Authorities Protection Act, although he is called a publican?—It does, though. The Home Secretary added that contraventions of the Act were reported to the Home Secretary as soon as they were discovered, so that the institution of proceedings for performance of illegal experiments was not dependent on the date of the Report. He did not for a moment say that the Public Authorities Act did not apply. I asked him the question, and my question distinctly referred to the Public Authorities Act.

20824. I should like to have the words of that answer because I rather think I am responsible for it?—I can give you the whole question and answer out of Hansard, and then you will see.

20825. Of course, he replied to it at the time, but I do not think he referred to that Act. A street hawker you see has to take out a licence, and a gamekeeper has to take out a licence, but you would not say that they come under the Public Authorities Protection Act, would you?—No, but I understand that the definition of public authorities in the Act covers the holders of licences.

20826. Not so far as I read it, but we need not discuss that?—Is it not so?

20827. No, I think not?—The Home Secretary had his attention drawn in the question to the Public Authorities Act, and he did not in his answer say anything about it, or whoever drafted the answer for him.

20828. What I was going to suggest to you is this: You are a magistrate, are you not?—Yes.

20829. Do you know Section 11 of the Summary Jurisdiction Act?—No, I do not know it.

20830. That is the section which provides that all summary proceedings, in the absence of special statutory direction, must be begun in six months before the

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magistrates. Is not that the enactment which protects the public generally?—I understood that it was the other. I referred to that in the question to Mr. Herbert Gladstone, and he did not seem to question it. It does not much matter which it is if it has the same effect.

20831. Assuming for the moment that the Public Authorities Act does apply, the repeal of it would not help you, would it, having regard to Section 11 of the Summary Jurisdiction Act?—Not if Section 11 of the Summary Jurisdiction Act has the same effect. You do not want to repeal the section in either case; all you want to do is to take the particular offence out of the section.

20832. That is to say, to put licensees under the Act in a worse position than the rest of the population as regards summary offences?—How is that?

20833. You have to prosecute an ordinary person who commits an offence within six months; but in this case you would take away that protection?—You might put it in that way; but you have also to put it in this way, that the method in which the Act is administered makes it impossible for prosecutions to be instituted in consequence of the block put upon them by either the Public Authorities Act or the Summary Jurisdiction Act, whichever it is, as the case may be, and therefore practically when you say in the Act of Parliament, the Cruelty to Animals Act, 1876, that no prosecution can be instituted except with the consent of the Secretary of State, as a matter of fact no prosecution can be instituted at all, whether the Secretary of State consents or not, under the existing law. That is what we argue.

20834. You argue that until the returns are published the Secretary of State knows nothing about what has happened?—Quite so. Our only source of information is in those reports.

20835. Do not you think that the Secretary of State occasionally has information before it is able to be collected and published?—Yes, I know it is a very heavy business.

20836. I am rather puzzled by your evidence generally. I am not quite sure how far your evidence represents what I may call a brief given you by experts. Where does your own evidence end, and where are you speaking to your brief?—I should be puzzled to tell you myself.

20837. (*Mr. Ram.*) Is the *précis* your own creation?—Some of it is.

20838. (*Sir Mackenzie Chalmers.*) I will tell you why I ask the question. I attach great importance to anything that you tell us yourself, but I do not attach much importance to anything that your experts tell you, unless I know who they are and what their means of information is. You cannot, I am afraid, separate for me what is yours and what is, so to speak, your brief?—It is all my brief, but of course the part with which I am specially familiar is the question of plague. I know something about the whole question, but the part I am specially familiar with is the passage of the Act through the House of Lords and the House of Commons, and the strong point which I made as to the hurry with which it was passed through the House of Commons.

20839. As you appear on behalf of the Association, may I ask you this. I understand that there were experts who helped you to prepare this statement?—I believe several, but I do not know them all.

20840. But who were the two you had communication with?—I had communication with Miss Kenealy.

20841. Who has given evidence here herself?—Yes.

20842. And who was the other?—Dr. Bourchier, I think.

20843. Who is Dr. Bourchier?—Miss Bourchier.

20844. Is she a practising medical woman?—Yes; she has been.

20845. Miss Kenealy has retired from practice, I think; she told us she was now a journalist and not a practising doctor?—I believe she is not in active practice.

20846. Miss Hageby, I suppose, you have not consulted with?—I do not know her even.

20847. But it is Dr. Bourchier and Dr. Kenealy?—Yes, and the members of the executive of the Association.

20848. Do you know at all what scientific qualifications they have?—No, I do not know.

20849. I just want to ask you one or two questions about plague. You have taken great interest in plague?—Yes, I have taken some.

20850. Have you read the reports by the Scientific Commission appointed by the Government of India, which have just been published; Dr. Martin's reports?—I have not seen them yet. I do not know whether they are issued as Parliamentary Papers.

20851. They are not issued as Parliamentary Papers, but they have been out for some months, some of them?—You see, my information comes largely from Parliamentary Papers. I get those.

20852. I am afraid those are often rather out of date, are they not?—And anything I get furnished with by others who are interested in the question.

20853. Have you read Dr. Martin's evidence to us. He was Chairman of the Plague Commission?—No, I have not.

20854. It bears a great deal on the statements that you have made to us to day, but if you have not read it it is hardly possible to discuss it with you, I am afraid?—Is it issued?

20855. It is in our Third Report?—Then I am sure to have it.

20856. Because these statements of yours—I do not know if I am putting it too strongly—are in every direct opposition to some of the statements contained in that report?—As I understand, they are in opposition.

20857. But you know that that Commission was a very important Commission, consisting of very eminent men?—Yes.

20858. Do you think that no weight is to be attached to their findings?—I do not say that no weight is to be attached to their findings, but I do say, as it appears to me, there is an exceptional case of an epidemic of plague, an epidemic which certainly has become endemic. And in what way does this epidemic differ from previous epidemics? It differs in the fact of the use of these sera, that is all; otherwise the conditions are better.

20859. Might you not apply the same line of reasoning to different earthquakes? You have the same conditions, but one earthquake is more severe than another?—I do not admit the analogy.

20860. Then may I put this as an analogy to you: A lifebelt will save you from drowning; but you read in a particular year, say this year, that more people have been drowned than last year. That would be no argument against lifebelts unless more lifebelts were used, would it?—No.

20861. Your argument would rather come to this, would it not: A greater number of deaths from drowning have occurred this year than last, therefore lifebelts are of no use. What you have got to take is the number of people inoculated by Haffkine and the number of deaths among the people so inoculated?—I know that, and I should have thought from what I could see that those statistics were quite valueless. I should have thought they were of very little value, because you see you take a certain number of people and you know the number of people who are inoculated, but you do not know the number of people who are not inoculated, not absolutely.

20862. In the jail case there was, of course, a comparison?—Yes, we know.

20863. How do you account for the very small propagation of plague among Europeans in India who are freely inoculated?—Has that anything to do with it? A good many Europeans are said to be inoculated. Have you considered why it spreads so much more among the native population?—I should be inclined to attribute it to cleanliness, as a non-expert.

20864. Are you aware that the Plague Commission who were mingling with plague night and day, were all inoculated themselves and all escaped?—I do not know that. They escaped a serious attack of plague, but I presume that they had plague in some shape.

20865. But still, if a very mild attack of plague will save you from a fatal disease, is it not worth while?—Yes, I do not deny for a moment that inoculation may give a mild attack of plague and give immunity for a certain time. All that I desire to try to enforce is that these people who are inoculated

are just as infectious as other people, and may give the plague to others.

20866. But do you say that plague is infectious. There is no danger in going through a plague hospital?—That is one of those points, I believe, which is not yet cleared up.

20867. There is one form of it, the pneumonic form, which is supposed to be infectious; but have you considered how far plague is infectious or contagious, or not?—Of course I have generally never observed cases of plague, but so far as one's reading goes, one is in very great doubt as to what the means of communication of plague are. What between rats and fleas, and all the rest of it, one does not know any more than one does with many other diseases.

20868. There is only one question more that I wish to ask you. How do you account for it (it is a point that presses upon us), that all the heads of the medical profession are unanimous in favour of the necessity of animal experimentation. We have had the President of the Royal College of Physicians, the President of the Royal College of Surgeons, representatives of the Royal Society, and representatives of the great universities, and the whole, what I may call heads of the profession, unanimous. How do you account for that?—The head of the Royal College of Surgeons himself was not, was he?

20869. If you read his evidence you will see that he was?—I was under the impression that he said that he did not consider it necessary.

20870. He only said that it was not necessary for students to perform experiments, but if you look at his evidence you will see that I am right in saying that the Royal College of Physicians, the Royal College of Surgeons, the Royal Society and the great universities are unanimous in favour of the necessity of experimentation. How do you account for that?—Because I think they look at it through coloured spectacles. They look at it through medically coloured spectacles. I do not mean to say that they are not honestly biased by considerations of so-called humanity.

20871. But are they not the people who are in daily personal contact with disease, and therefore have more means of judging than the rest of us?—Decidedly.

20872. (*Mr. Ram.*) Following up what Sir Mackenzie Chalmers has just asked you, can you suggest any reason why the heads of the profession and those who represent the universities and Royal bodies, should wear, as you expressed it, glasses of one colour, and that those who wear glasses of another colour should be few and not distinguished?—I was not drawing a distinction as regards glasses.

20873. I want to draw it rather?—I was not drawing a distinction as regards glasses between those medical men who are in favour of experiments upon animals and those medical men who are not. I was drawing a distinction between the medical profession generally and the general public.

20874. But I am rather drawing a distinction, if I might for the moment, between these two classes of people who have come here and given evidence before us, either directly, as the heads of the profession have done, or indirectly, as some others have done, and are doing now through you. How do you account for the fact that all that are eminent are on one side and those that are not eminent and few in number are on the other side?—They are not undistinguished on the other side. Surely Dr. Bantock is a very distinguished and very successful surgeon.

20875. Dr. Bantock gave evidence in favour of the necessity of vivisection?—No, he gave evidence practically opposed to it.

20876. No. I will read you the passage?—I know that there is a passage or two at the end of his evidence in which he made certain concessions, but the general tenor of his evidence was against it.

20877. Have you read Dr. Bantock's evidence all through?—Yes.

20878. Then you know the passage that I am alluding to?—Yes, you are alluding to a passage towards the end of his evidence. I could not tell you *viva voce* without referring to it.

20879. It justifies me in citing him also as an eminent man in favour of the necessity of experiments on animals?—I should not read it so.

20880. I will refer you to it if you like, but can you give me any answer to the other point that I put to you: Why is it that all the most eminent men advocate the necessity of experiments, if it is a fact, as your Association thinks, that experiments are useless?—I really could not say why they all do. I imagine that they are perfectly honest in their view, but I imagine that the general public, which we represent, and the Parliamentary feeling, which we represent very largely, have a right to our opinion. We look round us and we do not see any great advantage having been gained by these experiments, and we do not see that disease is less or that the population is becoming less degenerate.

20881. But on what ground do you ask the Commission to set aside the evidence of the most eminent persons who have made this subject a study, in favour of the evidence of inexpert persons who have not made this subject a study?—I have not asked the Commission to set aside that evidence. I have asked the Commission to consider the evidence that we give.

20882. We certainly shall do that with the utmost care. You say in your *précis*, on page 6:—"Our medical scientists are by no means driven to use experiments as their only means of research. They have other (and confessedly superior) methods of investigation." What is a confessedly superior method?—The clinical method; that is our view.

20883. By whom is it regarded as confessedly superior?—I presume at all events that they are confessedly superior in the sense that those methods of investigation are open to every medical man (the others, I understand, are not), they are part of the daily practice of every medical man.

20884. Is that the meaning of the words "confessedly superior"?—I cannot tell how those words got there exactly.

20885. Is that the meaning that you put upon them?—At all events, you would imagine that they were equally good, since all such knowledge as we have seems to have come from clinical research, and not from experimental research.

20886. In the next sentence you say:—"The present barren state of medical knowledge compels us to conclude that long years of vivisection have added perhaps not one assured and certain weapon to their armament. To any man of common intelligence this barren state of medical knowledge is obvious." You think that that is a true description of the present state of medical knowledge?—I am inclined to think—if I may say so, with all respect—that it is a true description.

20887. May I point out that, except so far as in comparatively recent years, medical science has been helped by experiment, it was wholly dependent on clinical observation?—Yes.

20888. It does not look, then, as if it had benefited very much by clinical observation if that on which it relied for all those years has left us in the present barren state of medical knowledge that exists to-day?—For many years before vivisection had been in vogue, long before the Cruelty to Animals' Act, 1876, and we at all events (I am speaking for the Association) do not find that any advance has been made beyond the small amount that has been gained by clinical research in all these years during which vivisection has been practised.

20889. Do you think that medical science is no more skilled in combating disease now than it was, say, 50 years ago?—It depends how many years ago you refer to.

20890. I said 50 years ago?—I do not know whether that was in the days when doctors used to hang the rooms in which there were scarlet fever patients with scarlet curtains as a means of keeping away the disease.

20891. Seriously, I should like your opinion as an answer to my question. Do you mean that medical science in your opinion is really in no degree more advanced to-day in combating disease than it was 50 years ago?—I do not think that medical science, so far as my recollection goes, or my observation goes,

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has much advanced since the time when I was a boy. I think sanitary science has improved enormously.

20892. Do you think that Lord Lister has done nothing for us to ameliorate pain and to assist in overcoming disease?—Whatever advantageous changes may have resulted in that way, what I rather meant was that the causes of diseases have not been ascertained so as to get rid of the causes; nor have the cures of diseases been ascertained. If you could only find out what the cure of a disease was you would get rid of the disease.

20893. If the causes of disease are still not ascertained, does it not seem reasonable that experiments should be made to endeavour to find out what the causes are?—Experiments are made with the object of finding out what the causes are, but they never have discovered from the experiments what the causes are so as to be able to prevent them.

20894. That is your opinion, from reading the state of science up to to-day?—That is my view, because the prevalence of disease appears to be very much the same as it always has been.

20895. Do you suggest that operations which to-day are performed, I may say daily, with a very small percentage of deaths—take one that we hear so much about, for instance, the operation for appendicitis—could have been done 50 years ago with any hope of success of saving the life of the patient?—I think that great advances have been made in surgery, but I believe those advances to have been made by operating on the dead body and observation of the dead body, not on animals. One gets rather confused between the medical art and surgery.

20896. (*Sir William Church.*) Might I be allowed to interpose one question? What makes you think that that improvement is due to operations upon the dead body now? How do operations on the dead body differ now from what they were 50 years ago? Are they more frequent, do you mean?—No, I did not mean that. I meant that our knowledge had come from operations on the dead bodies of human creatures rather than upon the living bodies of animals.

20897. That would not tell us anything as to the treatment of the wounds afterwards?—No, I admit that a great advance has been made in the treatment of wounds afterwards.

20898. I did not understand what you meant by operations. I thought you meant operations in teaching students and surgeons how to operate. You did not mean that?—No, because any improvements in dealing with wounds are surely sanitary improvements rather than medical.

20899. May we take it from you that you disbelieve wholly and entirely in the germ theory of disease; that you say is a myth?—I do not say that. I say that it is not proveable.

20900. You would not go so far as to say that it is altogether a myth?—No, I would not.

20901. (*Mr. Ram.*) Then you said that you objected to any kind of experiment on a living animal because it is demoralising to the operator?—Yes.

20902. Do you think it is demoralising to a man to dissect a dead animal?—No.

20903. Take the case of an animal under an anæsthetic from which it can never recover, which is wholly unconscious under that anæsthetic. Why is it more demoralising to a man to examine that animal than to examine an animal which is dead, which you have just told me is not demoralising?—Because the whole process of examining living animals, to a layman, the cutting and wounding and mutilation of living animals is at all events the most sickening process one can possibly imagine. It would be most sickening to me. I feel it would be most demoralising to me. If I could get myself into a condition so as to view without any pang or any feeling of disgust a long series of experiments on a number of animals one after another, day by day, week by week, month by month, tied upon a table and cut to pieces, by the time that I was at the end of the series of experiments I should feel that I was one of the most callous, demoralised brutes in the whole scale of creation. I am putting the instance of what I should feel myself.

20904. That is what I may call an *à priori* idea of what you would feel yourself?—Certainly.

20905. Have you any instance that you can give us of any person who to your knowledge has been demoralised or adversely affected morally after having performed experiments on living animals under anæsthetics?—I do not know exactly what you mean by being adversely affected morally.

20906. I mean in your own word demoralised?—Their sense of humanity with regard to animals and so forth, I should think, would be very strongly affected.

20907. Can you give me a concrete instance of any man in whom you know that that has been the effect?—I do not think that I could give you any specific instance of any man who by indulging in experiments on animals has become a thief or a murderer.

20908. I am not suggesting that?—Or even in other senses immoral.

20908A. I suggest in your own words callous or inhumane?—I answer that, from the analogy of my own feelings. I cannot imagine that any man can go through all those experiments without becoming demoralised.

20909. The question was, can you tell me of any man who, to your knowledge, has been so adversely affected?—Of course I cannot. You know very well that I cannot pick out any man, and more than that, if I did know of a man I should not be able to tell you.

20910. I hope you would. I take it that the answer is that you do not know any concrete case of that kind?—Certainly.

20911. What would you say to this case: Supposing that the Commissioners, after hearing all the evidence—a great deal of it on both sides—and after weighing it all very carefully, came to the conclusion that experiments have been of very great use to humanity, should you still urge them to advise that there should be no experiments at all allowed merely on the ground of possible suffering in a certain number of small cases in those experiments to some animal?—Your premise is that the Commissioners have come to the conclusion that such and such is the case. I should not urge the Commissioners to put in their Report anything which, after due deliberation, they concluded to be wrong.

20912. I am not suggesting that. I say supposing that the Commissioners, after weighing the evidence on both sides find that experiments on animals have been and are productive of great good as a means of the amelioration of pain to human creatures, should you still say on the other grounds that you have urged—grounds of morality and humanity, and so forth—that all experiments ought to be made illegal?—If, as you say, the Commissioners have concluded that such is the case, it is no use my offering any evidence.

20913. You may take it from me that they have not concluded that yet. I want the assistance of your mind in the matter?—But my duty is to place before you this evidence, and for you to weigh and consider and see how far it leads you to modify any views that you hold already.

20914. You said that you objected to all experiments on the ground that they were inhuman and useless—unproductive was your word. I am supposing that the Commission are bound to find as a matter of evidence that they are not unproductive, but are useful and have been of great good to mankind in alleviating suffering. Should you still say that although useful, they ought to be made illegal on the ground of inhumanity or immorality, of which you have been telling us?—I can only place my own view before the Commission. My view is that they are useless, unproductive, and inhuman. But you ask me, supposing that the Commissioners conclude one thing, would I advise them to modify their conclusion? That has nothing to do with me. All I have to do is to place my views before the Commission for them to consider.

20915. I want to go as far as I can with you to get the benefit of your assistance. You put it on the three grounds that it is immoral, inhuman, and useless. Supposing that the Commissioners come to the conclusion that it is not unproductive, but on the contrary is useful, ought they still, in your opinion, to endeavour to make it illegal on the ground that it is immoral or inhuman? It is a very plain question?—I could not say: that would be for the Commissioners to judge. I do not pretend to be the *arbiter pugnæ*.

20916. Reference was made just now by you to Mr. Morris; you quoted him as saying that in his opinion

students ought not to be allowed to do experiments, and that you were also under the impression that he had said that experiments generally were unnecessary. I will refer you to his answers on page 41 of our Report, on the 18th day, beginning at Question 7658:—"Experiments on animals in relation to aseptic surgery and the healing of wounds have been of material advantage, in your opinion? (A.) Most material. I quote Lord Lister. We could not have arrived at the knowledge we possess at the present day had it not been for experiments on animals. (Q.) You do not say that that knowledge has arisen from experiments on animals alone? (A.) No. (Q.) But there are other modes of investigation which have been going on in research? (A.) Certainly. (Q.) Chemical investigation, for instance? (A.) Yes, each of the forms of investigation has been essential, but experiments on animals have been equally, if not more, essential or important than the other forms of investigation. (Q.) And it is not easy, in your opinion, to separate the advantages which have been derived from each particular source? (A.) It is impossible. (Q.) It is your decided opinion, as an experienced surgeon, that vivisection has been essential? (A.) I have no hesitation in expressing that opinion." I wanted to bring that to your mind, because I think you are under the impression that Mr. Morris, whose opinion, of course, everybody greatly values, had said that experiments were unnecessary?—As a matter of fact, he himself has never performed an experiment.

20917. He never has. I think there is only one other matter that I need trouble you with, and it is this. You spoke about experiments which would come under Certificate C, that is, experiments for the instruction of students?—Yes.

20918. I want to draw your mind to this because it was not present to your mind, I think, when you were giving evidence before. Certificate C has to be signed by one or more eminent persons such as the Prsident of the Royal Society, the Presidents of the Royal Colleges of Physicians, and so forth, who state:—"We hereby certify that in our opinion the proposed experiments are absolutely necessary for the due instruction of the persons to whom such lectures are to be given with a view to their acquiring physiological knowledge, or knowledge which will be useful to them for saving or prolonging life or alleviating suffering." Then the note to that certificate is this:—"The animal must during the whole of the experiment be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain; and the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered"; and reference is made to Section 3 of the Act, sub-sections (3) and (4). I want to point out that that can only take place under what are termed the operative sections of the Act, and not under the exceptions. Those words that I have just read are quoted from the Act, and are not included in the savings which subsequently come in the Act, which have been termed relaxations of the Act?—You mean the provisos.

20919. You see this is the operative part of the Act?—Are you speaking of Section 3, sub-section (4)?

20920. I am speaking of Section 3, sub-sections (3) and (4)?—Sub-sections (3) and (4) can be nullified by proviso 3.

20921. But as a matter of fact, under the certificate of which I was speaking they are not only not nullified but the certificate is granted only under sub-sections (3) and (4), and admits of no nullification. Do you follow?—Yes, I see; but there is also sub-section (5).

20922. No, I am speaking of sub-sections (3) and (4), because that is the matter to which Certificate C relates.—Of course, and both those sub-sections (3) and (4) can be nullified by provisos 2 and 3.

20923. But the operation can only be done, if at all, under Certificate C, and Certificate C, as I have just pointed out, is limited to those two sub-sections which I have just quoted?—That certificate combines those two points.

20924. (*Dr. Gaskell.*) Do I rightly understand you that this *précis* of yours was partly drawn up by you and partly by others?—It is a united effort.

20925. You make a good deal of Dr. Pembrey's evidence, I see?—Yes. *Sir G. Kekewich, K.C.B., M.P.* 18 Dec. 1907.

20926. Is that your part?—Partly.

20927. But you put it in your *précis*. Are you responsible for it?—I should not like to disclaim the responsibility.

20928. Are you responsible for the correctness of it?—I do not wish to evade my responsibility for all of it. All I say is that I cannot be supposed as a non-expert to have put together the medical part.

20929. I am simply asking you, are you responsible for the accuracy of the statements that you have given us?—I do not wish not to be.

20930. Then you are responsible?—I take the responsibility.

20931. Then, in the first instance, you say, on page 9 of your *précis*, that Dr. Pembrey "expresses the opinion that the infliction of suffering is a meritorious act"?—Yes, he expresses the opinion that it is a meritorious act.

20932. I have read through Dr. Pembrey's evidence, and I have not been able to find that statement. I have not seen that he has expressed that opinion?—If I cannot find it, I imagine that "expresses the opinion" is a bit of loose writing; "he apparently believes," I should have thought would have been better.

20933. Quite so; it is a piece of loose writing in the part for which you have just accepted the responsibility?—Yes.

20934. But which does not necessarily give the actual opinion of Dr. Pembrey?—No.

20935. I am sorry that you accepted the responsibility?—I am quite ready to accept the responsibility.

20936. I will go a little further, please, to the next thing, which is at Question 14149. That is a statement of Dr. Pembrey's of which you have made a good deal. I will read it to you. Dr. Pembrey says:—"I will be perfectly straightforward. I say that you should not inflict pain if you can obtain the knowledge in any other way; but I say that even where there is an operation the pain there is of a protective nature; it may produce syncope, and therefore less sensation of pain. Further, I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of syncope. . . . . If you give them anæsthetics you are introducing a complication which you could remove, and therefore without anæsthetics you actually save life and actually diminish the infliction of pain." For the accuracy of that you are ready to vouch?—I head this as an except from Dr. Pembrey's evidence. I did not actually write it out myself.

20937. Your comment was that this was an extraordinary doctrine?—Yes.

20938. And one that filled you with sensations of horror, more or less?—It fills me with horror.

20939. Now, may I read what Dr. Pembrey did say?—Yes.

20940. "I will be perfectly straightforward. I say that you should not inflict pain if you can obtain the knowledge in any other way; but I say that even where there is an operation, the pain there is of a protective nature; it may produce syncope, and therefore less sensation of pain. Further, I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of hypnotism, which I offered to show to the Commissioners. That, I think, is one very important point. These animals pass into a condition, so far as one can see, comparable to hypnotism. If you give them anæsthetics you are introducing a complication which you could remove, and therefore without anæsthetics you actually save life and actually diminish the infliction of pain." Your *précis* has taken out the word "hypnotism" apparently, and put in the word "syncope"?—I thought that syncope was there as well as hypnotism.

20941. No, you say "than if the experiment were

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done on the animal in a condition of syncope"?—Yes. What does he say?

20942. He says:—"On the animal in a condition of hypnotism"; and then where you have put in those dots he explains the condition of hypnotism as I have read it to you?—Yes, I understand that, but that, of course, does not remove——

20943. It removes the whole of Dr. Pembrey's statement there?—I do not think it does.

20944. He is referring there simply to the condition of hypnotism. He is advocating hypnotism instead of an anæsthetic, and for that reason he says "therefore without anæsthetics," that is to say, if you give hypnotism without an anæsthetic "you actually save life, and actually diminish the infliction of pain"?—Surely you are incorrect in that. He says at the beginning:—"I say that even where there is an operation the pain there is of a protective nature; it may produce syncope." I presume that "syncope" is right there?

20945. Yes.—Yes, "and therefore less sensation of pain." The horror to me is in inflicting pain, without any anæsthetic, which shall be so terrible that it produces insensibility. That is what it comes to.

20946. Where in that do you find that Dr. Pembrey says that he was going to inflict pain without anæsthetics? He does not say so?—But he says that the pain in the operation there is of a protective nature. Obviously that must be where there is no anæsthetic given. "It may produce syncope, and therefore less sensation of pain." I presume that when a person is absolutely insensible, in a state of syncope, he does not feel any pain at all, but the horror to me is that pain should be of such a terrible character that it should produce syncope.

20947. Is Dr. Pembrey there giving anything more than an illustration of the protective mechanism?—I should not read it so.

20948. Is he not there giving simply an illustration of the protective mechanism, and is he not, in a subsequent part of the answer, advocating hypnotism instead of an anæsthetic; and have you not put down before us that he is advocating that the animal should be put in a condition of syncope instead of an anæsthetic?—No, I do not agree with that interpretation. As I understand his answer, which begins "Further," immediately after the words "sensation of pain," there is nothing about hypnotism in the early part of the answer. All he says is:—"Further, I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of hypnotism." But what does he mean by hypnotism? Does he mean the syncope which is produced by a terrible pain?

20949. May I ask whether you have read his evidence, because he explains what he means by hypnotism before. You have not read it?—I have read some of it.

20950. You cannot explain then why the word syncope is used here instead of hypnotism; how that unfortunate mistake got into your *précis*?—No, I do not know how it got there; but what I do say is that if pain produces syncope it is a perfectly horrible thing. If you inflict sufficient pain to produce syncope it must be horrible torture.

20951. Do you mean to say that Dr. Pembrey has ever advocated that? He advocates hypnotism. He is simply illustrating the protective mechanism in the shape of syncope. Is not that true? When you get severe pain you faint?—It may not be so, but I should be inclined to read it as if he advocated it.

20952. It would read so naturally if you substitute syncope for hypnotism, as has been done in your *précis*; but if you put in the words which Dr. Pembrey used, it cannot be read so?—I am very glad to find that he is not quite so bad as I have painted him.

20953. There is one more question that I should like to ask you, and that is, you and your society are desirous of getting at the truth of this matter, are you not, with regard to experiments on animals?—I hope we are all of us desirous of getting at the truth of the matter.

20954. And you have been especially doubtful with respect to the anæsthesia and the condition of the animals when they have been kept alive after an experiment?—Yes.

20955. And you have suggested a want of candour on Dr. Starling's part in other cases with respect to this very question?—Yes.

20956. Dr. Starling gave his evidence here, I think, on the 12th December, 1906?—Yes.

20957. That is a year ago, is it not?—Yes.

20958. In that evidence Dr. Starling requested that those responsible persons belonging to anti-vivisection societies, who were desirous to see what happened in the laboratories themselves should come to him and see what happened. Was that not so?—No, I do not read it so.

20959. He suggested it, I mean?—He said that he would admit any Member of Parliament or layman, but not any doctor.

20960. That is all I meant?—I should be no use if I went there. I am a Member of Parliament, but I should be of no use there.

20961. Has your Association, or have you, during the whole of that year made any attempt whatsoever to accept Dr. Starling's invitation?—No, because we should be of no use. Do you suppose that I should be of any use witnessing an experiment?

20962. But you have just told us that a man of common-sense and intelligence, as you consider yourself, would be able to judge on the subject as to whether a dog was properly anæsthetised or not?—Yes, but in order to do that, what I should like to do would be to be able to see through a hole in the door without it being known that I was there. I do not say that there would be anything underhand or wrong about it, but I do say that if an observer was present special care would be taken.

20963. But even supposing that special care was taken, would it not be better, if you really were desirous of finding out what was going on, to accept an invitation of that kind? What I want to understand is, why no member of your Association has attempted to accept that invitation of Dr. Starling's given in that way?—Because we do not think it would be of any practical use if we did. If we could get admission for a doctor whose views were in accordance with our own to see these experiments, we should be very glad. For instance, there is a Dr. Hadwen, who is very prominent in anti-vivisection societies. If we could get admission for him we should be very glad. If we could get admission for Miss Lind-af-Hageby I daresay it would be very useful to us; but for a mere Member of Parliament or layman as he suggests to go and witness experiments would be of uncommonly little use.

20964. Then the reason why you have not done it is simply that you, as a Member of Parliament or layman, consider that you would get no advantage whatever by so doing?—We think it would be a waste of time.

20965. (*Dr. Wilson.*) There is only one question that I want to put to you. Apart altogether from advantages which may be gained either in the prevention or the cure of disease or for the relief of human suffering, do I correctly understand that your Association takes up the position that on ethical grounds alone you press for the total abolition of vivisection?—I think that is so. At all events I should take that ground myself. If you put it on ethical grounds alone I should press for total abolition.

APPENDIX A.

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I.—DRAFT FORM OF APPLICATION FOR LICENCE SUGGESTED BY SIR V. HORSLEY, F.R.S., F.R.C.S

39 and 40 Vict., c. 77.

APPLICATION FOR LICENCE.

Address_____

Date_____

To the Right Honourable the Secretary of State for the Home Department.

Sir,

I, _____

_____

_____

beg to apply under the above-mentioned Act for a Licence for the performance of experiments on animals, and hereby append particulars of my proposed investigation.

1. General purpose and object of the investigation _____

_____

_____

2. Nature of experiments to be performed on anæsthetised animals which are killed before recovery from the anæsthetic _____

_____

_____

3. Nature of experiments to be performed with aseptic or antiseptic precautions on anæsthetised animals which are subsequently allowed to recover from the anæsthetic_____

_____

_____

_____

4. Nature of inoculation experiments to be performed on anæsthetised or non-anæsthetised animals

5. Class of animals to be employed in the investigation

6. Place in which the experiments are to be performed

This application is supported by the recommendations appearing below.

I am,

Sir,

Your obedient Servant,

* Here applicant to sign his name. *

We recommend that the above application be granted.

* Here the person recommending is to sign his name. 1. *

† Here specify statutory qualification (see Sec. II.). †

2. *

II.—DRAFT FORM OF RETURN OF EXPERIMENTS PERFORMED DURING A YEAR BY A LICENSEE

SUGGESTED BY SIR V. HORSLEY, F.R.S., F.R.C.S.

Form No. RETURN OF EXPERIMENTS PERFORMED DURING THE YEAR 190 .

No. of Licence .

Total Number of Experiments performed.	Number of Experiments in which the animal was killed before recovery from the anæsthetic.	Number of Experiments in which the animal was allowed to recover from the anæsthetic.	Number of Inoculation Experiments.	Number of Experiments in which pain was definitely observed.	General Remarks.

Reference to journal or periodical in which the results of experimental research have been published during 190 .

Date 190 Signature of Licensee.

ROYAL COMMISSION ON VIVISECTION.

FIFTH REPORT

OF

THE COMMISSIONERS.

Presented to both Houses of Parliament by Command of His Majesty.

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ROYAL COMMISSION.

WHITEHALL, 17TH SEPTEMBER 1906.

The KING has been pleased to issue a Commission under His Majesty's Royal Sign Manual to the following effect :—

EDWARD, *R. & I.*

EDWARD THE SEVENTH, by the Grace of God, of the United Kingdom of Great Britain and Ireland and of the British Dominions beyond the Seas King, Defender of the Faith, to

Our right trusty and well-beloved Cousin and Councillor WILLIAM COURT, VISCOUNT SELBY ;

Our right trusty and well-beloved Councillor AMELIUS MARK LOCKWOOD, Commander of Our Royal Victorian Order, Honorary Colonel of the 4th Battalion of the Essex Regiment ; and

Our trusty and well-beloved :—

SIR WILLIAM SELBY CHURCH, Baronet, Knight Commander of Our Most Honourable Order of the Bath, Doctor of Medicine ;

SIR WILLIAM JOB COLLINS, Knight, Doctor of Medicine ;

SIR JOHN MCFADYEAN, Knight ;

MACKENZIE DALZELL CHALMERS, Esquire, Companion of Our Most Honourable Order of the Bath, Companion of Our Most Exalted Order of the Star of India, one of the Under-Secretaries of State to Our Principal Secretary of State for the Home Department ;

ABEL JOHN RAM, Esquire, one of Our Counsel learned in the Law ;

WALTER HOLBROOK GASKELL, Esquire, Doctor of Medicine ;

JAMES TOMKINSON, Esquire ; and

GEORGE WILSON, Esquire, Doctor of Medicine ;

GREETING !

Whereas We have deemed it expedient that a Commission should forthwith issue to inquire into and report upon the practice of subjecting live animals to experiments, whether by vivisection or otherwise ; and also to inquire into the law relating to that practice, and its administration ; and to report whether any, and if so what, changes are desirable ;

Now know ye that We, reposing great trust and confidence in your knowledge and abililty, have authorized and appointed, and do by these Presents authorize and appoint you, the said William Court, Viscount Selby (Chairman) ; Amelius Mark Lockwood ; Sir William Selby Church ; Sir William Job Collins ; Sir John McFadyean ; Mackenzie Dalzell Chalmers ; Abel John Ram ; Walter Holbrook Gaskell ; James Tomkinson ; and George Wilson to be Our Commissioners for the purposes of the said inquiry

351. 2,500.—Wt. T.L. 306.—6/08. Wy & S.

And for the better effecting the purposes of this Our Commission, We do by these Presents give and grant unto you, or any five or more of you, full power to call before you such persons as you shall judge likely to afford you any information upon the subject of this Our Commission ; and also to call for, have access to and examine all such books, documents, registers and records as may afford you the fullest information on the subject, and to inquire of and concerning the premises by all other lawful ways and means whatsoever.

And We do by these Presents authorize and empower you, or any five or more of you, to visit and personally inspect such places as you may deem it expedient so to inspect for the more effectual carrying out of the purposes aforesaid.

And We do by these Presents will and ordain that this, Our Commission shall continue in full force and virtue, and that you, Our said Commissioners, or any five or more of you, may from time to time proceed in the execution thereof, and of every matter and thing therein contained, although the same be not continued from time to time by adjournment.

And We do further ordain that you, or any five or more of you, have liberty to report your proceedings under this Our Commission from time to time, if you shall judge it expedient so to do.

And Our further will and pleasure is that you do, with as little delay as possible, report to Us under your hands and seals, or under the hands and seals of any five or more of you, your opinion upon the matters herein submitted for your consideration.

And for the purpose of aiding you in your inquiries We hereby appoint Our trusty and well-beloved Charles Clive Bigham, Esquire, Companion of Our Most Distinguished Order of Saint Michael and Saint George, Captain in Our Army, to be Secretary to this Our Commission.

Given at Our Court at *St. James's*, the 17th day of September 1906, in the sixth year of Our Reign.

By His Majesty's Command.

H. J. GLADSTONE.

Note.—His Majesty was subsequently pleased to create Mr. Chalmers a Knight Commander of the Most Honourable Order of the Bath.

ROYAL COMMISSION ON VIVISECTION.

FIFTH REPORT.

TO THE KING'S MOST EXCELLENT MAJESTY.

MAY IT PLEASE YOUR MAJESTY,

Availing ourselves of Your Majesty's permission to report our proceedings from time to time, we beg to submit to Your Majesty, as an Appendix to this our Fifth Report, Minutes of the Evidence taken in the month of March, 1908, with Appendices and an Index of all the evidence we have heard, as we think the present publication of these documents is desirable.

ALL WHICH WE HUMBLY SUBMIT FOR YOUR MAJESTY'S MOST GRACIOUS CONSIDERATION.

SELBY (*Chairman*).

M. LOCKWOOD.

W. S. CHURCH.

M. D. CHALMERS.

A. J. RAM.

CLIVE BIGHAM (*Secretary*).

28 May, 1908.

ROYAL COMMISSION ON VIVISECTION.

APPENDIX

TO

FIFTH REPORT

OF

THE COMMISSIONERS.

MINUTES OF EVIDENCE,

March, 1908,

WITH

APPENDICES AND INDEX.

Presented to both Houses of Parliament by Command of His Majesty.

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ROYAL COMMISSION ON VIVISECTION.

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351. 1500—Wt. T.L. 306. 6/08.—Wy. & S.

ROYAL COMMISSION ON VIVISECTION.

LIST OF WITNESSES.

In the order in which they appeared before the Royal Commission.

Date.	Name of Witness.	Profession, Occupation or Residence.	Representing.	Number of first Question.	Page.
1908. 46th Day - Mar. 25th	*Lieut.-Col. Lawrie*, M.B., I.M.S., (Retd.)(recalled)	- - - - - - -	- - - -	20,966	1
	Mr. L. E. Shore, M.D. (recalled).	Lecturer on Physiology, University of Cambridge.	- - - - -	21,373	9
	Mr. WILLIAM HALL. -	Laboratory Assistant, Cambridge.	- - - - - -	21,581	14
	Mr. H. K. ANDERSON, M.D., F.R.S. - -	Lecturer on Physiology, Cambridge.	- - - - -	21,636	15
	Mr. W. H. GASKELL, M.D., F.R.S.	A member of the Commission	- - - - -	21,705	17

MINUTES OF EVIDENCE

TAKEN BEFORE THE

ROYAL COMMISSION ON VIVISECTION.

Wednesday, 25th March 1908.

FORTY-SIXTH DAY.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, M.D., LL.D.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Lieut.-Colonel E. LAWRIE, M.B., I.M.S., recalled; and further Examined.

Lt.-Col. E. Lawrie, M.B., I.M.S.
25 Mar. 1908.

20966. (*Chairman.*) You have sent us a printed statement?—Yes.

20967. There is only one page in it on which we desire to hear you, and that is page 10A, in which you speak of an operation on two dogs, and give some details, those details involving a charge of a distinct breach of the law, and, in the language you use, a wilful breach of the law on the part of Dr. Gaskell and of those who were assisting him; therefore we think it is material that we should examine you upon it. The rest of your statement is matter of comment, and on matter not very material to our inquiry; but this is distinctly and directly material to our inquiry, and therefore we shall confine ourselves to the one matter about which we wish to ask you, which, as I have already said, deals with what was done and said with reference to anæsthetics on the 7th of July, 1894?—Yes.

20968. You visited Dr. Gaskell's laboratory at Cambridge on that day?—Yes.

20969. That was for the purpose, I understand, of watching some experiments upon two dogs?—Yes.

20970. They were experiments in cross circulation?—Yes.

20971. Involving opening the throat of each dog and placing a tube in it and connecting it with the other dog?—Connecting the vessels in the two dogs' necks.

20972. You say that you arrived there before luncheon?—Yes.

20973. You saw two dogs on the table in the laboratory?—Yes.

20974. Were those the same two dogs that you watched the experiments on after luncheon?—Yes.

20975. How long were you in the laboratory before luncheon?—That I cannot recollect. I was not there very long.

20976. Was any work done then?—No, not that I remember.

20977. Did all of you examine the dogs, or did you merely see that they were there?—I was just taken in to see them.

20978. You did not stop to examine them or watch them?—No, there was nothing doing then. They were ready for the experiment.

20979. Then you did not, in fact, do anything before luncheon?—No.

20980. Were their throats open at that time?—Yes.

20981. Were the tubes in their throats?—Yes, so far as I recollect the tubes were in both dogs' throats.

20982. Are you quite sure?—That is the best of my recollection.

20983. I must ask you when you do not remember a thing distinctly to say so?—I have stated in the statement which I sent to you exactly what I recollect.

20984. I do not want you to read it from the paper, because there are other gentlemen who were present at this experiment, and there will be a good deal of dispute probably about it. I do not know what they are going to say, but they are here at any rate, and there may be considerable dispute, so I will ask you to speak from your memory, and when you feel that your memory does not establish a fact, to let us know?—Yes. I have stated exactly what I recollect in my statement.

20985. I want you to give it orally, please, *viva voce*, and not to read it from the paper, because I want to have your recollection?—I have stated all that I recollect there. I have nothing to add to it.

20986. You will follow what I tell you, please, is the proper method of giving evidence—that is, to answer questions. There are some matters that we have allowed to be read when they are matters of opinion and criticism, but when it is a simple matter of fact that is being spoken of, I would rather that you spoke from your memory if we are to have other witnesses who are to speak from their memories. Was anything said about anæsthetics?—Dr. Gaskell told me that the dogs had had no anæsthetic except morphin.

20987. What was it that he said?—He said they had had no anæsthetic; but they had had some morphin; and he led me to understand that they had had it so as to be able to report that the dogs had had an anæsthetic so as to hoodwink the Inspector.

20988. I want to know what he said?—That they had had no anæsthetic at all except a small dose of morphin.

20989. And at that time you say their throats had been opened?—Yes.

Lt.-Col. E. Lawrie, M.B., I.M.S.

25 Mar. 1908.

20990. Did he say that they had had none at all—no anæsthetic at all?—He said that they had had nothing but the morphin solution.

20991. Did he say how much morphin he had had?—He said ten drops of morphin solution.

20992. Would that in your opinion have produced anæsthesia?—No anæsthesia at all.

20993. Would three grains of morphin produce anæsthesia?—No, three grains does not anæsthetise a dog.

20994. Have you experimented on dogs yourself?—Yes, I have.

20995. With anæsthetics?—With anæsthetics and with morphin.

20996. And that is the result of your experiments?—That is the result of the experiments of the Hyderabad Commission on 600 animals.

20997. You mean as to how much anæsthetic should be given?—They were principally as regards the effect of chloroform; and the effects of certain other anæsthetics were inquired into incidentally.

20998. What I asked you was, have you experimented with morphin on dogs?—Yes, we have.

20999. You did?—Yes, repeatedly.

21000. Did Dr. Gaskell say anything as to why they had not had anæsthetics?—He said they were going to experiment with the chloroform afterwards, and that they did not give them any chloroform before the experiment began because they wanted to have the primary effects of the chloroform in the experiment.

21001. And that was the reason they used morphin?—He did not say that was the reason they had used morphin. He led me to suppose that they used morphin simply to comply with the Act, and to be able to say that they had used an anæsthetic.

21002. Can you remember Dr. Gaskell's words?—No, I cannot remember the words; but that is their exact meaning.

21003. You have sent me in a statement here that uses certain language. You do not say now that Dr. Gaskell said it was done to hoodwink the Inspector. Did he use any such words?—He used words that had that precise meaning.

21004. Did he use any such words?—I cannot remember the exact words, but that is the precise meaning of what he said to me.

21005. You must see that it is material to try and remember it. You have printed this document and circulated it among us with the word "hoodwink"?—That is the precise meaning of what he said to me.

21006. I think you must really know enough about evidence to understand why we want to know the exact words that Dr. Gaskell used?—I cannot remember the precise words that he used.

21007. Then you will not undertake to say that he did use the words "hoodwink the Inspector"?—No, I will not undertake to say he did use those words; but what he said had that precise meaning.

21008. It is rather odd that you should have circulated that if you are not sure that he said it, "and satisfy the Act"; he did say that apparently?—Those were the precise words he used so far as I recollect; but the meaning he gave me was that he meant to evade the Act. That was the impression he gave me.

21009. To satisfy the Act?—Yes.

21010. That was the first account you gave me just now when I asked you the question, that what he said was that it was to satisfy the Act?—Exactly.

21011. He would not have satisfied the Act without producing anæsthesia, would he?—What I understood was that if he was able to say that he had used an anæsthetic (and he called morphin, I suppose, an anæsthetic for that purpose) that would satisfy the Act.

21012. That is an inference you can draw from what he said?—That is what I understood from what he said.

21013. Did Dr. Shore say anything?—I do not remember his saying anything.

21014. Who was present at the time that these words were said?—There was no one present but myself

21015. I suppose Dr. Gaskell was present?—Of course; he said it to me.

21016. Who else was present?—I do not think anybody else was present besides Dr. Gaskell.

21017. Are you sure that nobody else was present?—I do not think anybody was.

21018. Did you then go in to luncheon?—Yes, we went in to luncheon then.

21019. Were these words said in the laboratory?—They were said in the laboratory.

21020. Then you went in to luncheon?—Yes.

21021. How long were you there?—I cannot tell you. We were there 20 minutes or half an hour.

21022. And you came back into the laboratory?—Yes, and then the experiment was done.

21023. Was chloroform used then?—Chloroform was used for the experiment.

21024. Was chloroform used in such a quantity as to produce anæsthesia?—Well, it killed the animal.

21025. (*Mr. Tomkinson.*) Was it given to both dogs?—No, it was given to one.

21026. (*Chairman.*) Was anæsthesia produced?—Apart from that there was no anæsthesia. It was given until the animal died, for the purpose of testing its effect upon the heart.

21027. I should have thought it was a simple question to answer. Were the animals anæsthetised when you came back from luncheon?—No, they were not.

21028. Were they operated on, or only watched?—Nothing was done except the administration of chloroform for the purpose of testing its action in the experiment, that is all.

21029. Did the dogs make any sign or sound?—They only appeared to be in pain.

21030. How did they show that?—By shivering.

21031. But they made no sound?—They had tubes in their throats.

21032. That would prevent it?—That would prevent it. Still, dogs do not always make sounds when they are in very severe pain if they are terribly frightened.

21033. I do not say that they did. I only wanted to know whether the dogs did. Was anything said as to the time the dogs had been under operation?—I was told that they had been under operation for four or five hours.

21034. By whom?—By Dr. Gaskell.

21035. It was before luncheon that you were told that?—Yes.

21036. (*Sir Mackenzie Chalmers.*) What time of the day would it be when you were told that?—It was between one and two o'clock in the day.

21037. (*Chairman.*) I think that is all I want to ask you.

21038. (*Sir William Collins.*) You have seen the evidence given by Dr. Shore as to the strength of the solution of morphia that was used?—Yes.

21039. Are you prepared to accept that statement?—Well, no, I am not; I prefer to believe what Dr. Gaskell told me at the time.

21040. But at the time had you reason to believe that the solution of morphia was of the pharmacopœial strength or not?—I understand it was the pharmacopœial strength; because when a medical man tells you that solution of morphia is used you understand that.

21041. I see Dr. Shore stated in answer to Question 19458: "Of the two dogs, one dog received ten cubic centimetres of a 2 per cent. solution of hydrochloride of morphia, and the other dog six cubic centimetres of the same solution." You think that is not correct?—If that is correct, then of course what Dr. Gaskell told me misled me; it may be from the way I took it.

21042. If what Dr. Shore says is correct, would the dogs then have been in a condition free from pain?—No, certainly not.

21043. Do you say that a dose of 3 grains of morphia will not render a dog anæsthetised?—No, I do not think it would have any anæsthetising effect.

21044. Did you see the bottle out of which the morphia that was used was taken?—No, I did not.

21045. (*Sir John McFadyean.*) Did you make any notes of the facts that you have just been telling the Commission at the time?—No, I did not. I did of the second experiment.

21046. Did it strike you at the time that whoever was responsible for the experiment was guilty not only of breaking the Act, but of absolute brutality?—I

thought at the time it was excessively cruel—the cruellest thing I had ever seen.

21047. Did you make any observation to that effect?—I said that we had given all our dogs chloroform for the cross-circulation experiments.

21048. Did you say anything by way of protest against the brutality of the operation?—No.

21049. Between that date and the time you appeared as a witness here have you ever given expression to the feelings of revolt that you must have felt?—I have repeatedly said it was cruel.

21050. To whom?—To various people.

21051. You did not formulate a charge like that when you gave evidence here before?—No, I tried to avoid saying anything about it.

21052. Then why are you so anxious to convince the Commission of it now?—Because of my evidence being contradicted by Dr. Shore. I am given the lie direct about several things that I said.

21053. Therefore you want ——?—I want to vindicate myself. Besides, I want to see the truth established about chloroform.

21054. This has hardly anything to do with the truth about chloroform, has it?—Yes, it has a great deal, because it is on these experiments that the present anomalous position of chloroform largely rests.

21055. With regard to this question of hoodwinking the Inspector; what Inspector was it?—I do not know; I cannot tell you anything about that.

21056. But you have admitted that you cannot vouch for the absolute words that Dr. Gaskell used, and one may therefore test the reliability of your memory by finding out whether there was any probability of there being an Inspector to hoodwink at the time?—You see, I knew nothing about the Vivisection Act. I had been in India many years then, and everything about this was new to me.

21057. You know something about the Act now?—I do not know very much about it now; I have never seen the Act.

21058. Are you aware that no Inspector under the Act has ever resided in Cambridge?—I am not aware of that fact.

21059. You will take it as a fact?—Yes.

21060. Does it not seem extraordinary that Dr. Gaskell should have said that he did this to hoodwink the Inspector when there was not one chance in a hundred, let us say, that the Inspector would turn up at that moment? It seems peculiar, does it not, that he should have used that expression?—I do not say that the word was "Inspector."

21061. No, but that he should have held the opinion that it was necessary to hoodwink the Inspector. If he said that he desired to hoodwink the Inspector he must have thought it necessary to hoodwink the Inspector?—I can only tell you what he told me.

21062. You are equally clear that he said that he desired to satisfy the Act?—Yes.

21063. To satisfy the Act he was bound to have the dogs free from pain?—No, I did not understand that at all.

21064. You did not understand it. But why do you refuse to accept that interpretation of his words. Assuming that he did use those words, why are you anxious to have the Commission believe that it was merely satisfying the letter and not the spirit of the Act?—Because he had used morphin.

21065. (*Chairman.*) You knew enough of the Act, did you not, to know that it was necessary to anæsthetise a dog before operating upon it?—No. I understood that if you said you had used morphin as an anæsthetic that would satisfy the Act.

21066. You had been in the medical service for some years before 1894, had you not?—Yes.

21067. How many years?—From 1872.

21068. And from 1872 onwards were you in England at all?—I was at home on leave in 1886.

21069. Your speciality was examining into the operation of anæsthetics, was it not?—No; I had to use anæsthetics a great deal in surgery, and in a great many experiments for the Hyderabad Commission.

21070. (*Sir Mackenzie Chalmers.*) On animals?—On animals.

Lt.-Col. E. Lawrie, M.B., I.M.S.

25 Mar. 1908.

21071. (*Chairman.*) Do you mean that you really did not know in 1894 that the Act of 1876 required that an operation of this sort on an animal should be under anæsthetics?—Well, morphia was mentioned to me as an anæsthetic.

21072. No. I am speaking about what satisfies the Act. Did you know in 1894 that in order to satisfy the Act you must anæsthetise a dog under an operation of that sort?—No, I did not.

21073. You really did not know that in 1894, after having had twenty-two years of medical service?—The Act did not apply to India at all.

21074. (*Sir John McFadyean.*) It seems to me that your view as to a legal obligation is rather remarkable, because I understand it to be with regard to any requirement of the law, that when an individual says he has satisfied a requirement of the law, that requirement is thereby fulfilled. Is that your position?—I know nothing about the law or your interpretation of it. I am simply telling you what Dr. Gaskell told me, and led me to understand.

21075. But the point is that the language that he used may be capable of two interpretations?—I am only giving you mine; I cannot give you any other.

21076. But I want to know why you refuse to accept the other interpretation?—Because I have always held to the first. I formed that opinion at the time, and still have it, and I cannot alter it.

21077. You think it absolutely incredible that Dr. Gaskell, when he said that he desired to satisfy the Act, may have intended to say that he desired to have the dogs free from pain?—I am sure that he had not that meaning.

21078. Why?—From the way he said it to me.

21079. That is a new item of evidence; it is not on what he said, but the manner in which he said it?—Exactly.

21080. Was it said scoffingly?—No, it was said intentionally to lead me to suppose that he should report the administration of morphin, and meant whoever was responsible, the Inspector or whoever it was, to understand that it was an anæsthetic.

21081. (*Chairman.*) That he should report it?—That is the opinion I formed.

21082. (*Sir John McFadyean.*) You say that it was done with the intention to lead you to suppose that?—Yes.

21083. But then that is a mere interpretation placed upon the manner in which the thing was said, is it not?—It is the way he said it, and the words he used which had the meaning I have given here.

21084. You would not admit the slightest possibility of your having misunderstood him?—Not about that, most surely.

21085. Then with regard to the state of the dogs, you wish us to believe that they were subjected to dissection for four hours before you saw them?—That is what I was told.

21086. You believe it?—I believe it, certainly.

21087. And you say that the dogs were "in frightful agony"; those are your own words?—Yes, they appeared to be in frightful agony.

21088. You were asked a minute ago as to what was the evidence of that, and you said shivering?—Shivering and appearing to be in pain.

21089. Let us hear what the other symptoms were?—As I have given you—they were shivering, and appeared to be in pain.

21090. But when you say that they appeared to be in pain, that is not sufficient evidence for the members of the Commission?—They were tightly tied down; they could not do anything.

21091. In case you may have misinterpreted the signs you saw exhibited by the dogs, I want you to be good enough to tell the Commission all the evidence on which you found this statement that the dogs were in frightful agony?—The dogs were both tied down, and could not show any other signs of pain except what I have told you.

21092. I may say that I am a person practically acquainted with the tying down of dogs. Can you tell me how you tie a dog down so that it cannot show evidence of pain?—They had their fore feet and hind feet tied down and their heads tied down.

Lt.-Col. E. Laurie, M.B., I.M.S.

25 Mar. 1908.

21093. Have you ever seen dogs struggle when so tied down?—Yes, I have seen them attempt to struggle very often indeed.

21094. You have seen them attempt to struggle?—Yes, I have seen them.

21095. I want to put it to you in this way and see whether you agree with me or not: that however firmly you tie a dog down by its fore and hind paws, and by its head, if you proceed to cause it pain it will succeed in executing quite extensive movements with its body. I ask you to agree to that as being what I have myself observed?—Yes, they sometimes do.

21096. No—as a rule. I want you to assent to that statement, as a rule?—I cannot say more than that they sometimes do, and, of course, generally they would.

21097. What I should regard as signs of frightful agony in a dog would be violent contortions of the body and great disturbance of the respiration in an attempt to howl. Was anything of that sort exhibited by these dogs?—When dogs are in the last extremity of pain, they are in such a fright of getting something more that they lie generally as quiet as they can; and the shivering I saw that day was a sign to me of terrific pain.

21098. But still they were lying quiet?—I could not say actually they were lying quietly. They were shaking with fright.

21099. It is hardly right in answer to my question, which is directed to finding out whether they were suffering agony or not, to say that they were shivering with agony. All it seems to me that you are entitled to say is that they were shivering?—Not at all. I am entitled to say what I saw—that they were shivering with agony; which they certainly were.

21100. (*Chairman.*) Shivering was the only outward symptom that you saw?—Shivering, and the general state of the dogs.

21101. Was there any other outward symptom—leave out the question of what you infer. What was the outward symptom? You told me shivering, and you did not mention any other when I asked you?—Shivering, and the way in which they breathed through the tubes.

21102. (*Sir John McFadyean.*) Did the behaviour become markedly different when the anæsthetic was administered?—The anæsthetic was administered to one dog and that dog died.

21103. Did the behaviour of that dog become markedly different?—I was watching the fall of blood pressure during the time the experiment was being done.

21104. You cannot say anything about it?—I cannot give any further evidence about what they suffered after that, because my time was taken up in looking after the fall of the blood pressure on the drum.

21105. How was the chloroform administered?—I do not remember how it was administered—whether it was administered through a bottle or with a motor—I do not know; it was administered through the tracheal tubes. I do not know how it was given on the other side.

21106. How were the tubes secured in the throat?—I cannot tell you.

21107. Do not you think that if these dogs had been left quite unanæsthetised they would have been very likely to have torn and disturbed the apparatus?—I do not see how they could have done that.

21108. You do not assent to the view that a dog, although it is tied down by the extremities and by its head, can execute very extensive wriggling movements?—I did not see them do anything of that sort.

21109. To that extent then that, as a symptom of agony, was not present?—No.

21110. (*Sir Mackenzie Chalmers.*) I want to ask you one or two questions. You have experimented a good deal with anæsthetics on animals?—Yes.

21111. Can you tell us at all how many animal experiments you have made or been present at?—I was present at the 600 experiments done by the Hyderabad Commission.

21112. They were done under your supervision?—Well, I was present at them.

21113. You have experimented yourself?—Yes.

21114. On animals?—Yes.

21115. How many; can you give us roughly the number?—I cannot tell you. I have done a great many, and, of course, I took part in a good many experiments at the Commission. I was President of the Commission.

21116. Have you yourself experimented with morphia on animals?—There were many experiments made by the Hyderabad Commission with morphia.

21117. What in your opinion, as an expert, is a sufficient dose of morphia to produce anæsthesia in a dog of this size?—I cannot tell you the amount that would be necessary, but it must be a lethal dose. I do not know what the lethal dose for a dog is.

21118. If Dr. Shore was correct in saying that, owing to some misapprehension as to the strength of the mixture, he gave 36 times as much morphia as you thought, would that be a lethal dose or not?—No, it certainly was not a lethal dose.

21119. In your opinion nothing short of a lethal dose can produce anæsthesia in a dog?—I should think not, that is my opinion.

21120. Have you made any experiments for the purpose of testing it at all?—No, I have not.

21121. If a sufficient dose of morphia is given to a dog to produce anæsthesia, how long would the dog remain alive under it?—I do not know.

21122. Does the effect of morphia in your experience last for any considerable time?—Are you talking about animals now?

21123. Yes?—I cannot give you an opinion about that, because I have not had sufficient experience of the effects of morphia on animals. I can tell you about the human subject.

21124. A human subject must not have what you call a lethal dose?—No, a dose to relieve ordinary pain.

21125. May I take it that in your opinion morphia, even in a non-lethal dose, deadens pain, and in a lethal dose abolishes pain. Is that what you say?—It would deaden certain kinds of pain.

21126. Only certain kinds?—Yes, it would not deaden the pain of a cutting operation—an ordinary medicinal dose.

21127. With a larger dose?—I should not like to give any patient more than a medicinal dose.

21128. Now, I want to understand a little more about this particular experiment. You went into the laboratory before luncheon?—Yes.

21129. And there you saw the two dogs which had already been operated on?—Yes. They were prepared for the experiment.

21130. Will you tell us exactly what had been done to those dogs?—I cannot tell you precisely, any more than that their vessels had been cross connected, and that they had got tracheotomy tubes in their throats. I cannot tell you what had been done precisely, because I was not even told that. They had been prepared for the cross circulation experiments, and complete dissection of the neck had been done in both dogs.

21131. The operative procedure consisted in putting in a tracheotomy tube, and what else?—Cross connecting the vessels.

21132. At the time when you saw the animals there was common circulation then, was there?—That I cannot tell you—whether the clamps had been opened or not.

21133. Who went into the laboratory with you, do you remember?—Dr. Gaskell.

21134. Was Dr. Shore there?—I do not remember. I remember seeing him about. I do not remember that he was there at that moment.

21135. Was Sir Clifford Allbutt there?—No, he was not.

21136. Was anyone else there?—At the first experiment I do not recollect anybody else being there at all except assistants.

21137. I do not quite understand. Were there two experiments on that day?—There were two experiments, one on the 7th of July and the other on the 11th. At the first experiment there was nobody present except the laboratory people, so far as I recollect. On the second occasion Sir Clifford Allbutt and Dr. Langley

and somebody else were there; there were three or four people present on the second occasion.

21138. (*Chairman.*) This is the 7th of July that you are speaking of?—On the 7th of July I do not think that anyone was there except the laboratory assistants.

21139. That is the occasion you are speaking about?—Yes.

21140. (*Sir Mackenzie Chalmers.*) You saw one experiment on the 7th of July and another on the 11th?—Yes.

21141. Were both those experiments of the same kind?—Yes.

21142. On both occasions you say that the animals had been prepared before the experiment?—Yes, before I got there both animals were prepared, on both occasions.

21143. Then you spoke about going to luncheon after that?—That is on the first occasion.

21144. Who was at luncheon on that occasion then?—I do not recollect anybody except Dr. Gaskell and Dr. Shore. There may have been somebody else, but I do not remember.

21145. It was on the second occasion that Sir Clifford Allbutt and Dr. Langley were present.

21146. At the laboratory? When you went into the laboratory and you saw the dogs prepared for the experiment, was the laboratory attendant there?—I cannot recollect; I fancy he was, but I cannot recollect anybody specifically.

21147. Was Dr. Shore there?—Dr. Shore was about the laboratory.

21148. You said nothing at the time, may I take it, of the inhumanity which you witnessed, or thought you witnessed going on at the time?—I thought it was frightfully cruel.

21149. And yet you said nothing?—What was I to say? I could not possibly say anything in another man's laboratory.

21150. But surely! is that so?—It is so certainly. I could not go into Dr. Gaskell's laboratory and criticise his way of doing his experiments. I should never dream of doing such a thing, and I cannot imagine that any other man could either.

21151. I should not have thought so myself. When did you first mention to anybody that you had seen what you call a cruel experiment?—I have always mentioned it, and I talked about it on that day. I told Dr. Gaskell that we had given all our animals choloroform, and I could not understand why he would not give chloroform to his. We never dreamt of doing any operation like this without chloroform.

21152. Would giving chloroform in the initial stage (that is for the purpose of putting in the tracheotomy tube and connecting the arteries) have in any way vitiated the experiment?—In my opinion not in the least.

21153. Chloroform could have been given as well as morphia?—Certainly.

21154. There was absolutely no object in giving morphia instead of chloroform?—Not that I know of.

21155. Was there any scientific object to be attained by not giving a sufficient anæsthetic?—I am quite prepared to believe that Dr. Gaskell thought that if he gave the chloroform it might vitiate the experiment. But that is not my opinion, of course.

21156. But he did not say so?—He said as much as that, that they did not want to give chloroform before it was given for the experiment.

21157. Was there any object in preparing those dogs as you say, three or four hours before, and leaving them?—It takes a long time to cross-connect the circulation; it takes a very long time to dissect the vessels out and connect them one with another.

21158. Have you seen it done yourself?—I have seen it done very often.

21159. It was done in your own laboratory?—In my own school.

21160. As a demonstration?—No; we were obliged to repeat these experiments when they gave us results diametrically opposed to the results we had obtained; we were, therefore, obliged to repeat the cross-circulation experiments that we had made them do.

21161. Did you do those under anæsthetics?—We did them under chloroform always so far as it was possible.

21162. When is it not possible?—Just before the experiment is begun, you must let the animals out of the chloroform so as to see what the effect of the chloroform is from the beginning, from the time that the blood pressure first begins to fall under the influence of the anæsthetic.

21163. It would vitiate the experiment, you mean, otherwise?—You cannot mix up the experiment with what you were doing before; you must begin the experiment, as it were, afresh.

21164. Then, assuming morphia to be a sufficient anæsthetic, that would be a good reason for giving it?—If morphia was an anæsthetic.

21165. In fact, you do not believe in morphia as an anæsthetic?—I know that it is not.

21166. (*Chairman.*) Except in a lethal dose, you mean?—Precisely.

21167. (*Sir Mackenzie Chalmers.*) By a lethal dose you do not mean a dose that kills at once, but kills in some hours, ultimately?—Yes.

21168. In your experiments, when you had to intromit the chloroform, did you give any anæsthetic?—No.

21169. The animals for that short period were without anæsthetics?—They were out of the anæsthetic.

21170. Did they show any signs of pain in an operation of this kind?—They did for a time, very often, not always.

21171. After tracheotomy in the human subject is there usually much pain?—Pain where?

21172. Anywhere?—Do you mean to say after the operation?

21173. Yes?—No, I should not think there is any pain at all, except the mere smarting of the wound—nothing else.

21174. You mean that the pain consists in the making of the cut, and the smart, and that soon passes away?—Yes.

21175. Then in the case of these dogs, which you told us were suffering pain, what were they suffering from exactly?—The whole of their necks were dissected, and opened up; all their vessels were exposed.

21176. That is to say, there was a large wound?—An enormous wound, the whole of the necks were dissected in both dogs, so as to cross-connect the vessels.

21177. Would there be many sensitive nerves there?—Yes.

21178. There would be considerably more pain than that following upon ordinary tracheotomy?—After tracheotomy there is no pain at all to speak of—nothing that would require any medicine.

21179. But after cross-connection dissection there would be?—There would be a great deal of pain, of course, but not as much pain as during the cutting.

21180. You told us that dogs in very great pain often do not growl, or make any sign?—Yes.

21181. What sort of experiments would you judge that from?—I am speaking generally. I cannot recollect any specific ones without the reports of the cases.

21182. Some of those were experiments you have made yourself?—Yes.

21183. In what kind of experiment have you had to cause a dog such pain?—In experiments on the action of chloroform, when we are obliged to let them out before we begin giving them chloroform in any particular observation.

21184. You have had yourself to perform very painful operations on animals?—No cutting operations.

21185. I want to know what operations you were referring to when you told us that dogs under great pain show very little sign except shivering?—Yes. I do not say that they always do.

21186. I want to know what operations you are referring to?—In the experiments of the Hyderabad Commission an enormous number of experiments were done to try and produce shock, by Sir Lauder Brunton, and those are the operations that I refer to. Sometimes they tried to produce shock in every stage of anæsthesia—in every stage of chloroform.

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21187. Those are experiments under anæsthesia?—Yes, but by that time they were allowed to come round again before the cross-connection.

21188. (*Mr. Ram.*) By Sir Lauder Brunton?—Yes, and the Hyderabad Commission; they were done by the whole Commission.

21189. (*Sir Mackenzie Chalmers.*) Who else?—The man who is now Surgeon-General of the Government of India, Dr. Bomford, and Dr. Rustomji.

21190. And yourself?—And myself.

21191. When did you first publish any account showing that these animals were, as you think, put to unnecessary suffering?—I have never published any account of it.

21192. When did you first put it in writing?—Here, for this Commission.

21193. Before you had given evidence here?—Yes.

21194. That is after fourteen years?—Yes.

21195. You did not think it your duty to inform any public authority about it?—No, certainly not.

21196. And you thought that, as it was in somebody else's laboratory, you were not even entitled to remonstrate with the people in charge of the laboratory?—No, I never should have dreamed of doing such a thing.

21197. Had you formed any opinion at the time whether the law was being broken or complied with?—I did not know.

21198. (*Mr. Tomkinson.*) In what capacity were you there, do you consider?—I was there as a mere spectator.

21199. As a guest?—As a guest.

21200. As an invited guest?—Yes.

21201. Not as a professional coadjutor?—Well, it hardly amounted to that. They wanted to show me what the results of their experiments were.

21202. You were there by invitation to see the effect of certain experiments?—Yes, they said they would produce certain effects by experiments which were the exact counterpart of those which formed the subject of report to the Nizam's Government.

21203. I think you say that you consider that your position there rather debarred you from making any protest against anything you saw that you might disapprove of?—Yes.

21204. You felt debarred from entering a protest against what you thought an act of cruelty?—Yes; it never would have entered into my head to do so at all.

21205. You say that Dr. Gaskell told you that the preparation for that experiment that you were to see had taken four hours?—Yes, about that.

21206. And you found these dogs so prepared, strapped down very firmly?—Yes.

21207. Without the possibility of more than shivering.

(*Chairman.*) He did not say that.

(*Witness.*) No, I did say that.

21208. (*Mr. Tomkinson.*) But you said I think that they showed no movement except that of shivering?—They could not do anything very much in the way of movement.

21209. Did you examine their eyes?—No.

21210. But you are tolerably satisfied that they were not unconscious?—I am sure they were not unconscious. They were not getting any anæsthetic.

21211. But, of course, there is a conflict about the quantity of the anæsthetic, at least the morphia?—I am only giving my own view, of course.

21212. Had that morphia been administered through the mouth?—I cannot tell. I understood it was administered subcutaneously.

21213. We have had it stated to this Commission that it is only when morphia is administered subcutaneously, that it is claimed to be an anæsthetic?—I see.

21214. The tubes in their throats would make the dogs voiceless, would they?—I should think so.

21215. You are not quite sure about it?—That is my opinion, that it would practically make them voiceless.

21216. If the dogs had been in that condition for some hours would not the struggling or demonstration of pain naturally die down rather; you would not expect them to show such active signs of pain as they would at the beginning of the operation, naturally?—No, certainly not.

21217. Then you do not think that being there and disapproving, as you did, of what you saw, you ought to have made a protest?—I could not have made a protest to a man in that position.

21218. You do not think that you in any way made yourself a party to the approval of the operation?—Not in the least, in no way certainly.

21219. You considered yourself bound by professional etiquette or by your position as an invited guest from making any protest?—These were two great physiologists, and it was not my business to speak to them about their own work and the way in which they did it.

21220. (*Mr. Ram.*) Will you tell me, please, is the operation of putting a tube into a dog's throat, as you describe it, a delicate one?—Do you mean difficult?

21221. Yes?—If you have had a little practice at it it is not difficult at all.

21222. Do you not require the dog to be immovable, or nearly so?—Well, you would have to have it fairly quiet.

21223. Do I rightly gather that in your opinion these dogs had been prepared for hours before, as you expressed it, for fully four hours before, and that the dogs were not under anæsthetics when their throats were being dissected and these tubes inserted?—Do you mean to say that during the whole time they were dissected from the commencement of the dissection until the time I saw them they had had no anæsthetics?

21224. That is what you say?—That is what I say.

21225. In your opinion would it be a possible thing to get the tubes into those dogs' throats in the way you saw them if dogs had been under no anæsthetics at all?—Certainly it would.

21226. Would it have been a difficult operation?—No, not particularly.

21227. Would it have been more difficult than if they had had chloroform?—Yes, I should think it might be a little more difficult.

21228. More difficult than if they had had a full dose of morphia?—I do not think morphia would make any difference one way or the other.

21229. (*Chairman.*) Does that apply not only to the operation of inserting the tube, but to the preparing of the arteries?—Does what apply?

21230. What you said about its making no difficulty or very little difficulty in performing the operation?—In many ways it adds to the difficulty to have to give chloroform to two dogs which are being prepared for a cross-connection experiment; it is extremely difficult to give a dose of chloroform so as to keep them anæsthetised without having accidental death.

21231. I did not say chloroform; I said an anæsthetic, or anything that would quieten them?—I am talking about chloroform, of course.

21232. (*Mr. Ram.*) Only chloroform?—Yes, or any anæsthetics of that kind that is given by inhalation.

21233. Does your opinion of the state of these dogs as to whether they suffered or not depend upon whether in fact morphia had been given to them to a sufficient extent to make them insensible?—No, it did not depend upon that at all.

21234. Do you then, in forming your opinion as to the state of those dogs, disregard whether they had been given morphia or had not?—I disregard it entirely.

21235. When you saw this operation performed, you saw the animals before you went to luncheon?—Yes

21236. You were of opinion that they were in great pain?—Yes, I was of opinion that they were in great pain.

21237. Did the statement that you have just made as to the suffering of the dogs and as to your belief that they were prepared for the operation without any anæsthetics, apply to the operations both on the 7th July and on the 11th July?—That was only spoken about on the first occasion, on the 7th July.

21238. But as to the state of suffering of the dogs, does it, in your opinion, apply to the feelings of the dogs on both occasions?—Yes, I should think it would.

21239. Now just revert to the occasion on which you went to luncheon; who were present at luncheon besides yourself?—I only remember Dr. Gaskell and Dr. Shore.

21240. Are you aware that both of those gentlemen have told us that in their opinion the dogs were under no suffering whatever?—Well, that may be their opinion.

21241. On the occasion of the luncheon, there were you three gentlemen there, so far as you recollect?—Yes.

21242. Those two gentlemen, as they told you, believed that the dogs were wholly free from suffering?—They have told you that.

21243. You, on the other hand, tell us you were sure the dogs were in a state of suffering?—I say so.

21244. How long did lunch last?—I cannot tell you precisely; it lasted probably 20 minutes or half an hour, but I do not recollect how long it lasted.

21245. In your last answer to Sir Mackenzie Chalmers, you stated that you did not know whether a breach of the law was being committed or not?—No.

21246. You did not know then?—No.

21247. Do you know now?—I have been told so.

21248. When did you first learn that a breach of the law had been committed?—I was told quite recently that a breach of the law had been committed.

21249. Have you read the Act?—No.

21250. All the evidence you have given us here without ever having read the Act under which we are proceeding; is that so?—Yes.

21251. When did you first learn that there had been a breach of the Act—when you were here the other day?—No, I cannot remember when it was. It was before that.

21252. I understand that, not knowing whether a breach of the Act was committed or not, and believing that those dogs were suffering torture the whole time of the lunch, you made no protest?—Yes.

21253. If you now became acquainted with a distinct breach of the law in any laboratory, should you consider it your duty to report it to the Home Secretary or not?—Certainly not.

21254. You say that in the operations that you performed yourself you have frequently allowed dogs to come out from under the influence from chloroform, and that they have suffered very much in consequence of your doing so?—They have suffered to a certain extent, yes.

21255. Where were those operations performed?—At Hyderabad. So far as possible, the animals were always kept under the influence of the anæsthetics.

21256. When you gave your evidence before, you made no reference before us to many of these important statements that have appeared in your comments here to-day?—No.

21257. Why did you not, if those facts were present to your mind then, bring them before our notice at that time?—Well, I did not wish to say anything about Dr. Gaskell, at that time especially.

21258. What caused you to change?—I did not wish to mention anybody by name.

21259. What caused you to change?—The fact that my evidence was contradicted.

21260. Had you between this date, fourteen years ago, and your giving evidence here, made no memorandum of what occurred on those occasions?—We have got the notes of the second experiment—our own notes.

21261. (*Chairman.*) Of the 11th you mean?—Yes, on the 11th.

21262. (*Mr. Ram.*) Nothing with regard to what took place on the 7th?—We did not take any notes on the 7th.

21263. If you did not know that there had been any breach of the law, or did not know (as I understand you to say) what the law was, what meaning do you attach to what you say that Dr. Gaskell stated "in order to hoodwink the Inspector and satisfy the Act"?—I did not attach any more meaning than what I state.

21264. What do you state?—I state that he told me that he gave the morphin in order to comply with the Act; that he was obliged to give some anæsthetic, and he gave the morphin and called it an anæsthetic.

21265. Then it was present to your mind that there was an Act?—Yes.

21266. And an Act that had been complied with?—Yes.

21267. And an Act that specified that you must have anæsthetics?—That I did not think of.

21268. What do you mean, then, by his giving morphin and calling it an anæsthetic in order to satisfy the Act?—I understood only what he himself said, that is all; that that was why he had given it.

21269. Have you ever in the fourteen years till a month ago repeated that statement of his to anybody?—Repeatedly.

21270. Can you name anybody to whom you have repeated it?—I have mentioned it to hundreds of people in India, Dr. Bomford, Sir Lauder Brunton, and everybody, of course.

21271. To Sir Lauder Brunton?—Yes.

21272. Did you name it as indicating that Dr. Gaskell had disregarded the Act?—I do not remember.

21273. What was your motive? How came you to mention it?—I just mentioned it as a matter of interest. It was in comparing it with our own experiments.

21274. Was it a matter that filled you with horror?—I cannot say that it filled me with horror. I thought it was horribly cruel.

21275. (*Chairman.*) I understand from what you said last that what you mentioned to those gentlemen was not the cruelty, but the fact of the experiments having been a failure in your view?—That has been mentioned also.

21276. I thought you said that was what you spoke about to Sir Lauder Brunton?—I have spoken about the whole of the reports.

21277. (*Colonel Lockwood.*) About its being cruel, as well as the other thing?—Yes.

21278. (*Mr. Ram.*) About the intention of hoodwinking the Inspector?—I do not remember saying anything special about that to Sir Lauder Brunton.

21279. (*Dr. Wilson.*) Do I correctly understand that all your cross-circulation experiments out in Hyderabad were painful experiments necessarily?—In parts they were, just before the experiment was begun.

21280. That is to say, the animal had always to come out of chloroform for the second part?—Before the experiment was begun.

21281. And were those experiments to which you referred as regards shock also painful experiments?—Well, I should think there was in some experiments a certain amount of pain, but they were always done more or less under the influence of an anæsthetic. One of the members of the Commission wanted to repeat the whole of the experiments as regards shock without any anæsthetic at all, but that the Nizam's Government would not allow.

21282. Was that request formally made in writing?—No, it was not made in writing.

21283. May I ask who made it?—I would rather not mention his name.

21284. You did not satisfy yourself at the time that those animals were suffering pain; it was merely an impression?—Merely what I saw.

21285. It was merely an impression?—It was rather more than an impression; in my own mind it was a certainty.

21286. But supposing, for example, that they did receive a large dose, what is called a lethal dose, of morphia, as Dr. Shore stated, do not you think it is possible that your impression might have been wrong?—But they had not had a lethal dose.

21287. How do you know?—Because they would have died of it.

21288. I think you admitted in answer to a question of mine, that they might have survived four or five

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hours with a lethal dose?—Yes, then they would have shown signs of having had a lethal dose if they had had a lethal dose.

21289. (*Sir William Church.*) In answer to Sir Mackenzie Chalmers's question you said that your dogs did show evidence of pain when they came out of the chloroform, when you waited for a short time before giving it again. What were the symptoms of their being in pain then?—Merely struggling and whining.

21290. Then how was it that these dogs at Cambridge, if they were in such agony, did not struggle too?—They did not struggle. They were fastened down.

21291. I suppose your dogs were fastened down?—Our dogs were not fastened down in the same way that those were.

21292. And therefore the dogs that have received chloroform and are allowed to come out struggled, and the other dogs did not?—I do not say that. I do not remember very much about our dogs' struggles; it was not very much; but they were not fastened down as these dogs were.

21293. Do you mean that they were not fastened down at all?—They were fastened down just sufficiently to enable us to do what we wanted.

21294. But I am speaking of dogs which you subsequently used for your cross-circulation experiments?—Yes.

21295. They were fastened down I suppose in the same way?—They were not fastened down in the same way as these were.

21296. Do you mean that they were not fastened down so tightly?—No, not nearly so tightly; and most of our operations were done with students holding them to a certain extent.

21297. Students holding them when the connections were made between the vessels?—While the chloroform was given, while they were in the struggling stages of the chloroform.

21298. I am not speaking of while the chloroform was given; I am speaking of after the chloroform had been given and when you were waiting to give it again to see the effect of the chloroform on the blood-pressure?—We did not wait more than was absolutely necesary; there was no unnecessary waiting at all. The blood-pressure was allowed to get up to its normal level and then the experiment commenced.

21299. But I presume that you chloroformed your dogs first and then you connected the vessels?—While under chloroform.

21300. And whilst you were connecting the vessels were the dogs only held by two students?—They were held by whatever number was necessary—two, or three, or four; it did not matter how many there were. I never said only two.

21301. Do you mean that this delicate operation of uniting the vessels was done with the dogs loosely held by students and not fastened in any way?—They were fastened, but not fastened particularly tightly; and of course they only struggled when they were out of the chloroform.

21302. But they did struggle when they were out of the chloroform?—When they were partially out. They were unconscious, but they were very often partially out of the chloroform.

21303. In the case of the dogs at Cambridge, the only indication of pain they gave was shivering?—That was the only indication that I noticed particularly.

21304. Is there any difference between the narcosis of morphia and that of opium?—Not that I know of.

21305. Have you ever seen a person poisoned with opium?—Yes.

21306. A lethal dose?—Yes.

21307. Have you ever seen cases in which people have taken opium or morphia and have recovered?—In what dose do you mean?

21308. Apparently in a fatal dose, or nearly so, so that great difficulty has been found in recovering them?—Yes.

21309. Have you ever asked those persons any questions afterwards?—What about?

21310. I ask you whether you had ever asked those persons any questions at all afterwards?—Not that I recollect.

21311. You have never asked them whether they have been conscious at all of what has happened to them during their period of narcosis?—No.

21312. Do you know what happens to a person who is narcotised with morphia or opium?—Is this part of the proceedings of the Commission—to ask me my opinion about opium poisoning.

21313. Yes, it bears upon the question in this way: that you have said most distinctly that morphia is not a narcotic for dogs.

21314. (*Chairman.*) You said that you were told by Dr. Gaskell that a certain amount of morphia had been given, and there seems to be some dispute as to what effect three grains of morphia on a dog may be. It is material to ask you what your experience is about the effects of morphia?—In my opinion a three-grain dose of morphin to a dog is not an anæsthetic dose.

21315. (*Sir William Church.*) But I ask you first of all, if you would not mind giving me an answer, whether you know the condition of a man who has been narcotised by morphia or opium to a severe degree?—You mean poisoned?

21316. Not to die?—Yes, I understand—poisoned and recovering.

21317. Yes?—Yes, certainly I have.

21318. What is his condition?—His condition is a condition of coma, as everybody knows.

21319. Is he conscious of what is done to him?—Not when he is comatose, he certainly is not.

21320. Therefore you would not deny that probably another animal than man can be in the same unconscious condition?—Quite so.

21321. I only wanted to get at what you meant when you say that a three-grain dose of morphia it is impossible should in any given degree narcotise a dog?—Yes, that is my opinion, from the experiments that we did in Hyderabad.

21322. (*Chairman.*) What is a lethal dose for a dog?—I could not tell you; I do not know.

21323. You have never tested up to that?—No.

21324. (*Sir William Collins.*) Were these dogs at Cambridge not comatose?—I should not think they were.

21325. (*Sir John McFadyean.*) Are you sure that were not?—I should not think they were. I never thought about it.

21326. (*Sir William Church.*) The fact that they did not struggle like the dogs you experimented with might be due to the fact that they were narcotised?—I should not think so.

21327. Do you know whether morphia or opium administered at the same time as chloroform has greater or less effects upon the animal to which it is given?—You mean, does it deepen the anæsthesia?

21328. Yes?—I do not believe it does, unless it is given in such a dose as to produce its own effects. It would not influence the anæsthesia.

21329. And the anæsthetic would not in any way influence the action of the morphia?—The anæsthesia might be a little bit deepened, but not much.

21330. (*Sir John McFadyean.*) I think in answer to a previous question you said that you did not think four hours was an unreasonable time to be expended in preparing a laboratory dissection?—Did I say that?

21331. You were asked as to why there should have been a period of four hours during which the dogs were not under anæsthetics? You will correct me if I am wrong:; perhaps you have not stated that?—I do not quite understand what you mean.

21332. I will put it in this way: How long do you think it would take to prepare the dogs for the cross-circulation experiment by a person who had done it two or three times before?—I cannot give an opinion about that.

21333. Please let me have it as near as you can?—I should not be surprised if it took four hours.

21334. That is not owing to the great extent of the cutting that has to be done, because, after all, it is only a small slit in the dog's neck, is it not? Is it owing to the delicacy of the operation?—It is extremely extensive and difficult.

21335. It is an extremely difficult operation?—I should think so.

21336. That is what I should have thought; but unless I misunderstood, you said, in answer to Mr. Ram, that the difficulty would not be very greatly increased owing to the fact that the animal was conscious?—No; I should think it would be much more.

21337. Do you desire to correct that answer?—I said that I should think it would be much more difficult to prepare two animals for cross-circulation experiments under chloroform than if they were not under chloroform.

21338. I thought you said a minute ago that it was a very difficult operation?—Yes. I am talking of the dissection.

21339. I am talking of dissection even under an anæsthetic. Is that so?—Yes.

21340. Do you really ask the Commission to believe that anyone, even an experienced surgeon, would be able to perform such a delicate dissection as this insertion of these tubes in dogs that were not under an anæsthetic?—Certainly they could.

21341. Have you ever seen it done?—No, I have not.

21342. You would not admit that it would add enormously to the difficulty of the operation if the dog were conscious?—I do not see that it would add enormously to it; it would make it more difficult, that is all, so far as the actual dissection is concerned.

21343. So that you do not think it would have been a great advantage to have the dogs unconscious?—As regards the mere dissection, I do not think it would.

21344. (*Sir William Collins.*) How did the condition of the Cambridge dogs differ from the condition of dogs in a state of coma from morphia poisoning?—I cannot tell you.

21345. Was there any difference?—I cannot give you an opinion of my own as to dogs in a state of coma, because I do not think I have ever seen one.

21346. Then you are not prepared to say that the condition of the dogs at Cambridge was not that of coma from the morphia?—I should not think it was, most decidedly.

21347. But the dose, if Dr. Shore is right, was a much larger one than you previously apprehended?—Yes.

21348. (*Chairman.*) There are one or two questions I ought to ask you as there may be some evidence about it. You said that the animals were tightly secured?—Yes.

21349. So as not to allow of much movement, is that so?—Yes.

21350. I must ask you: would it be right to say that the bands were not used to prevent movements of the animals, and that considerable voluntary movement of the limbs would have been possible if the animals had been conscious?—No.

21351. That would not be true?—No.

21352. Supposing that either chloroform or a narcotising dose of morphia had been used, it would not have been necessary to bind them together?—While they were in a state of anæsthesia they would be quite still, of course.

21353. It would not be necessary to bind them together in that case?—No.

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21354. Then did you examine the binding particularly?—No, not at all.

21355. It is your impression about their being tightly bound?—Yes.

21356. Then as regards the administration of chloroform, do you say that at no time whilst you were there did you see any chloroform administered except the concentrated dose that killed the dog?—Yes.

21357. And that was how long before the end of the experiment; how long did it take the dog to die under it?—I cannot tell you; I have not got the notes; it was only a few minutes.

21358. And up to those few minutes before the matter ended in the dog's death, there was no chloroform administered in your presence?—No.

21359. And your belief that there was no chloroform administered is derived entirely, I understand, from what Dr. Gaskell told you?—Certainly.

21360. If he had not told you so, you would not have undertaken to say that there was no chloroform administered?—I should have assumed that chloroform had been given if he had not told me so.

21361. (*Mr. Tomkinson.*) You say on page 11A that "when the two animals are ready for the experiment to begin, chloroform can only be given to the feeder"?—Yes.

21362. "As it is useless to give it to the fed"?—Yes.

21363. Do you mean that only one of the dogs had chloroform administered to it?—Yes, only the fed had chloroform given to it.

21364. Would it not affect the other?—No, not at all. The fed is the second dog.

21365. Then as regards the anæsthetic, for the latter part of the operation, the experiment, only one dog was being anæsthetised?—Yes, and that dog would not be anæsthetised if the experiment is properly done; it would not have any effect upon it.

21366. I say that only one of the two was being anæsthetised?—But I say that would not produce any anæsthesia in the fed dog if the experiment was properly done.

21367. In neither dog?—In neither dog if you give it to the fed; that is to say, it does not get to its brain if the experiment is properly performed.

21368. Then in your opinion was one of those two dogs absolutely without anæsthetics during the whole of the operation?—Yes, except the morphia.

21369. (*Sir Mackenzie Chalmers.*) How was that dog killed then?—So far as I recollect its artery was cut, I suppose; it is generally done in that way.

21370. Then after luncheon, when the experiment began, how long did it last in your presence?—It lasted a few minutes. I can tell you about the second experiment, because I have the notes; but I cannot tell you about the first, because I have not the notes.

21371. In either case the experiment that you yourself actually saw done only lasted a few minutes?—Yes.

21372. And that consisted in giving chloroform to one of the dogs?—To the fed.

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21373. (*Chairman.*) You have given evidence before us, as you will remember?—Yes.

21374. And to a certain extent upon this question of what happened on the 7th July, 1894?—Yes.

21375. You have been shown, I believe, a statement which has been made by Colonel Lawrie with reference to what took place on the 7th July?—Yes, Dr. Gaskell read to me a statement which is a criticism of my evidence.

21376. It is with reference to what took place on that 7th July with respect to these two dogs that I want to ask you some questions. Do you remember Colonel Lawrie arriving at Cambridge on that day?—Yes.

21377. When did you see him first?—I do not remember the time.

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21378. I will begin in another way. Perhaps you remember the dogs being prepared to be operated upon?—Yes.

21379. Did you take part in that preparation?—I prepared them myself.

21380. And who else was there whilst they were being prepared?—One laboratory assistant was there certainly all the time.

21381. Is he here today?—Yes, he is here; and generally another assistant was helping.

21382. And was there any other skilled person, any

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other physiologist?—Not until Dr. Gaskell himself arrived.

21383. When did he arrive—before luncheon?—Oh, certainly before luncheon. I do not remember the hour.

21384. Did he arrive before the preparation was over?—Certainly.

21385. Was the preparation over before Colonel Lawrie arrived, do you remember?—I do not remember the hour at which he arrived.

21386. Now will you just tell us exactly what happened. You had these two dogs in the laboratory; what did you first do with them?—They were first given an injection hypodermically of hydrochlorate of morphia; that was between half-past eight and nine o'clock in the morning.

21387. What strength was that?—One dog was given 10 cubic centimetres of a 2 per cent. solution, and the other dog was given 6 cubic centimetres of the same solution.

21388. Why was there that difference?—One dog was somewhat smaller than the other.

21389. Was it proportionate to their size?—Not quite proportionate; there was rather less given to the smaller dog than was proportionate to its size.

21390. It was given less than its full proportion?—Rather less compared with the dose given to the bigger one.

21391. Were the dogs put on the table when you gave them that injection?—They were standing on the floor.

21392. How was it administered?—A dog is just patted and the syringe is put under its skin, and it is injected without any difficulty at all.

21393. And when that had been administered, did you put them on the table at once, or did you leave them for a time?—They were left for half an hour until they became drowsy and laid down.

21394. And then what happened?—Chloroform was then applied to the nostril.

21395. Were they then put on the table?—No, they were on the floor. The chloroform was put to the nostril of the animals until they were unconscious; they were then lifted on to the table.

21396. Was that done on this 7th July, when they had the chloroform after they became drowsy?—Certainly.

21397. Are you quite sure of that?—Perfectly certain.

21398. Did you watch the effect of the chloroform on them?—Certainly.

21399. Did it render them insensible?—Absolutely.

21400. You say that then they were put on the table; and what was done to them then? Were they bound in any way?—Their limbs were gently restricted by bands to the table and their heads fixed by a holder.

21401. What was the nature of the binding; was it tight or loose?—Just one narrow band to each limb, just sufficient to keep them in the required position for the operation, but not enough to prevent the dog from moving its limbs quite freely if it had wished to do so—that is to say, if it had been conscious.

21402. Were these dogs laid on their backs?—Yes, on their backs.

21403. You mean they were just sufficiently fastened to prevent their rolling over and to keep them on their backs?—Quite so.

21404. But not to prevent a good deal of motion?—No.

21405. Is that the usual way in the case of such an experiment?—Yes.

21406. Then did you proceed to operate upon them?—A tube was then put into the trachea.

21407. You opened the trachea first?—Yes.

21408. And then you put a tube in?—Yes.

21409. Did you perform any other cutting operation or any other operation?—After the tube was put in the tracheal tube was then connected to the bottle containing the A.C.E. mixture before any further cutting operation was done.

21410. When you had done that did you go on with the rest of the operation?—I went straight on with the preparation of the arteries and veins necessary for the experiment.

21411. And how long did the whole thing take you?—About two hours, I think—perhaps rather more.

21412. Were these dogs kept under chloroform all the time?—They were kept under A.C.E. all the time.

21413. The first thing that you administered after the morphia was chloroform?—Yes.

21414. And by inhaling, I suppose?—Yes.

21415. And the A.C.E.?—The A.C.E. through the tracheal tube.

21416. Is that the usual way in which you would deal with an operation of this kind?—Yes, that is the usual way.

21417. And you are quite sure that you followed it?—Certainly.

21418. Was there any reason whatever for departing from that process?—None whatever.

21419. Do you remember Dr. Gaskell coming in?—I do not remember the exact hour at which he appeared.

21420. You are speaking about a long time ago, but you remember that he did come in?—Yes, he came.

21421. Before you had finished?—Oh, yes.

21422. Was he there part of the time while you were keeping the dogs under chloroform?—He was there during the latter part of the preparation of the animal when the animal was still under the A.C.E. mixture.

21423. You cannot speak more definitely than that?—No, I cannot.

21424. Then was the laboratory attendant administering the A.C.E. under your supervision or did you do it?—The tracheal tube was in the bottle of the A.C.E. practically all the time, and it was being looked after both by me and the laboratory attendant.

21425. You were both there?—Yes.

21426. Did the dog struggle or show any sign of pain?—None whatever.

21427. Are you quite sure of that?—Quite sure.

21428. And the animals were kept unconscious throughout?—Yes, they were kept quiet.

21429. Do you remember Colonel Lawrie coming into the laboratory?—Yes, I remember him coming, but I cannot say at what hour he arrived.

21430. He says it was a little before luncheon, and that it was after you had finished the preparatory operations?—Yes.

21431. Did any conversation take place about what had been done to the dogs?—We doubtless told him what preparations we had made in the animals, but I do not remember the conversation.

21432. At any rate, I can ask you this: Did Dr. Gaskell say to him that they had not had any anæsthetic?—I heard him say no such thing.

21433. As it might interfere with or spoil the subsequent experiment with chloroform?—I am certain that he would say no such thing.

21434. Did he say that each dog had been given a 10-drop dose of morphin solution?—It is absurd to suppose that he would say any such thing.

21435. He did not say that he gave a 10-drop dose?—I did not hear him say any such thing.

21436. It would not have been true, I understand you to say, if he had said it?—No.

21437. He had given a much stronger dose?—A much stronger dose was given.

21438. But the dog had been given a much stronger dose than a 10-drop dose?—Yes, much stronger.

21439. Did Dr. Gaskell say he had given them a dose of morphin not to prevent pain, but in order to hoodwink the Inspector and satisfy the Act?—He certainly said no such thing.

21440. Or anything to that effect?—Certainly not.

21441. While Colonel Lawrie was there did the dogs show any sign of pain?—They gave no sign of pain whatever.

21442. Do you remember Colonel Lawrie and Dr. Gaskell going to luncheon?—Yes, I remember the luncheon.

21443. And after luncheon they came back into the laboratory, I understand?—Yes.

(*Dr. Gaskell.*) The luncheon was in the laboratory.

(*Chairman.*) I did not know that the luncheon took place in the laboratory in the presence of these dogs.

(*Dr. Gaskell.*) No, we have a private room at the laboratory—the professors' room.

21444. (*Chairman.*) That is what I supposed. The laboratory being the operating room, they had left the operating room to go into another room in the laboratory for luncheon?—Yes.

21445. And you lunched there, too, I suppose?—Yes.

21446. Then did you all come back into the room?—We all came back, having been absent, I think, about a quarter of an hour.

21447. When you came back just tell us what the condition of the dogs was and what was done?—While we were at luncheon the laboratory attendant never left the room, having had instructions to see that the animals were kept quiet and, if necessary, given further A.C.E. mixture. When I came back they were still perfectly quiet, and no A.C.E. mixture had been necessary. They were quite unconscious, and there was absolutely no movement.

21448. Was anything said by Colonel Lawrie either then or before or at any time to the effect that these dogs had not been anæsthetised and that they were in agony or anything of that kind?—I do not remember his making any such remark.

21449. Surely you would remember it if he had said it?—Yes, I think I should.

21450. According to your account it would not have been true?—It would have been quite untrue.

21451. After you came back what was done?—Strong chloroform was given to one of the dogs in order to perform the object of the experiment, namely, to see the effect of chloroform upon the heart, and when the object of the experiment was reached both dogs were then killed by concentrated chloroform.

21452. The object being to watch by what process and method the dogs died under the chloroform?—No. The object of the experiment was to show that chloroform depresses the heart's action and ultimately makes it stop.

21453. Is not that the same thing?—No, my Lord, it is not the same thing. In this case, although the animal's heart stopped, the animal remained breathing and had to be killed by an administration of chloroform to its brain.

21454. I am not going into the merits of the experiment, or whether it was successful or unsuccessful, but at any rate the experiment was completed—it came to its end?—The crucial experiment was completed, and then the dogs were killed by chloroform, both of them.

21455. Did they ever come back from anæsthesia?—They never regained consciousness.

21456. They suffered no pain?—They suffered no pain.

21457. (*Colonel Lockwood.*) You were present the whole time during this operation and you must have heard any conversation which ensued between Dr. Gaskell and Colonel Lawrie, and you have no hesitation in saying that the whole of Colonel Lawrie's story as to Dr. Gaskell stating that he had operated with this solution only to hoodwink the inspector is a fabrication from beginning to end?—That is so.

21458. (*Sir William Church.*) Could you tell the Commission what is a fatal dose of morphia in a dog?—No, I cannot answer that question. I have never attempted to kill a dog by morphia alone.

21459. Could you give the Commission any idea as to what is the usual dose to cause complete narcosis in a dog?—That, of course, varies with the size of the dog.

21460-1. Exactly, but could you tell us at all?—The doses that were given in these experiments were just the ordinary doses which it has been the habit in the laboratory for many years to give in all similar cases. There was no special exception made in this case.

21462. But would three grains, in your opinion, be sufficient to produce unconsciousness in a dog of a given weight?—Three grains is a large dose for a dog, except a very large dog. Much less is sufficient.

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21463. For a dog of nine pounds would three grains be a dose,—or perhaps you do not wish to say?—

(*Chairman.*) That really is not very material, is it now, because we understand that the morphia was not relied upon as the anæsthetic. Chloroform was given.

(*Sir William Church.*) But it was relied upon to keep the dog unconscious, which is really the same thing as being in a state of anæsthesia, during the time that they were remaining on the table.

(*Chairman.*) I thought that chloroform and A.C.E. mixture were given.

(*Sir William Church.*) The A.C.E. mixture was not given continuously.

(*Witness.*) May I answer that point? The A.C.E. mixture was given until the defibrination of the blood was complete. As that went on it was wanted less and less, so that it was given more and more diluted; and so far as I remember the administration of the A.C.E. was discontinued altogether just before the cross-circulation was established.

21464. Then it was continued during the time you were at luncheon?—No, it was discontinued just before we went to luncheon, and in spite of that the animals remained perfectly unconscious.

21465. That unconsciousness was due to the influence of morphia?—It was due to the profound influence of the large dose of morphia that they had had, and no doubt also to the fact that they had not yet recovered from the chloroform and the A.C.E. which had been given to them for some three or four hours.

21466. Is not the effect of giving morphia at the time that you give or just before you give either chloroform or the A.C.E. mixture to intensify the action of the anæsthetic?—Very much indeed; only a very small amount of A.C.E. is required if there has been a large dose of morphia already. But I think that in these experiments it must not be forgotten that a long administration of A.C.E. induces an unconscious condition for a considerable time after the administration of A.C.E. is discontinued; for we know, from operations on man, that very often unconsciousness in a patient continues it may be for two or three hours after the administration of the anæsthetic is stopped.

21467. What I wanted to get, and I think I have got it, is that during the time when you were at luncheon and until you began the actual experiment no A.C.E. mixture was being used?—Certainly; it had then been discontinued.

21468. (*Sir William Collins.*) On the occasion of the experiment on July 7th I understand that you were present and Dr. Gaskell, Colonel Lawrie, and a laboratory assistant?—Yes, those were certainly present.

21469. Was anyone else present on the 7th?—I do not remember that anyone else was present except perhaps another laboratory assistant.

21470. And on July 11th who were present?—On July 11th at the latter end of the experiment Dr. Anderson and Sir Clifford Allbutt were also present besides ourselves. I believe there were two of Colonel Lawrie's students, I think I may call them, present on the 7th; I had forgotten them.

21471. Was there any time on July 7th when Dr. Gaskell and Colonel Lawrie were present in the laboratory and you were not present?—No.

21472. Are you quite clear about that?—I am quite certain. They might not have been in the same room, of course.

21473. Would it have been possible for a conversation to have taken place between Dr. Gaskell and Colonel Lawrie without your hearing it?—They might have gone into another room.

21474. Then it would have been possible for a conversation to have taken place between Dr. Gaskell and Colonel Lawrie without your hearing it?—In another room, but not in the same room.

21475. I do not know the relationship of the rooms; therefore it is for you to tell me whether it would have been possible for a conversation to have taken place between Dr. Gaskell and Colonel Lawrie on the 7th July and you not to hear it?—If they had left the

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room they might have carried on a conversation which I did not hear.

21476. Was the A.C.E. mixture being given during the whole time that Colonel Lawrie was present?—No, not during the latter part of the experiment.

21477. Was the A.C.E. mixture or chloroform being given prior to the experiment with the chloroform, during any of the time when Colonel Lawrie was present?—I cannot remember, as I have forgotten the exact moment when Colonel Lawrie arrived on the scene?

21478. Would it have been possible prior to the crucial experiment with chloroform during the period when Colonel Lawrie was present, for the dogs not to have been receiving either chloroform or A.C.E. mixture?—May I ask you to repeat the question?

21479. Would it have been possible on the 7th July prior to the crucial experiment with chloroform during the period when Colonel Lawrie was present, for the two dogs to be without the obvious administration of any other anæsthetic than the morphia?—Yes, it would have been possible.

21480. (*Dr. Gaskell.*) If perchance Colonel Lawrie came into the laboratory just before lunchtime, it would have been possible then?—Yes, if he came just after the defibrination was completed and before the crucial experiment was made, in which there was an interval of first of all a quarter of an hour while we had luncheon, and perhaps another ten minutes or a quarter of an hour while we were getting ready the cross-circulation experiment. During that period there may not have been any A.C.E. administered, and Colonel Lawrie may have noticed that.

21481. (*Sir William Collins.*) Can you say from memory whether during the time when Colonel Lawrie was present A.C.E. was administered or not?—I cannot say positively that it was administered while he was there; and I can say certainly that there was a considerable period when it was not administered.

21482. What was the total length of time from start to finish of this experiment with these dogs?—On July 7th we began giving chloroform about half-past eight to nine o'clock in the morning, and we finished about 2.30 in the afternoon.

21483. That would be about from five to six hours?—Yes.

21484. During how many of those hours do you think that the A.C.E. mixture was being administered?—Right up until lunch time, which was about half-past one.

21485. Was it necessary for the purposes of the crucial experiment with choloform to secure that the blood of the dogs was relatively free from chloroform or A.C.E. mixture before the experiment began?—It was necessary that they should not be deeply under a large dose of chloroform at the time, and that was the reason why a large dose of morphia was given to the dogs; but a very small quantity only of A.C.E. mixture would be necessary to supplement the action of the morphia, if it was required at all, and such a little would not have vitiated the crucial experiment that we had in view.

21486. You say if the morphia was required at all?—If the A.C.E. mixture was required at all to supplement the morphia.

21487. I wanted to ask you whether the morphia was required at all. Would these dogs have been in a painless condition from start to finish if morphia had not been administered at all?—No. It is true that one could have performed the experiment so far as the defibrination under chloroform or A.C.E.; but after that, after that had to be discontinued, it would not have been safe, I think, unless the animals had received a large dose of morphia.

21488. Then after the cessation of administration of the A.C.E. mixture until the time of the commencement of the crucial experiment, to what do you attribute the anæsthesia of the dogs?—To the morphia that they had received, and also, as I have just explained, perhaps partly to the lasting effect of the unconsciousness after a long administration of the A.C.E.

21489. Would it be more advantageous for the experiment that the anæsthesia should be due in the main to the morphia, or to the A.C.E.?—It would be better that the anæsthesia should be due to the morphia.

21490. Was that partly the reason why morphia was administered?—No, the morphia was administered in the ordinary routine way in which experiments on dogs are made.

21491. If nothing but the morphia had been administered up to the time of the crucial experiment, would the dogs have been free from pain?—No. In the early part of the day the cutting operations could not have been done under morphia alone without the animals feeling pain.

21492. Was that because the morphia had not reached the maximum of its influence?—Yes, morphia does not reach the maximum of its influence until later. It is often not until after two or three hours after it is administered that it is at its maximum.

21493. At what time on July 7th would you say that the dogs would have been free from pain from the morphia only?—The earliest moment, you mean?

21494. Yes?—I do not like to answer that question. I did not test it. They were under A.C.E. at the time, and I should not like to say when unconsciousness would have come on under morphia alone; perhaps in about two hours; but I only suggest that. I cannot say; it was not tested.

21495. But if you are using morphia and chloroform conjointly as an anæsthetic for the purpose of an experiment, can you say when it would be safe to abandon the chloroform and rely upon the morphia exclusively for the purpose of securing freedom from pain?—Of course it would depend upon the dose of morphia that was given; but I should think, as I have just said, that after two hours or so you might abandon the chloroform administration and rely on the morphia alone provided that one was not doing any severe operation. Of course, whether you could do a severe operation or not would depend upon the dose of morphia that you had given.

21496. With any dose of morphia would it be possible to do a severe operation with perfect satisfaction as to freedom from pain?—I think with a large dose of morphia it would be possible.

21497. With such a dose as you gave to those dogs?—With that dose I do not think it would be possible to do a severe operation within the first two hours, and that is why the A.C.E. mixture was given.

21498. What would you regard as the minimum dose of morphia that should be given to such a dog as that which was experimented with on July the 7th, in order to be able to perform a considerable operation with complete satisfaction that there would be no pain?—Under morphia alone?

21499. Yes?—I cannot answer that question. I have never done such an experiment. I have never experimented on a dog under morphia alone.

21500. Do you think it possible to predicate what dose could be employed if morphia alone was administered to secure complete anæsthesia during a considerable operation?—I have no doubt that a large dose of morphia would in a dog produce it, provided that you waited a sufficient time for that dose to produce its full effect.

21501. Would it only be near the period of death that that condition would be achieved?—Oh, no; long before the period of death.

21502. Is three grains of morphia a lethal dose for a dog?—I have never watched a dog die under morphia. I cannot say whether it would kill it or not.

21503. (*Sir John McFadyean.*) What was about the size of the dogs operated on. There was a large one and a smaller one; do you remember what breed they were, or comparable with what breed in point of size?—I do not remember very accurately. I think the larger dog was a greyhound. I do not remember what the smaller one was.

21504. With regard to the way in which they were fixed down, I suppose there was nothing special with regard to the method of fixing these two particular dogs?—No; they were just kept on their backs on the table.

21505. How many pieces of apparatus or cords or bandages were applied to each limb?—One to each; one narrow cord to each limb.

21506. Where is it fastened? Is it above or below the carpus, the knee?—It is generally at the foot with

the hind leg, and with the front leg it is generally round the upper part of the elbow.

21507. I want now to ask you whether you know of any method of rigidly fixing a dog, using only one strap or bandage to each limb?—No; it would be quite impossible to fix a dog rigidly with only one band.

21508. If you allow an animal sufficient freedom for its respiratory movements, then there must be room for it to exhibit signs of pain by struggling?—Certainly; in the method that I use they could quite freely move their limbs and bodies.

21509. Then I wanted to get your opinion as to what you think about the feasibility of performing the dissection that you did on these dogs on an animal that is fully conscious?—It would have been totally impossible to perform so delicate operations as I have performed on animals conscious.

21510. (*Sir Mackenzie Chalmers.*) If I have understood you aright, at the time when Colonel Lawrie came it was just before luncheon probably?—Yes.

21511. At that time you had disconnected the A.C.E. tube from the tracheotomy tube?—Yes.

21512. So far as you remember, did Colonel Lawrie make any inquiry as to the anæsthetic that was being used or otherwise?—I do not remember his asking me that point.

21513. And you heard no conversation between him and Dr. Gaskell with reference to the use of anæsthetics?—No, I did not hear any conversation.

21514. Had you been operating for any time in that laboratory when you made these experiments? Had you worked with Dr. Gaskell for any time?—I had been experimenting in physiology for several years, from 1887.

21515. In that laboratory?—In that laboratory.

21516. Have you ever known in that laboratory a cutting experiment of any sort performed on any dog or other animal with morphia alone?—No, I have not seen it done in that laboratory under morphia alone.

21517. If it had been the practice in that laboratory to experiment with morphia alone without any other anæsthetic, would you have known about it?—Oh, yes, I should have known all about it.

21518. Would it be incorrect to say that ever in that laboratory, so far as you know, an illusory dose of morphia has been given for the sake of a nominal compliance with the Act?—No such thing is ever done in that laboratory.

21519. From anything you know, could there be any possible reason for Dr. Gaskell, as the head of the laboratory, making such a statement?—It is quite impossible that Dr. Gaskell could have ever made such a statement.

21520. If he had made it, may I take it that, from your own personal knowledge, that statement would have been absolutely incorrect?—Absolutely.

21521. In this case do I correctly understand that you performed the necessary operations before the crucial experiment both on the 7th and on the 11th?—Yes.

21522. Were the conditions in both cases the same?—Yes.

21523. Was morphia first administered, and was it followed by the A.C.E. in both cases?—Precisely the same.

21524. If a dose of morphia has been given, and that is followed up with the A.C.E. mixture for a considerable time, how long does it take after the discontinuance of the A.C.E. before its effects would wear off? When would you expect the animal to again become conscious to pain?—Of course, it depends upon the dose of morphia that one has given and the length of time.

21525. In these two operations, I mean, with the doses that you gave them?—If those animals had been left alone they would never have regained consciousness, in my opinion, but I have never left them until death. But I believe that they would never have regained consciousness.

21526. You believe that if you had never come back after luncheon on those occasions those animals would never have recovered consciousness?—I do not like to answer that question positively, because I have never left animals under those conditions; but I should think that very likely they would not.

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21527. Did you from time to time after the discontinuance of the A.C.E. examine the dogs yourself to see whether there were any signs of consciousness or returning consciousness?—We were examining them continuously.

21528. With that object?—Certainly; we had got our eye on it the whole time. It was most important for the experiment that no consciousness should come back to the animals. If it had, and they had moved the least bit, all our labour would have been in vain, and the whole experiment would have been wrecked.

21529. I want to ask you about the statement of Colonel Lawrie, whether you agree with it. Colonel Lawrie says: "From personal observation I can state positively that before lunch on the 7th of July the two dogs had been lying with unconnected tracheotomy tubes in their throats" (that means unconnected, I suppose, with the A.C.E. mixture), "which in itself is presumptive evidence that they had not up to that time had chloroform or A.C.E. mixture." Do you agree with that statement?—No; I should have thought it was evident that one had got the tracheotomy tube ready for the very purpose of giving the A.C.E. mixture, which was the fact.

21530. For what purpose in this experiment did you perform tracheotomy?—For the easy administration of the A.C.E.

21531. That was the reason the animals were tracheotomised and why you put in the tracheotomy tube?—Yes.

21532. That was wholly independent of connecting the veins for cross-circulation?—Certainly.

21533. You said that you did it for the purpose of facilitating the long-continued administration of the A.C.E. mixture?—Certainly, that is its object.

21534. If you had intended to rely on morphia alone, without the A.C.E., would tracheotomy have been necessary?—It would not have been necessary.

21535. It would have been merely superfluous trouble?—Yes.

21536. Colonel Lawrie is correct in this when he goes on to say, "During the whole of that time the animals had no anæsthetic"; he is correct in that?—I was not present in the room at that moment, but so far as I remember the A.C.E. mixture had by that time been discontinued as unnecessary.

21537. Then you came back from luncheon. Are you perfectly positive that the animals, when you came back, were unconscious and free from pain?—Yes, absolutely.

21538. Colonel Lawrie says: "When I saw the dogs they appeared to be in a condition of frightful agony, and I believe that they were fastened on the dog boards, which must in itself be most exquisite torture, for five hours, and that their necks were dissected, without any anæsthetic." Is that an absolute misapprehension?—Yes.

21539. We asked him what signs of agony he saw, and he told us that he saw shivering in the dogs. Did you notice anything of that sort yourself?—No, I did not notice any shivering, but there may have been some.

21540. Would it be possible that there should be shivering without consciousness under the anæsthetics which you administered?—There might have been movement without consciousness.

21541. With anything like convulsive movements?—No, not with convulsive movements.

21542. Would there be shivering movement?—I suppose it is possible that some movement of that sort might have occurred in an unconscious state, but it is very easy to see the difference between a conscious movement, a voluntary movement, and an unconscious movement.

21543. By a voluntary movement you mean a movement caused by conscious pain?—Yes.

21544. Were the dogs so fastened that any conscious movement of pain must have been observed?—It could have been observed at once.

21545. When you are operating upon a dog under those conditions do you apply any test of conscious-

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ness, such as touching the eye?—We are watching carefully the breathing of the animal, and we are watching carefully to see that there is no voluntary movement, and we occasionally test the conjunctival reflex.

21546. Do you remember whether you tested it in this case?—Yes, we tested it.

21547. At what periods?—Off and on throughout the whole experiment; from time to time throughout the whole experiment.

21548. By the whole experiment you mean beginning in the morning, not merely the actual administration of the chloroform, but the whole time from the first tracheotomy?—Yes.

21549. The first thing you did, of course, was tracheotomy?—Yes.

21550. That was the first cutting incision that you made, I suppose?—Yes.

21551. And then after that you applied the A.C.E. tube and then the necessary incisions for the purpose of cross-circulation were made under the A.C.E., plus morphia?—Yes.

21552. *(Mr. Tomkinson.)* Do I correctly understand that the tracheotomy had nothing to do with the main experiment?—Tracheotomy was necessary to give the anæsthetic.

21553. The anæsthetic could not have been administered through the nostrils?—It could, but it could not be controlled so readily.

21554. Then the tracheotomy had nothing to do with the experiment of cross-circulation?—No, it was simply to facilitate the means of giving the A.C.E. mixture to keep the animals unconscious.

21555. Then I suppose that the long time occupied was the preparation for the great experiment?—Yes, quite so.

21556. The tracheotomy, I suppose, took but a short time?—It is done in a few moments.

21557. Then the two things are quite separate?—Yes.

21558. I did not understand that. During the whole time that Colonel Lawrie was there, did you or did you not gather from him any expression whatever of disapproval?—I did not hear him make any remark of disapproval of what was being done.

21559. He certainly made no protest to you?—None whatever.

21560. And he did not call attention to the fact that in his opinion the dogs were suffering?—No, he did not.

21561. Nor make any allusion to it?—I heard him make no allusion to anything of the kind.

21562. And no impression of a feeling of that kind on his part was conveyed to your mind?—None whatever.

21563. Therefore I suppose you were very much surprised to hear what he said about it afterwards?—Naturally, I was very much surprised.

21564. You spoke of the continuing effects of the anæsthesia after the operation. Is that more than the somnolency caused by anæsthesia. To illustrate what I mean, my experience is that a person after an operation, on the anæsthetic being removed and recovery of consciousness, says something: "I have not felt it; I am told it is all over," and then goes to sleep for a couple of hours or three hours. Did you mean more than that?—Of course you often get that condition of continued unconsciousness, whether we call it sleep or not.

21565. You would not call it insensibility to pain, would you?—During the time that the patient is asleep the pain of the operation is not felt certainly.

21566. But if the operation were continued, the patient would wake up—like operating on a person in a sound sleep. I think your contention in this case is that the effect of a subcutaneous infusion of morphia had a long continuing effect?—Yes, the dose of morphia was producing its narcotic effect for many hours.

21567. And was sufficient to continue the anæsthesia after the A.C.E. had been discontinued?—Yes, that is my opinion.

21568. *(Dr. Wilson.)* Did Dr. Gaskell give you any instructions as to the amount of morphia that should be administered to these dogs?—No, I decided it myself.

21569. So that he could not even say what was the dose that was given?—No, he left it to me.

21570. And he was not present when the dose was given?—No.

21571. So that he could not have made that statement from actual knowledge which Colonel Lawrie stated that he made?—No, it was impossible, but I doubtless told Dr. Gaskell the dose I had given.

21572. I think you also said that the assistant was in the room during the time when you were at luncheon?—Yes.

21573. And that he had received instructions if there were any signs that the dog was suffering pain to give another dose of the A.C.E. mixture?—Yes.

21574. *(Sir Mackenzie Chalmers.)* Were exactly the same conditions repeated on the 11th that took place on the 7th?—Yes, the conditions used for anæsthetising the animals were precisely the same, and were carried out in precisely the same way.

21575. Was Colonel Lawrie present only at the final experiment on both occasions?—On the second day Colonel Lawrie appeared in the afternoon, if I remember rightly, and saw only the termination of the experiment. I do not remember exactly when he came.

21576. When did you first hear that Colonel Lawrie thought that he had seen a cruel experiment?—It was when Dr. Gaskall read to me this document that has been circulated.

21577. The other day?—Yes.

21578. But during these fourteen years you never heard a word to the effect that Colonel Lawrie thought he had seen a cruel experiment?—There was some letter, I believe, that passed between Colonel Lawrie and Dr. Gaskell which Dr. Gaskell mentioned to me, which seemed to imply that Colonel Lawrie had thought our experiments cruel.

21579. When was that?—I do not remember how many years ago that was.

21580. *(Dr. Gaskell.)* Was there the slightest difference, so far as you remember, of course, between the conditions of the animals on July 7th and July 11th?—No, there was no difference between the conditions on the two days.

Mr. William Hall, called in; and Examined.

Mr. W. Hall

21581. *(Chairman.)* You were assisting in the laboratory, were you not, on the 7th July, 1894?—Yes. I could not be sure of the date.

21582. But you remember those cross-circulation experiments which took place in July, 1894?—Yes, quite well.

21583. There was one on one day and another four days afterwards?—Yes.

21584. I am taking you to the first day?—I recollect more about the day when Professor Allbutt was there.

21585. That is the second day?—Yes, I remember more about that date.

21586. I will ask you first in a general way, were the operations done in the same way as regards preparing the dogs on both occasions?—Exactly.

21587. You assisted at both?—Yes.

21588. Will you just describe the way in which the dogs were treated. There were two dogs on the first day and there were two other dogs on the next day?—Yes.

21589. Take the case of each pair of dogs. Were they treated in the same way?—Exactly.

21590. What was done?—In the morning with the help of another laboratory assistant, and Dr. Shore being about, I injected the dogs with morphia—both dogs with a 2 per cent. solution under the skin on the back.

21591. You injected them both with morphia?—Yes, I did.

21592. Do you remember what quantity you injected?—Yes, the first dog was a medium sized dog, that would be with 6 to 8 cubic centimetres. I was not particular to a cubic centimetre.

21593. And the larger dog?—The larger dog I injected with nine or ten; I could not say to a cubic centimetre.

21594. Proportionately?—Yes, in proportion.

21595. Then after they had been injected with morphia, were they left for a time?—Quite half an hour.

21596. And then?—Then, with some help, I laid the dog on its side, with Dr. Shore present, and I held a cloth over the nose of the dog with chloroform upon it, and the dog did struggle a little, but nothing to speak of.

21597. When you put the chloroform over its nose?—Yes.

21598. And then?—Being pretty well under morphia, it did not struggle much, but it did a little till it got really under the influence of the chloroform.

21599. Did it get thoroughly under the influence of chloroform?—Yes, on the floor, quite.

21600. Then what did you do?—I lifted it up with some help on to the operating table.

21601. Did you fasten it?—The legs were fixed with string, just round each leg. The head was fixed in a sort of clamp.

21602. Was it laid on its back?—Yes.

21603. Was that all the fastening it had—what you have described?—Yes.

21604. Do you remember Dr. Shore cutting the trachea?—Yes, quite well.

21605. And putting in the tube in the trachea?—Yes.

21606. That I understand was for the A.C.E. to be administered?—Yes, it is much easier that way than any other way to be done.

21607. When that was done did you give it any A.C.E.?—Immediately the tube was in, yes, the A.C.E. was put in. As a rule, it is always done.

21608. At that time was the dog insensible when you began to administer the A.C.E.?—Quite; it kept well under the chloroform all the time the duster was kept round the nose.

21609. Did you put the chloroform over its nose at intervals, or was it there all the time?—I kept it on until the trachea tube was in.

21610. Then did Dr. Shore proceed to operate on the dog?—Yes.

21611. That would be making fresh incisions altogether in the dog for the purpose of laying open the arteries?—Yes.

21612. And he made those?—Yes.

21613. The tracheotomy tube remaining in the slit in the trachea?—Yes, all the time.

21614. Did you keep on administering the A.C.E.?—I took the tube in and out of the bottle very likely, but when I did that I should say to Dr. Shore, "I have taken the tube out, sir," or, "I have put it in again, sir," so as to give warning. *Mr. W. Hall.* 25 Mar. 1908.

21615. But it was continually applied?—Yes, put in and out of the bottle in case of necessity.

21616. Then was the cross-circulation established in the glass tube?—Yes, when it was ready to be done.

21617. You mean after luncheon, just before the end of the operation, that was the last thing?—Yes, getting towards the last thing to be done.

21618. You do not remember, I suppose, about the luncheon, the times and the hours particularly?—I know I should not have gone at all, because it would be over at about three or half-past. I often stay until the thing is over, and then go to lunch afterwards. I could not be relieved in those particular cases.

21619. You never left the room then?—No.

21620. During the whole time, from the time that you administered the injection of morphia until the dogs were dead, you were in the operating room the whole time?—Yes.

21621. At any time during that period after you had administered the chloroform for the first time up to the death were the dogs sensible; had they any consciousness of pain?—I did not see anything of consciousness.

21622. Were they under the influence of these narcotics or anæsthetics?—Yes, one or other of them.

21623. Did you see any struggling?—No, I did not.

21624. And did you hear, or do you remember, at any rate, any conversation between Dr. Gaskell and Colonel Lawrie?—I cannot say I did. I should not take any notice. I should be too busy.

21625. You have no recollection after this lapse of time?—No.

21626. But you are quite sure that that was the process of treatment of the dogs that you have described to us?—Yes, I am quite sure.

21627. And that applies also to the 11th, I understand?—Yes, it was all the same.

21628. (*Mr. Tomkinson.*) Then the dogs were under your special eye the whole time, were they?—I and Dr. Shore, yes. I stopped all the time; I stayed in the room all the time.

21629. Was the anæsthetic connected with them from another room?—In the same room. A Woulff's bottle stood beside them against the head.

21630. It did not come from another room?—No; there was no need.

21631. Was it under your superintendence as well?—Yes. I had to help anyhow.

21632. (*Dr. Gaskell.*) Just one word, William. Dr. Shore and myself had done a number of other experiments previous to those two?—Yes.

21633. Of exactly the same character?—Yes.

21634. And you had assisted in those?—In every one, I think.

21635. They were precisely the same character in every case?—Exactly the same.

Mr. H. K. Anderson, M.D., F.R.S., called in; and Examined.

21636. (*Chairman.*) You were present, I believe, at the cross-circulation operation which took place on the 11th July, 1894, in the Cambridge Laboratory?—Yes.

21637. Are you yourself experienced as a physiologist and experimenter?—I have had seventeen years' experience, because I was a medical man to begin with. In 1894 I had had three years' experience as an experimenter. I became a Demonstrator of Physiology in 1891, and since that time have been experimenting with Professor Langley.

21638. Did you go to the laboratory at Dr. Gaskell's request?—Yes.

21639. To watch the operation?—Distinctly; that was why I was there. *Mr. H. K. Anderson.* M.D., F.R.S.

21640. As to its success?—And to answer for what exactly happened.

21641. You mean with a view to seeing what was ascertained by the experiment?—Yes.

21642. I am not going at all into the success or want of success or anything of that kind—it is merely on a very small point that I want to ask you, and I should not have asked it you if Colonel Lawrie had not said that what took place on the 7th and 11th was precisely the same, and that he observed the

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same things. You saw the experiment on the 11th?—Yes.

21643. And you did not see that on the 7th?—No.

21644. Were you there when the dogs were prepared?—No.

21645. You were only there on the 11th after the trachea had been cut and the chest laid open for the experiment?—Yes, I went in and out of the room; my recollection is that I went in and out while Dr. Shore was whipping the blood, but I was not in the room continuously till the real experiment began.

21646. But do you remember enough of what you saw to say whether the dogs were under chloroform or not?—Clearly.

21647. Or under an anæsthetic, I should say, because the A.C.E. was used as well as chloroform?—Yes.

21648. But during the period when Dr. Shore was operating, were the animals under anæsthetics?—Certainly, I should say they were anæsthetised.

21649. Have you any doubt about it?—None.

21650. Then you watched the experiment afterwards?—Yes, closely.

21651. How long would that watching period of the experiment take?—My recollection is that I had luncheon with Dr. Gaskell and Colonel Lawrie's two assistants, and that the experiment lasted, I should say, three hours—from two to three hours.

21652. After luncheon?—Yes.

21653. Watching the process?—Yes.

21654. Watching the action of the blood vessels?—Watching the effect on the heart and the respiration.

21655. During that time were the animals under anæsthetics or not?—Yes, they were under morphia.

21656. Were they conscious of pain?—I should say distinctly not.

21657. If there had been any struggling, would it have interfered with the observation?—As an experimenter myself I cannot imagine the experiment being performed in that case. I should expect that either the small glass tubes would break or the blood-vessels, especially the veins, would give way. I had never seen a cross-circulation before, and I was quite surprised at the delicacy of the operation.

21658. Stillness on the part of the dogs was essential to its success?—Yes.

21659. Is it an operation that you would think of performing unless the absence from pain was secured first?—No.

21660. (*Colonel Lockwood.*) You say that you did not closely observe the dogs when you went into the room. What reason have you, then, for stating that you believe the dogs to have been thoroughly anæsthetised?—I did not mean that. I meant that I was working in the laboratory and went in and out of the room to begin with but when I came in to watch the actual experiment, I observed the dogs most closely.

21661. Anyhow, you noticed that the dogs took sufficient of the anæsthetic to be absolutely certain that they were totally unconscious of pain?—I am quite convinced of that.

21662. (*Sir Mackenzie Chalmers.*) I do not think we have your scientific qualifications. You are a Fellow of the Royal Society?—Yes, and Doctor of Medicine and Fellow of Gonville and Caius College.

21663. And what is your appointment?—I am now Lecturer in Physiology. I was then Demonstrator.

21664. We may take it, then, that you are continually seeing experiments on animals and are familiar with all the procedure?—I have performed a very great number of experiments with Dr. Langley and by myself since I went to Cambridge in 1891. I was then performing experiments almost daily.

21665. This was in 1894?—Yes.

21666. At that time you were an experienced physiologist?—Yes.

21667. You were able to form an opinion, and a skilled opinion, as to whether the animals were suffering or not?—I think so, quite.

21668. Do you know anything whatever about the experiment on the 7th of July; were you in the laboratory then?—No, I had nothing to do with that experiment; I heard of it.

21669. Were you in the laboratory—in and out?—I was usually there daily. It is most probable that I was there.

21670. Do you think that if the animals under experiment then had been in great pain you would have noticed it and perceived it?—Well, I was not in the room.

21671. You were not in the room in which the experiment on the 7th was performed?—No, not on the 7th; I do not think I was in the room on the 7th at any time.

21672. Can you give us any opinion on this point? Colonel Lawrie said that he observed shivering in these animals; would that be a sign of pain or not?—No, I think not. When an animal is under anæsthetics, unless great care is taken to keep it warm, the animal may shiver. Mere cold may excite slight twitching of the skin in a dog.

21673. But any skilled person observing that animal and operating upon that animal, would know whether or not the animal was suffering from pain?—Certainly.

21674. When an animal has been originally under an anæsthetic, and is tied down, when the anæsthetic passes off is there any difficulty in knowing when consciousness of pain begins?—I do not think I have ever seen consciousness of pain in an animal during an experiment under the licence alone. I have experimented also on animals as I would on human beings. In such cases the animals as they come round from the anæsthesia, like human beings, exhibit uneasiness; but I do not think I have ever witnessed anything that I could call consciousness of pain, except the discomfort of a surgical wound after an operation.

21675. I take it that as soon as you observe any uneasiness the anæsthetic is again applied?—Yes, as soon as the animal began to move—and any movement at once calls attention to the anæsthesia—I should try the corneal reflex. In most experiments if the animal moved it would destroy the success of the experiment. and I should notice that the anæsthesia was passing off by the change in the respiration even before the animal moved.

21676. You have seen several cross-circulation experiments?—No.

21677. Only one?—Only one.

21678. Can you tell us what is the object of performing tracheotomy on the animal in connection with these cross-circulation experiments?—The greater ease of giving the anæsthetic. By means of the slit in the tracheal tube the amount of anæsthetic necessary to keep the animal evenly under anæsthetics can be adjusted to a nicety. That is my object in inserting a tracheal tube.

21679. That is the reason why tracheotomy would be performed in addition to the necessary incisions for the cross-circulation?—Yes.

21680. (*Mr. Ram.*) Do you think it is possible that the dogs could have been prepared for this operation, and have tracheotomy tubes inserted, without having an anæsthetic when they were so inserted?—It would be quite impossible except by using sheer force.

21681. You told us that the operation is an exceedingly delicate one?—Most delicate.

21682. In your opinion, would it be, in the first place, reasonably feasible to insert these tubes at all unless the animal were insensible under an anæsthetic?—I should say it would be impossible.

21683. You said, in answer to Lord Selby, that you would not think of doing such an operation as this without the animal being thoroughly under anæsthetics?—Yes.

21684. That is the reason why you would not do it; do you mean either on humanitarian grounds or on the ground of getting a successful operation, or both?—For both reasons.

21685. But, quite apart from humanitarian desire, would it be possible, in your opinion, to hope for a successful operation unless the animal were properly anæsthetised during the whole time?—It would be impossible unless it was curarised.

21686. (*Colonel Lockwood.*) Made immovable?—Yes. Made immovable.

21687. (*Dr. Gaskell.*) Would this operation be pos-

sible under curare?—No, because the whole point of the experiment depended on watching the respiration.

21688. (*Mr. Ram.*) Apart from that, this particular operation, I understand, could not have been done under curare?—No.

21689. It would have neutralised the effect that you desired to secure?—Yes.

21690. (*Mr. Tomkinson.*) Could these dogs, if they had been in great pain, have evinced it to you?—Yes.

21691. If they had been suffering the agonies which they have been represented to have been suffering, what signs could they have shown to you?—In the first place, it is quite a misrepresentation to say that the dogs were tied tightly to the stand. The stand used in the laboratory allows quite free movement of the dog. Any dog would begin to move its body and wriggle its head if it was in pain.

21692. And there were no such signs?—None whatever.

21693. Would the tracheotomy prevent the emission of a cry on the part of the dog?—Certainly.

21694. Then the dog could not cry?—No.

21695. Does tracheotomy stop the voice?—It must.

21696. Then you could not depend upon a sound from a dog upon which tracheotomy had been performed as an indication of pain?—No, of course not.

21697. But other signs they could have given in the way of movement or restlessness if they had been suffering?—Yes. *Mr. H. K. Anderson, M.B., I.M.S.* 25 Mar. 1908.

21698. And you did not see any?—No.

21699. You are quite satisfied of that?—Yes, quite satisfied.

21700. (*Dr. Wilson.*) As regards the shivering to which reference has been made, I think you said that there might be some possibility of that having been witnessed?—Yes.

21701. From cold?—Yes, and also if the muscles are bared in the neck there may be twitching of the muscles. The mere exposure of the muscles may excite twitching.

21702. But shivering from cold would not be because the dog felt the cold; it would be reflex action?—Yes, and be due to the depressing effect of the morphia and the anæsthetic on the dog's temperature.

21703. Not because any cold was felt, of course?—No.

21704. May I ask whether at all times in the Cambridge Laboratory every possible care is taken to carry out the intentions of the Act—that is to say, that all experiments shall be carried out painlessly?—Every attention.

Mr. W. H. Gaskell, M.D., F.R.S., a member of the Commission, Examined.

21705. (*Chairman.*) You were not examined on this particular point, I think. Your name was not mentioned by Colonel Lawrie when he gave his evidence, although you were rather pointed to as the physiologist who had performed certain experiments?—No, in the original evidence certainly my name was not mentioned, and I was wondering in my own mind when he said it whom he meant when he spoke of one-twelfth of a grain of morphia. I did not conceive for a single moment that he could mean me.

21706. Colonel Lawrie came down by appointment to Cambridge, I believe?—Yes, he came down at his own suggestion, not by any suggestion of mine, but because he desired to show us that his experimentations in cross-circulation were right and that ours were wrong. We said, We will show both if you like; so he brought two Indians over with him for that purpose.

21707. He suggested that he came as your guest. He was not coming merely as your guest, but as a business matter in physiology?—He was coming there on a business matter for that purpose. The reason why he did not show us anything was because I had written over to India to explain to him that he must get a licence first before he could make those experiments in Cambridge, and he did not get that licence.

21708. He came over on the 7th of July. You have heard the evidence of Dr. Shore as to what I will call the preparation of the dogs for the final experiment that you were to make—the experiment of observation?—Yes.

21709. What did you see of that part of the business?—I have endeavoured to carry back my memory to remember when I came into the laboratory, but I cannot tell you. All I know is that I did come before luncheon in order to receive Colonel Lawrie. You must understand that we had made many of these experiments before; the research was finished, the whole thing was done, and there were simply two extra ones to please Colonel Lawrie.

21710. You had made this very experiment frequently before at the request of Colonel Lawrie?—At the request of the Nizam's Government; and we had sent our report over, and it was all finished and done with. That was two years before or more.

21711. Then I want to know this. This was not the first time that you had had this experiment in your laboratory by any means?—No, not by any means; but what I want to point out in respect of that is that I was not in hardly any case or very seldom present right through, because I was not doing the experiment myself. It was Dr. Shore, with the assistance of the laboratory boy, who was doing it, and could do it much better than if I tried to put my oar in. *Mr. W. H. Gaskell, M.D. F.R.S.*

21712. In all these experiments that you have had, have you gone in and seen what was going on from time to time?—Yes. The experiments were of my devising.

21713. What was the regular course as regards morphia and anæsthetics?—The course was exactly what Dr. Shore and William Hall have described to you.

21714. And that had been followed in all previous experiments?—Yes.

21715. Was there any reason on earth why it should not have been followed in this one experiment?—Absolutely none. There is every reason against it.

21716. I suppose you find it difficult to recall your exact observation on that particular morning?—I have not the very faintest conception what I talked to Colonel Lawrie about. I only know what happened, but as to what the conversation was when he first came and I received him, and Dr. Shore was in the operating theatre, I have not the slightest notion.

21717. Can you tell me this? Can you say whether you told him that you had given a 10 drop dose of morphin solution not to prevent pain, but in order to hoodwink the Inspector and satisfy the Act?—I can only say with respect to the first statement that he probably did ask me what dose was given, and I told him the dose, for the simple reason that the actual dose that was given is on the record or the protocol written in my own handwriting, and that was written on that very day. I looked it up the other day in Dr. Shore's rooms; it was covered with dust, old and shabby, but there it was, the exact amount given. That was done, I know, on that very day. It was 10 cubic centimetres of a 2 per cent. solution, that is what I wrote, and that is what I told Colonel Lawrie If Colonel Lawrie manufactures it into 10 drops of morphin, that has nothing to do with me.

21718. If it was only that memories might be mistaken about whether you said 10 drops or 10 grains, that would be a very different matter; but he goes on to say what is more important. Did you say to him that the morphia had been given not to prevent pain, but in order to hoodwink the Inspector?—I have told you I have not the faintest remembrance of what I did say, but I am absolutely certain that I never said that. I should never have dreamed of saying it; and it is such a silly thing to say.

21719. How often does the Inspector come and visit your laboratory?—There are no regular times; he turns up at odd times.

Mr. W. H. Gaskell, M.D., F.R.S.

25 Mar. 1908.

21720. I do not quite see what the suggestion is how it could have hoodwinked him?—Not in the least; it could not have been done.

21721. You did not say it?—I am certain. I never could have said it.

21722. Do you remember watching the experiment after luncheon?—I remember perfectly well watching the experiment, and I remember also perfectly well watching the second experiment, and asking Sir Thomas Clifford Allbutt and Dr. Anderson to come in because I was dissatisfied. I wanted the evidence of witnesses, I mean to say, after the first experiment.

21723. I will ask you this: Did Colonel Lawrie say anything to you about the dogs being not sufficiently anæsthetised or in pain or anything of the kind?—I am absolutely certain that he never mentioned that, for that is a sort of remark that I should have remembered. When I came in—I cannot say whether it was just before Colonel Lawrie came in or not—the dogs all the time I saw them were absolutely quiescent. There was not a movement or a sign of movement, and in addition to that the record shows that the blood-pressure tracing was perfectly steady, and the respiration was perfectly steady, too. So that those two things which would have shown any indication of pain in the dog as well as movement were absolutely absent. I am as convinced as I am of anything in my life that those two dogs were fast asleep.

21724. If dogs when undergoing an experiment of that sort were without anæsthetics and in a condition of frightful agony—which is the phrase Colonel Lawrie used—would the operation have been possible?—As a member of this Commission, and also as a person of a certain amount of common sense, I say it stands to reason that it is utterly impossible. You have got here two dogs fastened together simply by stitches in the skin. Upon that flap of skin you have those delicate tubes connecting their two jugulars and delicate tubes connecting their arteries. If those dogs were not anæsthetised—as Colonel Lawrie tries to make out from what I said to him—those dogs were not under anæsthetics at all. There was, according to him, only 1-12th of a grain of morphia given, and that was nothing, and there were no anæsthetics of any kind.

21725. And his statement is that you said so?—I say that is the evidence he has put in. I absolutely defy any person whatever of any kind to make that experiment under those conditions. It is absolutely and utterly impossible. The very smallest amount of struggling must break through that connection and must damage the experiment. There is one essential condition of success—that is, absolute quiescence of these animals, and that is why I say that the charge brought against me—at least, the reminiscence of what I said to Colonel Lawrie—is silly.

21726. If there had been any struggling you could hardly have forgotten it—it would have affected the success of the experiment immediately, and the thing would have failed?—The thing is in my memory perfectly well, seeing the animals absolutely quiescent. There was not a movement, and as I ventured to suggest, Sir Thomas Clifford Allbutt would tell you the same. He was a person, mind you, not accustomed to laboratory experiments, and who would, therefore, naturally notice anything of the kind rapidly.

21727. Thank you. I only wished to put to you the exact statement Colonel Lawrie put into your mouth?—Quite so.

21728. (*Sir William Church.*) Have you got any tracing or record of the circulation or respiration of these dogs that you could put in?—I have got it absolutely.

21729. (*Sir Mackenzie Chalmers.*) On the 7th as well as the 11th?—Yes, both of them, perfect from beginning to end.

21730. (*Sir William Church.*) Would you wish to put in a tracing recording the pulse and circulation of these dogs?—I should like it very much.

21731. (*Sir Mackenzie Chalmers.*) It is quite possible that you had some conversation with Colonel Lawrie about the administration of morphia?—I think it is very likely.

21732. But, as a fact, in these experiments, you did not rely on morphia alone, but A.C.E. was given in addition?—Absolutely.

21733. (*Chairman.*) And chloroform to start with?—Chloroform to start with.

21734. (*Sir Mackenzie Chalmers.*) Chloroform followed by A.C.E.?—Yes.

21735. In your opinion, would the morphia alone given in this case have been sufficient to produce complete anæsthesia?—I have no experience of morphin, you must understand, but from all I have ever read or heard I should have said that 3 grains of morphin would have produced complete unconsciousness to pain after an interval of time. That is what I should have said, without the slightest doubt.

21736. You are certain that these dogs throughout the whole time you saw them were absolutely free from pain?—The whole time I saw them they were lying absolutely quiet. There was no movement, and they were breathing regularly and quietly. That was, to my mind, conclusive evidence that they were not suffering pain.

21737. I want to get this clear. You saw them during the whole time that Colonel Lawrie saw them?—I saw them during the whole time that Colonel Lawrie saw them.

21738. Is it absolutely incorrect to say that in the laboratory either on this experiment or any other morphia has been given by itself to hoodwink the Inspector or for any similar purpose?—It is absolutely ridiculous.

21739. If Colonel Lawrie understood you to say that it is an absolute misapprehension?—An absolute misapprehension.

21740. If he had asked you how these animals were anæsthetised you must have told him it was not only morphia, but A.C.E. mixture as well?—Most certainly.

21741. The A.C.E. mixture, I suppose, always gives a smell, does it not; it must have been perfectly obvious coming into the room where these dogs were that A.C.E. had been used?—I think that would be a little difficult to say, because a physiological laboratory rather smells of different smells, you see, among which you distantly have A.C.E. and chloroform, and that kind of thing. I mean that I do not think my nose would detect that it was freshly used.

21742. (*Mr. Ram.*) There are one or two statements that I want to put to you, if I may, other than those which Lord Selby put to you. The first is this. Colonel Lawrie said: "The two dogs were lying ready for the commencement of the experiment with unconnected tracheotomy tubes in their throats. Dr. Gaskell said they had not had any anæsthetic because it might interfere with or spoil the subsequent experiment with chloroform." We have been told to-day by the laboratory assistant that he gave them morphia, but, after leaving them for half an hour, he then gave them chloroform, and that before the throats were opened at all. Is it a fact that any anæsthetic might interfere or spoil the subsequent experiment with chloroform?—Only, as Dr. Shore and Colonel Lawrie himself have pointed out, at the stage when you desire to test the action of chloroform. At the preliminary stage, of course not.

21743. Following that up is a second statement that he made that "the two dogs had been lying with unconnected tracheotomy tubes in their throats, which in itself is presumptive evidence that they had not up to that time had chloroform or A.C.E. mixture." What is your comment upon that?—That sentence conveys no meaning to me whatever—absolutely none. I do not know what he means by unconnected tracheotomy tubes; what he expected them to be connected with. I asked someone to ask him that question to see whether he knew how chloroform was given in the laboratory, and he clearly did not know how it was given. He had no conception that all that was required was to dip the end of the tube into the Woulff's bottle containing the A.C.E. mixture. The tube itself, the metal tube, with that little piece of india-rubber at the end of it, you put in and out of the bottle as you like. He had no conception apparently of that. He talks about the absence of any evidence of chloroform being taken when the bottle was on the table all the while.

21744. (*Chairman.*) That bottle did not contain chloroform?—A.C.E. There were two bottles, one with chloroform and one with A.C.E.

21745. (*Mr. Ram.*) May I take it, therefore, that what Colonel Lawrie says he saw here is not presump-

tive evidence or any other evidence that the animals had not had at that time chloroform or A.C.E. mixture?—Absolutely.

21746. Did you, in fact, say that the dogs had not had any anæsthetic?—As I have already said before, I have not the faintest notion what I said or did not say, but I am absolutely certain that I could not have said that.

21747. The fact is that they had to your knowledge had it?—Yes.

21748. I have only one other matter to trouble you with. Colonel Lawrie says, and he has elaborated it to-day: "A certain amount of cruelty is inseparable from any cross-circulation experiment"—and he stated that in his own experiments he allowed the animal at a certain moment to recover from the chloroform, and that it suffered?—That it struggled.

21749. And in his opinion suffered?—Yes.

21750. In your opinion is that a necessary accompaniment of any cross-circulation experiment?—No, if he had given a sufficient dose of morphia, as we did, that would not have been the case. It would not have interfered with the experiment.

21751. At the moment the chloroform was removed the morphia would have prevented the animal suffering any pain?—Absolutely. I am sure the animal was fast asleep. His animals distinctly show pain by struggling. In our case not even he was able to say that there was any movement.

21752. *(Dr. Wilson.)* During the crucial experiment, of course, only one dog was under the influence of chloroform?—It was not a question of the influence of chloroform. It was a question of proving the action of chloroform; that was all. The chloroform was not given as an anæsthetic then; it was given to test the action of chloroform.

21753. And, of course, complete narcosis was induced in both animals during the crucial operation by the amount of morphia that had been given?—And, as Dr. Shore has explained to you also, by the previous administration of A.C.E. and chloroform.

21754. And these two experiments on the 7th and 11th were really only repeat experiments of a considerable number that had been performed previously?—I will not say a considerable number, because it is a long, laborious, and difficult operation, and it was not necessary to make many.

21755. The same course was pursued?—Yes; the number is published. I do not remember now how many.

21756. And the same amount of morphia given?—The same procedure absolutely.

21757. *(Sir Mackenzie Chalmers.)* Did I rightly understand you to say that the very object of the tracheotomy was to enable a respirable anæsthetic to be administered and regulated?—Yes.

21758. *(Mr. Ram.)* Accurately?—Accurately.

21759. *(Chairman.)* I will just ask you this. Sir Thomas Clifford Allbutt was present on the 11th, but not on the 7th?—Yes.

21760. He is ill, and not able to be here?—Yes.

21761. He has sent a letter and a certificate?—Yes.

(Chairman.) I think we had better not have the letter read in the absence of the writer.

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APPENDIX I.

HOME OFFICE FORMS

IN USE UNDER THE

VIVISECTION ACT, 1876.

(Put in by Home Office.)

Form 1.

APPLICATION FOR LICENCE.

39 & 40 Vict., c. 77.

Address

Date

To the Right Honourable the Secretary of State for the Home Department.

SIR,

I* beg to apply under the above-mentioned Act for a Licence for the performance of experiments on animals.

The object which I have in view in applying for a Licence is†

The experiments which I propose to perform are‡

The place in which it is proposed that the experiments are to be performed is§

* Here insert name and profession (see Sec. 11 of Act) of Applicant.

† Here state for what purpose the Applicant desires to obtain a licence to perform experiments on living animals.

‡ Here indicate the general nature of the experiments which it is intended to perform; also state, if that is the case, the intention of Applicant to send in a certificate or certificates (describing each certificate by its appropriate letter) with reference to the same experiments, or any other circumstances that may be material. See also Note at the end of the application.

§ Here insert registered place. If the place is not registered it will be necessary for the person having authority over the building to apply to the Secretary of State for its registration.

Form 1 (*cont.*).

This application is supported by the recommendations appearing below.

I am,

SIR,

Your obedient Servant.

* _____

* Here Applicant to sign his name.

We recommend that the above application be granted.

1. _____

† Here the person recommending is to sign his name.

‡ _____

‡ Here specify statutory qualification (see Sec. 11).

2. † _____

‡ _____

N.B.—If the Licence is held alone without Certificate, the animal must be kept in anæsthesia throughout the whole of the experiment, and if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, it must be killed before the anæsthesia has passed off.

Certificate A dispenses altogether from the obligation to use an anæsthetic. It will be necessary in cases of simple inoculation calculated to give pain but not involving any surgical operation.

„ B dispenses from the obligation to kill the animal before the anæsthesia has passed off; it is necessary therefore whenever the initial operation is to be done under anæsthetics but the animal is to be allowed to survive.

„ C is necessary for Experiments illustrating Lectures.

„ E is never held alone, but is necessary whenever any experiment is to be performed on a Dog or Cat under Certificate A.

„ E E is never held alone, but it is necessary whenever any experiment is to be performed on a Dog or Cat under Certificate B.

„ F is necessary whenever any experiment is to be performed on a Horse, Ass, or Mule.

P T.O

Form 1 (*cont.*)

39 & 40 Vict., c. 77, Sec. 11.

11. Any application for a licence under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons; that is to say,

The President of the Royal Society;

The President of the Royal Society of Edinburgh;

The President of the Royal Irish Academy;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin;

The President of the General Medical Council;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

Form 2

CERTIFICATE A.

Official Copy.

39 & 40 Vict., c. 77.

WHEREAS*

* Here insert name, address, and profession of person to whom Certificate is to be given. (Sec. 11 of Act.)

has represented to us†

† Here insert name, address, and statutory qualification of each person certifying. (Sec. 11 of Act.)

that he proposes, if duly authorised under the above-mentioned Act, to perform on living animals certain experiments described below: We hereby certify that, in our opinion, insensibility in the animal on which any such experiment may be performed cannot be produced by anæsthetics without necessarily frustrating the object of such experiment.

‡ This Certificate is given for_____experiments.

‡ This Certificate shall be in force until the_____day of_____19____

‡ A certificate may be given for such time, **OR** for such series of experiments as the person signing may think expedient; and it is desirable that such limitation should be here inserted. If a Certificate is unlimited, or limited *by time* only, the Secretary of State usually imposes a limit on the number of experiments to be performed.

Experiments are not to be performed under this Certificate until the Licensee has been informed that it has not been disallowed by the Secretary of State. The Certificate, even if given for a longer period, ceases to have effect on the expiration of the Licence.

Signatures of Certifiers to be attached here

Date

§ Description of proposed experiments

§ *At the end of the Description the animals are to be named, and it is necessary to submit, in addition, if dogs or cats are used, Certificate E, or if horses, asses or mules are used, Certificate F.*

‖ Object of these experiments:—

Here state in what manner the experiments will be of service in advancing by new discovery physiological knowledge, or knowledge which will be useful for saving or prolonging life or alleviating suffering.

N.B.—This Certificate is subject to any conditions that may be assigned in the Licence, and it will be the duty of the holder to refer to his Licence and ascertain whether any of the conditions attached to the Licence limit the number of Experiments authorised by this Certificate or in any other way place restrictions on what may be done under this Certificate.

Form 2 (cont.).

39 & 40 Vict., c. 77, Sec. 11.

11. Any application for a licence under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons; that is to say,

The President of the Royal Society;

The President of the Royal Society of Edinburgh;

The President of the Royal Irish Academy;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin;

The President of the General Medical Council;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

Form 3.

CERTIFICATE B.

Official Copy.

39 & 40 Vict., c. 77.

WHEREAS*

Here insert name, address, and profession of person to whom Certificate is to be given. (Sec. 11 of Act.)

has represented to us†

† Here insert name, address, and statutory qualification of each person certifying. (Sec. 11 of Act.)

that he proposes, if duly authorised under the above-mentioned Act, to perform on living animals certain experiments described below, such animals being, during the whole of the initial operation of such experiments under the influence of some anæsthetic of sufficient power to prevent their feeling pain. We hereby certify that, in our opinion, the killing of the animal on which any such experiment is performed before it recovers from the influence of the anæsthetic administered to it would necessarily frustrate the object of such experiment.

‡ This Certificate is given for _____ experiments.

‡ This Certificate shall be in force until the _____ day of _____ 19

‡ A Certificate may be given for such time, **OR** for such series of experiments as the person signing may think expedient; and it is desirable that such limitation should be here inserted. If a Certificate is unlimited, or limited *by time* only, the Secretary of State usually imposes a limit on the number of experiments to be performed.

Experiments are not to be performed under this Certificate until the Licensee has been informed that it has not been disallowed by the Secretary of State. The Certificate, even if given for a longer period, ceases to have effect on the expiration of the Licence.

Signatures of Certifiers to be attached here

Date

§ Description of proposed experiments.

§ *At the end of the Description the animals are to be named, and it is necessary to submit, in addition, if dogs or cats are used, Certificate EE, or if horses, asses or mules are used, Certificate F.*

Object of these experiments :—

‖ Here state in what manner the experiments will be of service in advancing by new discovery physiological knowledge or knowledge which will be useful for saving or prolonging life or alleviating suffering.

N.B.—This Certificate is subject to the conditions laid down by the Act and to any conditions that may be assigned in the Licence, and it will be the duty of the holder to refer to his Licence and ascertain whether any of the conditions attached to the Licence limit the number of Experiments authorized by this Certificate, or in any other way place restrictions on what may be done under this Certificate.

[P.T.O

Form 3 (cont.).

39 & 40 Vict., c. 77, Sec. 11.

11. Any application for a licence under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons; that is to say,

The President of the Royal Society;

The President of the Royal Society of Edinburgh;

The President of the Royal Irish Academy;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin;

The President of the General Medical Council;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

Form 4.

CERTIFICATE C.

Official Copy.

39 & 40 Vict., c. 77.

WHEREAS*

* Here insert name, address, and profession of person to whom Certificate is to be given. (Sec. 11 of Act.)

has represented to us†

† Here insert name, address, and statutory qualification of each person certifying. (Sec. 11 of Act.)

that he proposes, if duly authorized under the above-mentioned Act, to perform at

by way of illustration of lectures to be there delivered, certain experiments described below on living animals, such experiments being performed under the provisions contained in the said Act as to the use of anæsthetics: We hereby certify that, in our opinion, the proposed experiments are absolutely necessary for the due instruction of the persons to whom such lectures are to be given, with a view to their acquiring physiological knowledge, or knowledge which will be useful to them for saving or prolonging life or alleviating suffering

This Certificate does not come into operation until one week after a copy has been forwarded to the Secretary of State, and even if given for a longer period ceases to have effect on the expiration of the Licence.

Signatures of Certifiers to be attached here }

‡ Description of proposed experiments.

‡ The animal must during the whole of the experiment be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain; and the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered. Sec. 3 (3) and (4).

§ Persons before whom the proposed experiments are to be performed.

§ The class of students and the curriculum for which they are being prepared should be given here.

Objects of the proposed experiments.

N.B.—This Certificate is subject to any conditions that may be assigned in the Licence, and it will be the duty of the holder to refer to his Licence and ascertain whether any of the conditions attached to the Licence limit the number of Experiments authorised by this Certificate, or in any other way place restrictions on what may be done under this Certificate.

[P.T.O

Form 4 (*cont.*).

39 & 40 Vict., c. 77, Sec. 11.

11. Any application for a licence under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons; that is to say,

The President of the Royal Society;

The President of the Royal Society of Edinburgh;

The President of the Royal Irish Academy;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin;

The President of the General Medical Council;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

Form 5.

CERTIFICATE D.

39 & 40 Vict., c 77.

WHEREAS† has represented to us‡ that he proposes, if duly authorized under the above-mentioned Act, to perform on living animals certain experiments described below, for the purpose of testing the former discoveries described below, alleged to have been made for the advancement of physiological knowledge, or knowledge which will be useful for saving or prolonging life or alleviating suffering: We hereby certify that, in our opinion, such testing is absolutely necessary for the effectual advancement of such knowledge.

† Here insert name, address, and profession of person to whom Certificate is to be given. (Sec. 11 of Act.)

‡ Here insert name, address, and statutory qualification of each person certifying. (Sec. 11 of Act.)

§ This Certificate is given for _____ experiments.

§ This Certificate shall be in force until the _____ day of _____ 19 _ _

§ A Certificate may be given for such time or for such series of experiments as the person signing may think it expedient; and it is desirable that such limitation should be here inserted. If a Certificate is unlimited, or limited *by time* only, the Secretary of State usually imposes a limit on the number of experiments to be performed.

Signatures of Certifiers to be attached here ‡

Date

* Description of proposed experiments.	Description of former discoveries for the purpose of testing which the proposed experiments are to be made.

* This Certificate, when required, is in addition to any other Certificates that may be required for the same experiments. The experiments should be described alike in all the Certificates.

N.B.—This Certificate is subject to any conditions that may be assigned in the Licence, and it will be the duty of the holder to refer to his Licence and ascertain whether any of the conditions attached to the Licence limit the number of Experiments authorized by this Certificate, or in any other way place restrictions on what may be done under this Certificate.

[P.T.O.

Form 5 (cont.).

39 & 40 Vict., c. 77, Sec. 11.

11. Any application for a licence under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons; that is to say,

The President of the Royal Society;

The President of the Royal Society of Edinburgh;

The President of the Royal Irish Academy;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin;

The President of the General Medical Council;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

Form 6.

CERTIFICATE E.

Official Copy.

39 & 40 Vict., c. 77.

WHEREAS*

has represented to us†

* Here insert name, address, and profession of person to whom Certificate is to be given. (Sec. 11 of Act.)

† Here insert name, address, and statutory qualification of each person certifying. (Sec. 11 of Act.)

that he proposes, if duly authorized under the above-mentioned Act, to perform on dogs and cats the experiments described below without anæsthetics: We hereby certify that, in our opinion, for the reasons specified below, the object of any such experiment will be necessarily frustrated unless it is performed on an animal similar in constitution and habits to a dog or cat, and that no other animal is available for any such experiment.

‡ This Certificate is given for_____experiments.

‡ This Certificate shall be in force until the _____day of_____19_____

‡ A Certificate may be given for such time OR for such series of experiments as the person signing may think expedient, and it is desirable that such limitation should be here inserted. If a Certificate is unlimited, or limited *by time* only, the Secretary of State usually imposes a limit on the number of experiments to be performed.

This Certificate does not come into operation until one week after a copy has been forwarded to the Secretary of State, and even if given for a longer period ceases to have effect on the expiration of the Licence.

Signatures of Certifiers to be attached here

Date

§ Description of experiments to be performed :—

§ *This Certificate never stands alone, but if it is necessary it accompanies Certificate A, and the description of the experiments in this Certificate should be identical with the description in Certificate A. This Certificate is not required when experiments are performed under the Licence alone.*

Reasons why the object of any such experiment will be necessarily frustrated unless it is performed on an animal similar in constitution and habits to a dog or cat, and why no other animal is available for any such experiment :—

N.B.—This Certificate is subject to any conditions that may be assigned in the Licence, and it will be the duty of the holder to refer to his Licence and ascertain whether any of the conditions attached to the Licence limit the number of Experiments authorized by this Certificate, or in any other way place restrictions on what may be done under this Certificate.

Form 6 (*cont.*)

39 & 40 Vict., c. 77, Sec. 11.

11. Any application for a licence under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons; that is to say,

The President of the Royal Society;

The President of the Royal Society of Edinburgh;

The President of the Royal Irish Academy;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin;

The President of the General Medical Council;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

Form 7.

CERTIFICATE EE.

39 & 40 Vict., c. 77.

WHEREAS*

* Here insert name, address, and profession of person to whom Certificate is to be given. (Sec. 11 of Act.)

has represented to us†

† Here insert name, address, and statutory qualification of each person certifying. (Sec. 11 of Act.)

that he proposes, if duly authorized under the above-mentioned Act, to perform on living animals certain experiments described below, such animals being, during the whole of the initial operation of such experiments under the influence of some anæsthetic of sufficient power to prevent their feeling pain; and that he is submitting to the Secretary of State a Certificate B, which, if not disallowed, will dispense from the statutory obligation to kill the animal on which any such experiment is performed before it recovers from the influence of the anæsthetic. We hereby certify that, in our opinion, for the reasons specified below, the object of any such experiment will be necessarily frustrated unless it is performed on an animal similar in constitution and habits to a dog or cat, and that no other animal is available for any such experiment.

‡ This Certificate is given for_____experiments.

‡ This Certificate shall be in force until the_____day of_____19____

‡ A certificate may be given for such time **OR** for such series of experiments as the person signing may think expedient, and it is desirable that such limitation should be here inserted. If a Certificate is unlimited, or limited *by time* only, the Secretary of State usually imposes a limit on the number of experiments to be performed.

This certificate does not come into operation until one week after a copy has been forwarded to the Secretary of State, and even if given for a longer period ceases to have effect on the expiration of the Licence.

Signatures of Certifiers to be attached here

Date

§ Description of experiments to be performed :—

§ *This Certificate never stands alone, but if it is necessary it accompanies Certificate B, and the description of the experiments in this Certificate should be identical with the description in Certificate B. This Certificate is not required when experiments are performed under the Licence alone.*

Reasons why the object of any such experiment will be necessarily frustrated unless it is performed on an animal similar in constitution and habits to a dog or cat, and why no other animal is available for any such experiment :—

N.B.—This Certificate is subject to the conditions laid down by the Act and to any conditions that may be assigned in the Licence, and it will be the duty of the holder to refer to his Licence and ascertain whether any of the conditions attached to the Licence limit the number of Experiments authorized by this Certificate, or in any other way place restrictions on what may be done under this Certificate.

P.T.O.

Form 7 (cont.).

39 & 40 Vict., c. 77, Sec. 11.

11. Any application for a licence under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons; that is to say,

The President of the Royal Society;

The President of the Royal Society of Edinburgh;

The President of the Royal Irish Academy;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin;

The President of the General Medical Council;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

Form 8.

CERTIFICATE F.

39 & 40 Vict., c. 77.

Official Copy.

This Certificate is always required when experiments are performed on horses, asses or mules, and, in addition, there will be required, if the experiments are to be performed without anæsthetics, Certificate A, or if the experiments are under anæsthetics, but the animals are to remain alive after anæsthesia has ceased, Certificate B. It is necessary that the description of the experiments should be the same in all the certificates.

WHEREAS *

* Here insert name, address, and profession of person to whom Certificate is to be given. (Sec. 11 of Act.)

has represented to us †

† Here insert name, address, and statutory qualification of each person certifying. (Sec. 11 of Act.)

that he proposes, if duly authorised under the above-mentioned Act, to perform on horses, asses or mules, the experiments described below: We hereby certify that, in our opinion, for the reasons specified below, the object of any such experiment will be necessarily frustrated unless it is performed on a horse, ass, or mule, and that no other animal is available for such experiment.

‡ This Certificate is given for _____ experiments.

‡ This Certificate shall be in force until the _____ day of _____ 19____

‡ A Certificate may be given for such time, **OR** for such series of experiments as the person signing may think expedient; and it is desirable that such limitation should be here inserted. If a Certificate is unlimited, or limited *by time* only, the Secretary of State usually imposes a limit on the number of experiments to be performed.

Experiments are not to be performed under this Certificate until the Licensee has been informed that it has not been disallowed by the Secretary of State. The Certificate, even if given for a longer period, ceases to have effect on the expiration of the Licence.

Signature of Certifiers to be attached here }

Date

§ Description and object of experiments to be performed:—

§ At the end of the Description state in what manner the experiments will be of service in advancing by new discovery physiological knowledge or knowledge which will be useful for saving or prolonging life or alleviating suffering.

Reasons why the object of any such experiment will be necessarily frustrated unless it is performed on a horse, ass, or mule, and why no other animal is available for any such experiment:—

N.B.—This Certificate is subject to any conditions that may be assigned in the Licence, and it will be the duty of the holder to refer to his Licence and ascertain whether any of the conditions attached to the Licence limit the number of Experiments authorised by this Certificate, or in any other way place restrictions on what may be done under this Certificate.

Form 8 (*cont.*).

39 & 40 Vict., c. 77, Sec. 11.

11. Any application for a licence under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons; that is to say,

The President of the Royal Society;

The President of the Royal Society of Edinburgh;

The President of the Royal Irish Academy;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin;

The President of the General Medical Council;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

Form 9.

RECORD OF EXPERIMENTS.*

on Living Animals, performed by_____, at_____.

No.	Date.	Animals.	With or without Anæsthetics.	Under what Certificate.	Description of Operation or Dissection, if any is required.	Medicinal, Poisonous, Infectious or other agents and the mode of administration, if any are employed.	Further particulars or remarks by Holder of Licence.
1							
2							
3							
4							
5							
6							
7							
8							
9							
10							

* This Record is to be sent in to the Inspector whenever it is demanded by him.

*Signature*_____ *Date*_____

Form 10.

RETURN OF EXPERIMENTS

Performed in the Year 1905 by the holder of Licence No. ____

TABLE 1.—NUMBER OF EXPERIMENTS.

Under Licence alone.	Under Certificates.						
	A	A + E	B	B + EE	C	D	F

N.B.—Should the Licensee hold two or more Certificates distinguished by the same letter, he is requested to give the number of experiments performed under each of such Certificates.

TABLE 2.—GENERAL PURPOSE OF EXPERIMENTS.

Number of Physiological.	Number of Pathological.	Number of Therapeutical.

TABLE 3.—EXPERIMENTS ATTENDED BY PAIN.

Number.	Animals used.	General Nature of Experiments.

TABLE 4.—LIST OF WRITINGS BASED UPON EXPERIMENTAL WORK, PUBLISHED IN 1905.

Full Title of Paper.	Exact reference to Periodicals in which it appeared.	Name of Society to which it was communicated.

Signature _____

Date _____

Form 10 (*cont.*).

41.

Home Office,

Whitehall,

15th December, 1905.

It is requested that you will state on the annexed form, 1st, the Number of Experiments on living Animals performed by you in the year 1905, under your Licence, or any Special Certificate you may have held during the year; 2nd, the general purpose of the Experiments; and 3rd, the number of those Experiments in which there was reason to believe that any appreciable pain was inflicted, specifying the kind of animal that suffered, and stating in general terms the nature of the Experiment that occasioned pain. I shall be obliged if you will also inform me whether any of the experiments were performed on behalf of any Public or Official Body.

You are also requested to furnish a complete list (on the enclosed form) of any writings, based upon experimental work, which you may have published during the year 1905, giving the full title of each paper with an exact reference to any periodical in which it may have appeared, and the name of any Society to which it may have been communicated.

You are reminded that the furnishing of copies of such papers is made a condition of your Licence, and are requested, if you have not already done so, to forward copies to the Secretary of State without delay.

G. D. Thane,

Inspector

Form 11.

(*Pro Formâ* Copy of a Licence endorsed with all "Conditions" which are in general use).

39 & 40 Vict., c. 77.

No. of Licence *100.* .. 100,000.

LICENCE.

In pursuance of the powers vested in me by the above-mentioned Act, I, the Right Hon. *Herbert John Gladstone*, one of His Majesty's Principal Secretaries of State, hereby grant to *John Smith, Esq.*, of *1, Blank Street, London, W.*, this Licence to perform experiments subject to the restrictions and provisions contained in the said Act.

This Licence will not authorize the holder to perform any of the experiments for the performance of which a Certificate is required by the Act, unless he also holds the necessary Certificate or Certificates

This Licence, unless earlier revoked, shall be in force up to the *28th day of February, 1907*, or for such longer period as may from time to time be hereafter authorized by a Secretary of State in writing, and is given subject to the conditions annexed hereto.

Home Office, Whitehall,

25th day of *June, 1906.*

CONDITIONS.

1. Attached to all licences.

1. This Licence authorises the holder to perform experiments at the following place only, viz.:—[*The Medical and Scientific Departments, University College, London.*]

2. Attached to licences granted to Students of Medicine.

2. That all experiments carried out under this Licence shall be performed under the supervision [*Professor A. B., F.R.S., of University College, London.*]]

3. Attached to all licences.

3. The holder must keep a written record of his experiments. Such record shall be open to examination at any time by the Inspector, and a report of all experiments performed shall be furnished to the Inspector on the 31st day of December, and at any other time when required.

4. Attached to licences of all holders of Certificates A. or B. where Secretary of State does not limit the number of experiments to be performed.

4. That after the completion of the experiments detailed in Certificates (*A. No. 1., A. No. 2, E. No. 1., A. No. 3, and F. No. 1*) or on the 31st day of December, at latest, in each year in which the said Certificates are in force, and at any time when required by the Inspector, the holder of this Licence is to report to the Inspector, on the prescribed form, the number and nature of the experiments performed.

5. Used in cases where a Certificate A. would permit of experiments more severe than simple inoculations, etc.

5. That no operative procedure more severe than simple inoculation or superficial venesection may be adopted in any of the experiments under Certificates (*A. No. 1, A. No. 2, E. No. 1, A. No. 3, and F. No. 1*).

6. Used in all Certificate A. cases; and in some Certificate B. cases where antiseptics would not be applicable.

6. That if an animal, after and by reason of any of the said experiments under the said Certificates (*A. No. 1, A. No. 2, E. No. 1, A. No. 3, and F. No. 1*), is found to be in pain, which is either considerable in amount or is likely to endure, and if the main result of the experiment has been attained, the animal shall be immediately killed under anæsthetics.

7. Used in all Certificate A. or Certificate B. cases where Secretary of State limits the number of experiments to be performed.

7. That not more than (*20*) of the experiments detailed in Certificate (*B. No. 1*) may be performed and of these, not more than (*10*) may be on cats or dogs under Certificate (*EE. No. 1*), and after the completion of the said (*20*) experiments, or on the 31st day of December, at latest, in each year in which the said Certificates are in force, and at any time when required by the Inspector, the holder of this Licence is to report to the Inspector, on the prescribed form, the number and nature of the experiments.

8. Used in Certificate B. cases where two operations may be performed in the course of the experiment.

8. That all operative procedures in connection with the Certificates (*B. No. 1 and EE. No. 1*) be carried out under anæsthetics of sufficient power to prevent the animal from feeling pain.

9. Used in Certificate B. cases wherever there is a wound to which antiseptics could be applied.

9. That the animals experimented on under Certificates (*B. No. 1 and EE. No. 1.*) be treated with strict antiseptic precautions, and if these fail and pain results, that the animals be immediately killed under anæsthetics.

10. Used in all Certificate C. cases.

10. On the completion of any experiment under Certificate (*C. No. 1*) the animal shall be killed forthwith by or in the presence of the experimenter.

11 Attached to all licences.

11. That in the event of descriptions of any experiment performed by the said Licensee and requiring a Licence under the said Act appearing in any medical, scientific, or other journal or magazine, or in a report of any lecture delivered by the said Licensee, printed for publication or for private circulation, the said Licensee shall transmit to the Secretary of State the said journal or magazine, or the fullest of such printed publications or reports of lectures, accompanied by a letter drawing attention to the publication.

12. Attached to all licences.

12. Such conditions as the Secretary of State may think fit to prescribe.

Whitehall, 25th June, 1906.

Form 11 (*cont.*).

Extracts from 39 *and* 40 *Vict.*, *c* 77,

2. A person shall not perform on a living animal any experiment calculated to give pain, except subject to the restrictions imposed by this Act. * * * *

2. The following restrictions are imposed by this Act with respect to the performance on any living animal of an experiment calculated to give pain; that is to say,

(1.) The experiment must be performed with a view to the advancement by new discovery of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering; and

(2.) The experiment must be performed by a person holding such licence from one of Her Majesty's Principal Secretaries of State, in this Act referred to as the Secretary of State, as is in this Act mentioned, and in the case of a person holding such conditional licence as is hereinafter mentioned, or of experiments performed for the purpose of instruction in a registered place; and

(3.) The animal must during the whole of the experiment be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain; and

(4.) The animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered; and

(5.) The experiment shall not be performed as an illustration of lectures in medical schools hospitals, colleges, or elsewhere; and

(6.) The experiment shall not be performed for the purpose of attaining manual skill.

Provided as follows; that is to say,

(1.) Experiments may be performed under the foregoing provisions as to the use of anæsthetics by a person giving illustrations of lectures in medical schools, hospitals, or colleges, or elsewhere, on such certificate being given as in this Act mentioned, that the proposed experiments are absolutely necessary for the due instruction of the persons to whom such lectures are given with a view to their acquiring physiological knowledge or knowledge which will be useful to them for saving or prolonging life or alleviating suffering; and

(2.) Experiments may be performed without anæsthetics on such certificate being given as in this Act mentioned that insensibility cannot be produced without necessarily frustrating the object of such experiments; and

(3.) Experiments may be performed without the person who performed such experiments being under an obligation to cause the animal on which any such experiment is performed to be killed before it recovers from the influence of the anæsthetic on such certificate being given as in this Act mentioned, that the so killing the animal would necessarily frustrate the object of the experiment, and provided that the animal be killed as soon as such object has been attained; and

(4.) Experiments may be performed not directly for the advancement by new discovery of physiological knowledge, or of knowledge which will be useful for saving or prolonging life or alleviating suffering, but for the purpose of testing a particular former discovery alleged to have been made for the advancement of such knowledge as last aforesaid, on such certificate being given as is in this Act mentioned that such testing is absolutely necessary for the effectual advancement of such knowledge.

4. The substance known as urari or curare shall not for the purposes of this Act be deemed to be an anæsthetic.

5. Notwithstanding anything in this Act contained, an experiment calculated to give pain shall not be performed without anæsthetics on a dog or cat, except on such certificate being given as in this Act mentioned, stating, in addition to the statements herein-before required to be made in such certificate, that for reasons specified in the certificate the object of the experiment will be necessarily frustrated unless it is performed on an animal similar in constitution and habits to a cat or dog, and no other animal is available for such experiment; and an experiment calculated to give pain shall not be performed on any horse, ass, or mule except on such certificate being given as in this Act mentioned that the object of the experiment will be necessarily frustrated unless it is performed on a horse, ass, or mule, and that no other animal is available for such experiment.

6. Any exhibition to the general public, whether admitted on payment of money or gratuitously, of experiments on living animals calculated to give pain shall be illegal. * * * *

7. The Secretary of State may insert, as a condition of granting any licence, a provision in such licence that the place in which any experiment is to be performed by the licensee is to be registered in such manner as the Secretary of State may from time to time by any general or special order direct; provided that every place for the performance of experiments for the purpose of instruction under this Act shall be approved by the Secretary of State, and shall be registered in such manner as he may from time to time by any general or special order direct.

8. The Secretary of State may license any person whom he may think qualified to hold a licence perform experiments under this Act. A licence granted by him may be for such time as he may think fit, and may be revoked by him on his being satisfied that such licence ought to be revoked. There may be annexed to such licence any conditions which the Secretary of State may think expedient for the purpose of better carrying into effect the objects of this Act, but not inconsistent with the provisions thereof.

9. The Secretary of State may direct any person performing experiments under this Act from time to time to make such reports to him of the result of such experiments, in such form and with such details as he may require

Form 11 (*cont.*).

11. Any application for a licence under this Act and a certificate given as in this Act mentioned must *be signed by* one or more of the following persons ; that is to say,

The President of the Royal Society ;

The President of the Royal Society of Edinburgh ;

The President of Royal Irish Academy ;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin ;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin ;

The President of the General Medical Council ;

The President of the Faculty of Physicians and Surgeons of Glasgow ;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science ;

and *also* (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college, in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

22. This Act shall not apply to invertebrate animals.

APPENDIX II.

(Put in by Home Office.)

SUMMARY OF REPORTS ON THE LAWS RESTRICTING THE PRACTICE OF VIVISECTION IN BRITISH INDIA, BRITISH COLONIES AND FOREIGN COUNTRIES

NOTE.—These reports were received in 1904 from H.M. Representatives abroad in reply to enquiries initiated by the Home Office.

(i.) BRITISH INDIA.

There is no legislation restricting vivisection in British India. A Bill drafted in 1892 was allowed to drop as unnecessary. In scientific institutions over which the Government has any control the lines of the English Act 39 and 40 Vict., c. 77, are followed.

(ii.) BRITISH COLONIES.

Apart from the general law as to cruelty to animals, there is no legislation dealing with vivisection in Canada, Cape of Good Hope, Natal, New Zealand, Jamaica, or Barbadoes. In the Australian Colonies the only enactments dealing directly with this question are those of Victoria and Queensland.

In Victoria, sections 12 and 13 of the Animals Protection Act, 1890, exempt persons practising vivisection from the penalties of that Act, and provide for the registration of practitioners and for the making of regulations.

In Queensland, section 12 of the Animals Protection Act of 1901 is in similar terms.

(iii.) FOREIGN COUNTRIES.

There are no special enactments or regulations restricting the practice of vivisection in Belgium, Bulgaria, France, Greece, Hesse, Hungary, Portugal, Roumania, Russia, Servia, Spain, Sweden, and Norway, Turkey, or the United States of America.

In two countries there is definite legislation, viz. :—Denmark and Switzerland (cantons of Geneva and Zurich).

In the following countries the practice is regulated by Government instruction issued to universities, schools, and other institutions :—Austria, Baden, Bavaria, Germany, Holland, Italy, and Saxony.

In Bavaria, Belgium, Germany, and Russia, there is, however, general legislation providing penalties for those guilty of cruelty to animals, which might presumably be applicable to those who practise vivisection ; but in France the general law relating to cruelty to animals only applies to such experiments held in public as are attended with excessive cruelty, not to those conducted in private ; in Holland the law as to cruelty to animals does not apply to ill-treatment with a scientific object ; in Sweden the general law would not seem to affect cases of cruelty to animals occurring in a laboratory, while in Norway an Act coming into force this year (1905) as to cruelty to animals specially provides that the Sovereign or anyone to whom the Royal Authority may be delegated shall not be restricted from granting special permission to special persons for conducting experiments on animals which may entail suffering ; in the United States of America, the law as to cruelty to animals specially excepts from the provisions of the Statute scientific experiments conducted under the authority of the faculty of some regularly incorporated college, university, or scientific society.

As to other foreign countries, it is not stated whether or not there is legislation as to cruelty to animals which might be applicable to those who perform experiments on living animals.

APPENDIX III.

MEMORANDUM BY MR. G. D. THANE.

(Inspector under the Act.)

(*a*) Inspection of Unregistered Places.

Mr. S. Coleridge stated (qu. 10262 and 10645) that by giving a licensee permission to vivisect in private places he is thereby placed beyond the possibility of legal inspection, because by Clause 10 of the Act of 1876 the inspector has no right of entry into, or inspection of, any unregistered place.

Experiments performed in unregistered places fall into two groups.

In the first the experiments are performed in order to investigate outbreaks of disease in animals, such as "braxy," "Louping ill," and "struck," in sheep. It is impossible to say where an outbreak will occur; it may affect a wide area, the place may be remote, and it is often necessary to inoculate a considerable number of animals in the open country. It is evident that registration is not practicable in these cases; but the licensee is required, when he reports the experiments to the inspector, to state the name of the place or places in which such experiments have been performed.

In the second group are included cases in which, owing to special circumstances, it is not possible to perform the experiments in a registered laboratory.

In the few instances, within my experience, in which permission has been granted to perform certain experiments in a specified place, not registered for the performance of experiments generally, it has been for a limited period, a condition has been added to the licence that the place shall be open to inspection by the inspector, and they have all been indicated in the annual reports. Thus, in the report for 1903, it is stated :—

> "... two licensees, who were permitted to perform certain experiments dealing with the effects of increased atmospheric pressure, and requiring the employment of apparatus which was not available at a laboratory, at a place which was not registered; this place was open to inspection, and has been frequently visited."

During the year 1907 a similar permission was granted in one case, and the place was visited while the experiments (inoculations) were in progress.

(*b*) Certificate A, and the "Pain" or "Inoculation" Condition.

This condition was introduced in 1887 in the following form :—

> "That if severe pain has been induced in an animal after any of the said experiments has been performed under the said Certificate A, No. , and if the main result of the experiment has been attained, it is a condition of this licence that the animal be immediately killed under anæsthetics."

In 1899 it was slightly modified, thus :—

> "That if an animal, after and by reason of any of the said experiments under the said Certificate , is found to be in pain which is either considerable in amount or is likely to endure, and if the main result of the experiment has been attained, the animal shall be immediately killed under anæsthetics."

Since 1887 the condition, substantially unaltered, has been invariably used with Certificate A, except in cases when it was obvious that pain could not ensue from the procedures contemplated and that it was consequently unnecessary. I am not able to recall a case in which Certificate A has been allowed to come into operation without this condition. The suggestion that the condition has been from time to time abandoned, or materially modified (see Questions 10272–10275) is unfounded.

I may be allowed to call attention to the following extract from the first Annual Report which I made, viz., that for the year 1899 :—

> "No experiments requiring anything of the nature of a surgical operation, or that would cause the infliction of an appreciable amount of pain, are allowed to be performed without an anæsthetic."

A similar statement is contained in every Annual Report which I have presented, and since 1901 it has been in the following form :—

> "In no case has a cutting operation more severe than a superficial venesection been allowed to be performed without anæsthetics."

It is further stated, from 1900 onwards :—

> "In no instance has a certificate dispensing with the use of anæsthetics been allowed for an experiment involving a serious operation."

It will therefore be seen that, during the time as to which I can speak from personal knowledge, Certificate A, which dispenses with the use of anæsthetics, has not been allowed for any operative procedure more severe than the opening of a subcutaneous vein, for which no one would think of using an anæsthetic in man; that is, in other words, that vivisection of an animal, in the proper sense of the term, without anæsthetics, has not in any case been allowed by the Secretary of State.

In reply to Question 12918 reference is made by a witness to Certificate A2. The numeral here means that this is the second Certificate A which has been submitted by the holder. There is only one form of Certificate A, or of any other certificate; although of course when the wording of a certificate has been altered there have been for a time both the old and the new forms in operation until the former expired or was disallowed.

(*c*) Certificate B.

In reply to question 10560, Mr. Coleridge stated—"that that certificate admits of a very large amount of extreme pain and awful suffering, because under that certificate the animal may be kept disembowelled alive without anæsthetics as long as actual cutting operations are not being performed upon it."

The change in the wording of Certificate B from "during the whole of the experiments" to "during the whole of the initial operation of such experiments" was made in 1892 because the experiment continues during the whole of the time following the operation while the effects of the operation are being developed, and only terminates when the animal is finally killed.

The "initial operation" covers all proceedings until the wound is closed and dressed. It has never been regarded otherwise, either in the Home Office or by licensees, and in no case within my experience have procedures calculated to cause pain been carried out on an animal during or after the operation without the animal being anæsthetised. I never heard the suggestion that such a thing was permissible until it was made by Mr. Coleridge. In my report for 1906 I state that "no observations or stimulations of a character to cause pain are allowed to be made without the animals being anæsthetised."

No certificate B, which would permit of painful procedures without anæsthetics, has in my experience been allowed to come into operation by the Secretary of State

G. D. Thane.

March 7th, 1908.

I N D E X.

ABERDEEN:

Experiments on animals meet with small opposition in, *D. J. Hamilton*, 20210-11.
Municipal laboratory at, *G. D. Thane*, 339.

University of:

Demonstration experiments at, *A. R. Cushny*, 5159.
Evidence on behalf of, *see* HAMILTON, D. J., 19984-20378.

ABERNETHY, DR, reference *Rev. J. P. Hopps*, 8329.

ABRATH, DR., prosecution of, for abuse of Vivisection Act, Section VI., and conviction *A. G. Scott*, 19568-75, 19681-5.

ACTINOMYCOSIS, *see under* PARASITIC DISEASES.

ACTS OF PARLIAMENT, *see under* PARLIAMENT.

ADDISON, clinical observations of, as to functions of suprarenal glands, *Miss A. Kenealy*, 6637, 6642.

ADVISORY COMMITTEE ON PLAGUE IN INDIA, work of, and Commission emanating from, *C. J. Martin*, 12286-91.

AFRICAN COAST FEVER:

Failure of Koch's experiments and present accurate knowledge of cause of, *S. Stockman*, 3403-8.
Koch's preventive inoculation for, not successful in Rhodesia *S. Stockman*, 3331-50.
Transmitted by agency of blood-sucking ticks *S. Stockman*, 3410-14.

AGRAMONTE, DR., member of United States Commission to investigate cause of yellow fever in Havannah, *W. Osler*, 16531.

ALCOCK, DR., assisted in experiment described in *Shambles of Science*, Chapter XI., *Miss Lind-af-Hageby*, 7448-50.

ALDENHAM, laboratory at, held by Lister Institute of Preventive Medicine *G. D. Thane*, 344.

ALEXANDRIA, plague outbreak in, and successful treatment by application of sanitary methods *Sir G. Kekewich*, 20485-90.

ALLBUTT, SIR T. CLIFFORD, present at Dr. Gaskell's cross circulation experiments *L. E. Shore*, *W. H. Gaskell*, 19465, 21470, 21726.

ALLINGHAM, WILLIAM, M.D., letter to, *re* "incomplete anæsthesia" *Hon. S. Coleridge*, 10993.

ANÆSTHESIA:

Analgesia not necessarily coincident with, *A. R. Cushny*, *F. Gotch*, 5270-4, 13898-900.

"Complete":

Can be always obtained in experiments, *Sir R. D. Powell*, *H. Morris*, *E. A. Schäfer*, *F. Gotch*, *Sir J. F. Moulton*, 5771-3, 8020, 10047-53, 12809-11, 13586-7.
Can occur without abolition of corneal reflex *D. W. Buxton*, 12523-4.
Definition *D. W. Buxton*, 12544-56.
First movements which return after, are not indicative of consciousness *J. N. Langley*, 15274-5.
Must be capable of preventing all shock *D. W. Buxton*, 12662-6.
Narcotics can induce, *D. W. Buxton*, 12681.
No difficulty in maintaining, throughout experiments *D. W. Buxton*, 12679-80.
No difficulty in securing, for length of time *Sir J. Russell*, 695.
Present guarantees of, unsatisfactory *J. H. Levy*, 18477-83.

ANÆSTHESIA—*cont.*

"Complete"—*cont.*

Same precautions taken to ensure, in experiments on animals as in operations on human beings *D. W. Buxton*, 12461-6, 12684-7.
Secures against all pain *E. A. Schäfer*, 10069-71A.
Skilled anæsthetist has no difficulty in ascertaining, *D. W. Buxton*, 12419-20.
Symptoms indicating, vary slightly with nature of anæsthetic used *D. W. Buxton*, 12421-7.
Vivisection only justifiable if there is guarantee of, and animal is not allowed to survive *J. H. Levy*, 18477-83.

Consciousness, and reasons for supposing sharp dividing line exists between *S. F. Smith*, 13082-3.
Consciousness disappears long before movement, and power of motion returns before sensation *H. Morris*, *E. A. Schäfer*, 7937-52, 10222-4.

Deaths from:

No inquest or public inquiry held in Scotland as to, *Sir V. Horsley*, 15861-2.
Possible difficulty in differentiating between death from shock or, *Sir V. Horsley*, 15863-4.

Different degrees of, *E. H. Starling*, *D. W. Buxton*, 3889-90, 4062, 12510-22.
General, for human beings sometimes prohibited by patient's condition *H. Snow*, 2399-400.
Hypnotism an unreliable substitute for, *J. N. Langley*, 15270.
Imperfect, implied by use of curare *H. Snow*, 2357-75.

"Incomplete":

Alleged deception in letters to surgeons with regard to meaning of, unfounded *Hon. S. Coleridge*, 19930-2, 19967-74.
Correspondence with surgeons and Home Office as to use of, in human operations *Hon. S. Coleridge*, 10991-11043.
Definition *D. W. Buxton*, 12544-56.
Experiments described in *Shambles of Science* were performed under, *Miss Lind-af-Hageby*, 9130-1, 9157-97.
Explanation of term as used in connection with Dr. Crile's experiments, *Sir V. Horsley*, 15721-8.
Frequently advised in physiological text books *Miss Lind-af-Hageby*, 7223-97.
Movement of animals on operating table indicate, *Miss Lind-af-Hageby*, 9661-4.
No possibility of, during experiments *A. R. Cushny*, 5175-8.
Signifies patient is not necessarily free from pain *D. W. Buxton*, 12437-8.
Suggested use of term by Dr. Crile in reference to his experiments in different sense *D. W. Buxton*, 12578-82.
Surgeons' statements as to, *Hon. S. Coleridge*, 10305-14.

"Light":

Nature of certain experiments frequently demands, *Miss Lind-af-Hageby*, 7438-41.
Meaning of term, *Miss Lind-af-Hageby*, *D. W. Buxton*, 9546-62, 9658-61, 12431-5, 12544-56.
Movement and cries possible without sensation of pain under, *H. Morris*, 7996-8002.
Pain may be felt under, *Miss Lind-af-Hageby*, 9137-40, 9486 96.
Use of, does not indicate any degree of pain *A. D. Waller*, 18081-5.

Maintenance of, during experiments tested by inspector *Sir J. Russell*, 547-8.

ANÆSTHESIA—*cont.*

Maintenance of, in surgical operations on men *H. Snow*, 2379-91.
Maintenance of, possible for long periods *E. H. Starling, Sir L. Brunton, J. N. Langley*, 3627-34, 7012-13, 15114-18.
Maintained throughout all cutting experiments, and no cruelty ever witnessed, *Sir V. Horsley, Lorrain Smith*, 16188-98, 18280-3.
Maintained throughout all demonstration experiments *J. N. Langley*, 15387, 15102-8.
Marked by progressive effects divided into degrees of narcosis and definition of such degrees *D. W. Buxton*, 12389-96, 12575-6.
Meaning of term, and use of term "slight anæsthesia" by vivisectors *Hon. S. Coleridge*, 10385-411.
Meaning of term varied, but usually denotes absence of pain *J. N. Langley*, 15411-16.
Measures taken to resuscitate animal if anæsthesia is pushed too far, *E. A. Schäfer*, 10072-82.
Narcotics initially produce sleep, and large dose produce, *A. R. Cushny*, 5181-2.
Object of modern research on, is to know how to paralyse higher centres of brain *Sir V. Horsley*, 15900.
Painful sensation is first to be abolished concluded from personal experience, *L. E. Shore*, 19479-81.
Profound, condition of patient under, *E. H. Starling*, 3755-7.

Reflex movements under:

Occur in all degrees and indicate no sensation of pain, *D. W. Buxton*, 12525-31.
Skill required to distinguish between movements due to sensation of pain and, *D. W. Buxton*, 12612-19.
Some, persist in third degree but no sensation of pain *D. W. Buxton*, 12400-10.

Slight, meaning of term, *Hon. S. Coleridge*, 11548-61.
Small doses necessary for continuation of, when once produced *W. E. Dixon*, 18668.

Tests:

Absence of purposive movement is criterion of unconsciousness to pain *Sir V. Horsley*, 15929-35.

Blood pressure:

Effective *E. H. Starling, W. E. Dixon* 4053-4, 18737-9.
Not used in operations on human being *W. B. Dixon* 18789-92.
Conjunctival reflex is most practical, *D. W. Buxton, A. D. Waller*, 12414-18, 18144-9.
Corneal reflex, *W. E. Dixon*, 18669, 18787-9.

ANÆSTHETICS:

A. C. E. mixture:

Chloroform preferable to, for dogs *F. Hobday*, 16322-6, 16473-5, 16497-500.
Gradual action of, *J. N. Langley*, 15311-313.
Preferable to chloroform, for animals *Sir T. Stoker*, 1023, 1025-7.
Use of, *D. W. Buxton*, 12451.

Action:

Established by experiments on animals *D. W. Buxton, Sir V. Horsley* 12457-60, 15652-3.
Differences in, upon different nerve centres *J. N. Langley*, 15120-9.
Sensory cells paralysed before motor cells *W. E. Dixon*, 18672.
Similar upon men and animals *D. W. Buxton*, 12439-46.
Upon whole central nervous system *D. W. Buxton*, 12599-604.

Actual discovery of, not due to experiments, but knowledge of their precise nature entirely due thereto, *D. W. Buxton*, 12539-40.

Administration:

Consistent and efficient in experiments *Sir J. Russell, G. D. Thane, E. H. Starling, E. A. Schäfer*, 637-9, 700-1, 1765-8, 3470, 3603-6, 3639-56, 10213-21, 4154-9, 4242-52.

ANÆSTHETICS—*cont.*

Administration—*cont.*

Discomfort arising from, *M. S. Pembrey* 14160-1.
Discomfort following, may be greater than slight pain involved in performing operation without, *J. N. Langley*, 15325-8.
Discomfort and fear not caused to animals by methods employed in India *Col. E. Lawrie*, 17039-42.

to Dogs:

Method and apparatus used *G. D. Thane, E. H. Starling, D. W. Buxton, W. E. Dixon* 1230-1, 3585-8, 12488-500, 18677-9.
More difficult, than to human beings *E. H. Starling, S. F. Smith*, 3481-7, 13082
Not difficult *D. W. Buxton*, 12488-500.
Used to be frequently attended by fatal results *F. Hobday*, 16294, 16378-80.

Dose renewed during experiment if necessary *W. E. Dixon*, 18679-85.
Experiments should not be performed under present system of, *Col. E. Lawrie*, 16798, 16808.
Gradual diminution of sensitiveness to pain during, *Sir V. Horsley*, 16012-14.
Great improvement since 1875 in, *Sir J. Russell*, 729.
Invariable, in all experiments more extensive than simple inoculation *E. H. Starling*, 3445.
Knowledge of, should be acquired by practice on animals *Sir V. Horsley*, 15644.
Knowledge of, to animals would be of advantage in human practice, *J. N. Langley*, 15268.
Methods adopted in laboratories, *E. H. Starling, W. E. Dixon*, 3828-35, 3878-80, 4242-52, 18677-9.
in Midwifery cases *G. D. Thane, Sir V. Horsley*, 1720-22, 16015.
Necessity of teaching, by experiments *Sir V. Horsley*, 15651-2.
Nerve stimulating, efficient during *E. H. Starling*, 3657-8.

Overdoses:

Frequent, *Sir J. Russell*, 549.
Occasional, *E. H. Starling*, 3601-2.

Present system of, necessitates preliminary operation of tracheotomy *Col. E. Lawrie*, 16801-7, 16838-53.
Regulations as to, strictly conformed with in all laboratories, *G. D. Thane*, 450, 1588-96.
Skilled anæsthetist unnecessary, to assist during experiments *W. E. Dixon*, 18687.
Special branch of medical practice *G. D. Thane*, 1718-9.
Students are taught methods of, *E. H. Starling*, 3750-2.
Students learn wrong methods of, *Col. E. Lawrie*, 16905-9.
Students should learn, by a course of experiments on animals *Sir V. Horsley*, 16112-114.
Tracheotomy never performed on animals previous to, and such procedure would be illegal *A. D. Waller, J. Lorrain Smith* 18115-19, 18460-1.
Unreliable methods of, in vogue *Sir G. Kekewich*, 20470.
Woulff's bottle used for, *G. D. Thane*, 1615-16.

After effects of, on animals *Sir J. Russell, F. Hobday*, 731-2, 16441-6.
All severely painful experiments should be carried out while animal is completely under, *A. G. Scott*, 19507-8.
Alcohol, mixture of chloroform, and, used *D. W. Buxton*, 12451.
Animals always fully under, in operation and nature of those, employed *A. R. Cushny*, 5039-57.
Animals recover rapidly from effects of, *Sir J. Russell*, 540.
Anti-vivisectionists' persistent misstatements regarding neglect of, *Sir V. Horsley*, 15728-9.

ANÆSTHETICS—*cont.*

Castration should be performed under, *J. W. Graham*, 5928-31, 6111-13.
Cases of second operations for testing purposes, all conducted under *G. D Thane*, 1216-8.
Certain experiments for pharmacological research cannot be satisfactorily performed under, *A. R. Cushny*, 4847-8.
Certificates disallowing use of, imply torture of animals *Sir G. Kekewich*, 20736-43.
Certificate dispensing with, never allowed for severe operations *W. P. Byrne, G. D. Thane*, 160, 440.
Certificates go to release conditions as to *W. P. Byrne, Mrs. K. Cook*, 221-3, 2000-7.
Certificates do not necessarily prescribe particular forms of, *H. Morris*, 7964-7.
Certificate B requires, *Sir J. Russell*, 557.

Chloral:

Doses of, necessary to anæsthetise rabbits and man *W. E. Dixon*, 18654-9.
Efficiency of, *G. D. Thane, F. Gotch*, 1635-8, 13890-2.
Insensibility produced by use of, *Sir L. Brunton*, 6902-6.
Not often used, but method of administering, when employed *W. E. Dixon*, 18682.
Overdose of, has complete narcotic effect, and experiments in connection with, *Sir T. L. Brunton* 6785-98.
Rarely used as an anæsthetic for man, *W. E. Dixon* 18654-7.
Used generally as an anæsthetic for animals *W. E. Dixon* 18658.

Chloroform:

Action of, diminished blood pressure apart from any connection with respiration *A. D. Waller*, 18087A-90.

Administration:

Can be given with perfect safety, so long as regular respiration is maintained *Col. E. Lawrie*, 16952-6.
Clinical experience rather than laboratory experiments have been truer guide in, *Col. E. Lawrie*, 16975-86.
Danger is at beginning of procedure *F. Hobday*, 16455-60.
by Means of a mask *F. Hobday*, 16382.

to Dogs:

Evidence before previous Commission as to, *Sir J. Russell*, 697.
Series of experiments in connection with, and small percentage of deaths *F. Hobday*, 16434-5.

Hyderabad method, is the only safe method *Col. E. Lawrie*, 16935-7.
Inhalation a better method than through trachea *Col. E. Lawrie*, 17080-5.
Investigations into methods of, and records of cases *F. Hobday*, 16306-16.
Investigations into method of, chiefly carried out on dogs and cats *Sir V. Horsley*, 16301-5.
in Midwifery cases *Sir T. Stoker*, 1629-32.
No manner of measuring quantity respired when administered in natural way, *Col. E. Lawrie*, 16927-30.
Nurses and hospital orderlies should be taught, *Col. E. Lawrie*, 16938-9.

Safe, so long as regular respiration is maintained, *Col. E. Lawrie*, 16952-6.
by Students in Edinburgh during last century without any fatal results *Col. E. Lawrie*, 16942-51.
Symes' method, based on clinical experience and quite satisfactory *Col. E. Lawrie*, 16963-5.
System of introducing it through tube from another room quite satisfactory *Sir V. Horsley*, 15910-12.
through Tracheal tube, interferes with natural respiration and vitiates experiment *Col. E. Lawrie*, 16886-98.
through Tracheal tube, should be abandoned *Col. E. Lawrie*, 16911-22.

ANÆSTHETICS—*cont.*

Chloroform—*cont.*

Administration—*cont.*

Tracheotomy, illegality of performing, before, *Col. E. Lawrie*, 17001-7, 17058-66.
Value of giving morphia before, *W. E. Dixon*, 18672.

Alcohol, used in mixture with, *D. W. Buxton*, 12451.
All animals struggle more or less against, *Sir T. Stoker*, 837.
All experiments can be performed painlessly under correct administration of, *Col. E. Lawrie*, 17053-7.
an Absolute anæsthetic *Col. E. Lawrie*, 17000.
Atropin counteracts fatal tendency of, *E. A. Schäfer*, 10088, 10152-4.
Blood pressure just as delicate an indication of action of, as reflexes *W. E. Dixon*, 18690-3.
Blood pressure lowered as first effect of, and respiration stops from want of nutrition *Col. E. Lawrie*, 16825-30.
Cambridge experiments vitiated *Col. E. Lawrie*, 16969-74.
Continuous dose can be maintained in operations on human beings *D. W. Buxton*, 12675-7.
Cross circulation experiments on dogs with object of ascertaining effect of, on heart, methods adopted and results *L. E. Shore*, 19437-19456, 21373-21580.
Death under, occurs from failure of respiration leading to heart failure *Col. E. Lawrie*, 16860-1.
Discovered by accidental inhalation *Sir V. Horsley*, 16258.
Discovered by experiments on man, but its present position depends on animal experimentation *Sir L. Brunton, Col. E. Lawrie*, 7118-21, 16961-2.

Dogs:

Can be kept for several hours under, *Sir V. Horsley*, 16318-28.
Do not show anticipatory signs of terror *F. Hobday*, 16334-6.
Statement as to susceptibility of, not consistent with inspector's experience *Sir J. Russell, G. D. Thane*, 753, 1222-8.
Susceptible to death under, *Sir T. Stoker*, 813-14, 836, 1039.

Experiments brought into disrepute owing to physiologists' rejection of work of Hyderabad Commission *Col. E. Lawrie*, 16836.
Has no direct influence on heart, as proved by the Hyderabad Commission *Col. E. Lawrie*, 16822-4, 16834-5, 16862-6.
Margin between dose of, securing complete unconsciousness and lethal dose *F. Hobday*, 16454.
Noises made by animals under, not indicative of pain *E. A. Schäfer*, 10226-7.
Opium used before discovery of, *D. W. Buxton*, 12642-7.
Percentage to secure unconsciousness much the same in all warm-blooded animals *Sir V. Horsley*, 15645-9.
Preferable to A.C.E. mixture *F. Hobday*, 16322-6, 16473-5, 16497-500.
Progressive diminution of sensibility to pain under, *Col. E. Lawrie*, 17008-17.
Reflexes possible under, *W. E. Dixon*, 18997-19000.
Respiration affected by, and danger of using, *D. W. Buxton*, 12428-30.
Safe, if administered correctly *Miss Lind-af-Hageby, Col. E. Lawrie*, 7599-602, 17045-52.
Sensory nerves paralysed before motor nerves by, *W. E. Dixon*, 18897-900.
Specialists in anæsthesia called into existence by belief that fall of blood pressure under, is synonymous with heart failure *Col. E. Lawrie*, 16940-1.
Used with ether, *D. W. Buxton*, 12451.

Conflicting testimony as to efficacy of, *J. W. Graham*, 6096-110.

ANÆSTHETICS—*cont*

Cross circulation experiments performed on dogs without, *Col. E. Lawrie*, 16810-21, 16992-5.
Curare, use of, implies inefficiency of, *J. H. Levy*, 18500-1.
Curare administered only while animal is effectively under, *G. D. Thane*, 1705.
Distinction between nature of, employed in experiments and veterinary operations *Miss Lind-af-Hageby*, 7350-4.

Dogs :

Can be easily kept under, *Sir V. Horsley*, 15909.
Doubts as to possibility of securely anæsthetising, for long periods *Sir T. Stoker*, 862-8, 891-2.
Not more liable to succumb to effects of, than other animals *Sir L. Brunton*, 7179-85.
Overdosed, and methods adopted for revival *E. H. Starling*, 3481-7.
Unlikely to be completely unconscious during experiment on spleen *Mrs. K. Cook*, 1919.
See also under Chloroform *above*.

Doubts as to efficacy of, employed in experiments and cases where Act is evaded as regards, *Sir G. Kekewich*, 20715-24.
Drowning experiments, some conducted with, and some without, *G. D. Thane*, 1290-2.

Ether :

Experiment performed under inefficient, *Mrs. K. Cook*, 2058-2063.
Fleeting effect of, *Mrs. K. Cook*, 1955.
Used with chloroform *D. W. Buxton*, 12451.

Experiments with view of discovering action of digitalis carried out under, *A. R. Cushny*, 4684-8.
Experiments under Certificates B, B,EE or B,F all require, *G. D. Thane*, 467.
Experiments alleged to have been conducted without, *J. W. Graham*, 5908

Experiments performed without :

Condition enforcing painless killing of animal when main result of experiment attained *W. P. Byrne*, 10-11, 13.
Licence revoked for contravention of Act as to, *W. P. Byrne*, 38-40.
Necessity of certain, *F. Gotch*, *M. S. Pembrey*, 13754-60, 14067-71.
Permitted by licence without Certificate A *G. D. Thane*, 409-10.
Strict enforcement of antiseptic treatment in all cases *W. P. Byrne*, 12-13.
Universal condition for, *W. P. Byrne*, 6-9, 159.

Foreign experiments rarely carried out now without, *Sir V. Horsley*, 15887.
for Gelding horses, *S. Stockman*, 2875-9.
Horses frequently fired without, *F. Hobday*, 16520-3.
Impossible to remove consciousness to pain completely by, *Miss A. Kenealy*, 6426-44.
Inoculation of rabbit for rabies requires, *S. Stockman*, 2590-7, 2912.
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